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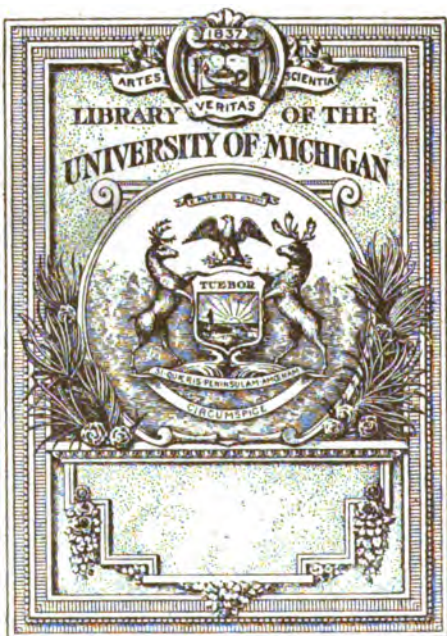
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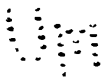
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THE CHEMIST.

ORIGINAL COMMUNICATIONS.

MODE of EXTRACTING the ALKALOIDS, STRYCHNIA and BRUCIA from NUX VOMICA without ALCOHOL.*

By JOHN HORSLEY.

Methods of obtaining strychnia from nux vomica are, besides the less expensive owing to the alcohol used, far from satisfactory, in a toxicological point of view is particularly the case, the small quantity of strychnia naturally contained in nux vomica the production of the alkaloid for its characteristic matter of importance. I have been induced to make experiments on the different methods in use, and it appears to me the simplest and best is that which I now propose, viz., to extract by kneading up, say a quarter of a pound of nux vomica with an equal quantity of commercial acetic acid, turning the pulpy mass with two or three pints of cold water, and letting it digest for a few days; the clear liquor must then be poured off, and an equal quantity of fresh water poured on the residue for a day or two longer, or till all soluble matter is dissolved. The clear liquor is then to be decanted, and the remainder filtered through a flannel filter. The liquid which passes through should be mixed with the former decanted liquor, and evaporated to a syrupy consistency; about three or four ounces. When this is cold dilute it with an equal quantity of water, add liq. ammoniæ in excess, and set it for a day or two that the strychnia may crystallise out, which is the various little white tufts which collect within the fluid, and on the sides of the glass vessel. When the crystallisation is complete, the dark green supernatant fluid is to be passed through a flannel filter, and the residuum with the crystals adhering to the vessel is to be thereon to drain; the dark green mass consisting of the tufts of strychnia and brucia with resinous matter is next to be scraped off, and dried in a water bath digested in hot diluted acetic acid, and then filtered. The strychnia and brucia may be thrown down from the solution, or the strychnia only by the addition of a solution of potassa, when a chromate of strychnia will be obtained in brucia, if the solution be tolerably acid which retains the

* Read before the British Association.

not, I believe, been accurately ascertained; at least, if I may judge from Professor Taylor's work on poisons, where he represents about 5-10ths, or half a grain per cent. I cannot help thinking the exhaustion in that case must have been but imperfectly performed as my own experiments show that nearly twice that quantity is capable of being extracted, for in my first concentration of the liquors from a quarter of a pound of nux vomica, I obtained the following results:

From the 1st concentration, 11 grains of strychnia.

"	2nd	"	4	"
"	3rd	"	2	"

17 making 17 grains of strychnia.

This difference in quantity is necessary to be borne in mind by the medical practitioner, when prescribing the extract and other preparations of nux vomica.

A NEW METHOD OF INSTITUTING POST MORTEM RESEARCH ON STRYCHNIA. By JOHN HORSLEY.*

THE following will be found an exceedingly simple and practical method of obtaining strychnia, in cases where it is practicable to examine the tissues of the body. I have already hinted at the probability why we are sometimes unable to detect strychnia in animals that have been thus poisoned, so that it is unnecessary for me to

The weather, at the time of making these experiments, being hot, the effluvia evolved from so much putrifying animal matter induced me to adopt some means for remedying such an annoyance. I therefore thought of a solution of ordinary chloride of lime (a liquid), but fearing, lest that agent should destroy the strychnia, I first tried its effect on a weak acetic solution of strychnia. I was surprised to find that a milky white precipitate of strychnia hypochloride ensued, insoluble even on the addition of a large quantity of acetic acid. This precipitate, when filtered and dried, was freely soluble in alcohol (which is the best spirituous solvent), but did not readily dissolve in dilute acetic acid, except with the aid of heat. Its best acid solvent

This result gave me such confidence, that I at once proceeded to operate on animal matter. I therefore took some of the liver in which the liver of a dog poisoned by strychnia had been found, which liver I should tell you had not hitherto

duced a portion of the alkaloid, boiled them a few cold added the liquid chloride of lime in excess, or (animal or otherwise) was precipitated, and then through a cloth. No trace of bitterness could then be detected. The drained precipitate of gelatine, fibrine, casein was next digested in alcohol, acidified with diluted sulphuric acid, filtered and evaporated to the consistency of a syrup; this time the whole of the smell of chlorine will have disappeared, and a sulphate of strychnia obtained, which can be purified in the usual way.

Cheltenham,
1856.

*of PLATINUM instead of PORCELAIN or GLASS, in MARSH'S
METHOD of DETECTING ARSENIC.*

DAVY, F.R.S., M.R.I.A., F.C.S.L., &c. Professor of Agricultural Chemistry to the Royal Dublin Society.

The method, as is well known, is founded on the facility with which arsenic gas, generated in the usual way, reduces arsenious oxide, if present, forming arseniuretted hydrogen, which being in contact with air, deposits on particular surfaces, the arsenic being in the state of metal or oxide, or a mixture of both, under various circumstances.

Porcelain or glass surfaces have been generally recommended by Marsh, but I have seen no mention made of platinum. On lately trying this metal, it appeared to me to offer peculiar advantages, decidedly superior to either porcelain or glass. In my first experiment I used a slip of thin platinum foil, but there was no deposition; the flame, though small, produced sufficient heat to carry off the arsenic as arsenious acid; but on trying the blade of a spatula, a spot of arsenic was at once deposited on its surface resembling that produced by my electro-chemical method of depositing arsenic by the agency of zinc and muriatic acid on platinum). A series of similar spots may be produced at pleasure, by holding the spatula for successive instants very near the orifice of the tube through which the gas, the flame being small; then the spots of arsenic may be in the metallic state, or only partially oxidated, and they adhere strongly to the spatula; but if the spatula be held a little further from the orifice, then only arsenious acid will be deposited on it. The heat of a candle is sufficient to volatilise the arsenic as arsenious

to be pretty obvious. The former is more manageable for such experiments. The arsenic strongly adheres to platinum, but not to glass or porcelain, from which surfaces it is easily removed by a finger. It is readily sublimed, producing the garlic-like odor. There is no ambiguity in the results; if antimony be present it remains on the platinum. The apparatus I have employed is simply an ounce phial, with a mouth about an inch in diameter, by which a cork, furnished with a funnel tube reaching nearly to its bottom, and a small tube drawn out to a point for the discharge of the gas. The zinc I employed was in the state of small lumps or in the form of sulphuric or muriatic acid very diluted, viz., one volume of acid to 15 or 20, and even 25 volumes of water.

*An ACCOUNT of some MISCELLANEOUS EXPERIMENTS
TO DETERMINE the relative DEODORISING POWERS of DRUGS
when APPLIED to HUMAN EXCRETA, both SOLID and LIQUID.*

By EDMUND DAVY, F.R.S., M.R.I.A., F.C.S.,
Professor of Agricultural Chemistry to the Royal Institution.

THESE experiments are a continuation of those published in the Society's Journal for April last; the reason for their publication is to avoid repetition, and for the few remarks I have done I regard only as an introductory inquiry, which may lead to useful applications. The view was of a practical nature. It occurred to me that it might be adopted to diminish or deodorise the air in sick chambers, sewers, cess-pools, &c., which are due to decomposition of human excreta; and as a preliminary step, it was desirable to ascertain the relative deodorising powers when applied to excreta, with some latitude, and applied to the most offensive odor was removed, and as far as possible. This, however, is only confined to the destruction of one odor is not sufficient for another. By a reference to the results of many experiments, it will be seen that the powers of the essential oils, and especially of the best deodorisers, such as camphor, deodorising power to clove, peppermint, &c., seems to be very great, and especially than coal tar is also highly

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&c. Professor of
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reference. What I
to a novel subject of
The object I had in
to me, that some means
of offensive odors which
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sic matters, and chiefly to
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ve used the term *deodorising*
rally to cases where an offen-
sive or agreeable one produced.
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s remarkable the high deodorising
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side of lime, supposed to be one of
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tion, that though it is applicable to deodorise putrescent urine, scarcely so well adapted to overcome the more powerful odor of nitric acid. Volatile oils have been long used to reconcile obnoxious medicines to the stomach, why not apply them to remove offensive odors from urine? Volatility, indeed, would seem to offer a greater impediment to application in this way, than even their expense; but they may be rendered less volatile or more durable in their effects, by being associated with other substances, and I have made some experiments on this part of the subject, namely, the best mode of applying the essential oils, in situations and under circumstances where offensive odors are prevalent: but they are not now in a state of sufficient forwardness to be communicated.

Although the deodorising power of alcohol is not very great, it is only equal to that of ground coffee, yet it is proper to mention it occasionally during these experiments I suffered from nausea, and was readily and agreeably relieved by washing the mouth with drops of alcohol. Olefiant gas has considerable deodorising power, a portion of it is absorbed by agitation with putrescent urine, becomes darker colored, and by additional agitations evolves a malodorous gas.

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B.—TABULAR VIEW OF THE RELATIVE DEODORISING POWER OF THE FOLLOWING SUBSTANCES.

The figures express the number of grains, or parts of a grain, required of each substance to deodorise 125 grains of Putrescent Urine.

SUBSTANCES.	Grains	OBSERVATIONS.		SUBSTANCES.	Grains.	OBSERVATIONS. ODOR OF MI
		ODOR OF MIXTURE.				

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

DERIVATIVES of THYMOL and the PREPARATION of BIFORMIC ACID and POLYMER of MARSH GAS. By M. A. LALLEMAND.

analogousness with camphor of essence of thyme, and phenic acid. I formerly mentioned, is confirmed by their chemical properties. In fact, there does not exist any compound of the derivative of thymol which has not its corresponding derivative from thymol. When nitric acid is made to act on thymol, two homologous series of acids are obtained, homologous to binitrate of phenol and of phenol.

The direct action of weak or concentrated nitric acid on the camphor-thymol, is always too great and irregular; whatever precautions may be taken, resinous products and oxalic acid are produced. To obtain pure binitrothymic acid, the nitric acid must be poured drop by drop into an aqueous solution of sulphothymic acid, or of any sulphate; the mixture heats and a reddish oil is precipitated, which becomes solid. The binitrothymic acid thus prepared, $C^{20}H^{11}O^3$, is fusible at $55^\circ C.$ ($130^\circ F.$), sparingly soluble in water, and in all proportions in alcohol and ether. It produces salts which crystallise in needles, sparingly soluble in water, and remarkable for their coloring power. The binitrothymate of potassa may be mentioned for its slight solubility in water, and its beautiful orange or red color, according as to whether it is hydrated or anhydrous.

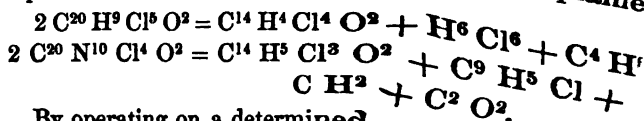
The trinitrothymic $C^{20}H^{10}N^3O^{13}$, HO , homologous to picric acid, is obtained by dissolving binitrated thymol in a mixture of concentrated sulphuric acid and a small quantity of nitric acid; great care is necessary to avoid a too great elevation of temperature, and by adding water to the mixture the acid is precipitated in yellowish white flocks. It is deposited from a boiling aqueous solution in beautiful lemon-colored needles, it is fusible at $110^\circ C.$ ($230^\circ F.$) and suddenly decomposes at a higher temperature. The trinitrothymates are more soluble than the binitrothymates, of a yellow or pale orange tint, and all decompose with an explosion at about $150^\circ C.$ ($302^\circ F.$).

The action of chlorine on thymol likewise gives solid and crystallisable products, which are interesting in consequence of the products of their decomposition. I have already described trichloruretted thymol, which is prepared by stopping the action of the chlorine when the thymol has doubled its weight. But more frequently the product remains liquid, and by prolonging the action of the dry chlorine we obtain a very viscid reddish yellow oil, in the midst of which gradually form some very hard colorless crystals, which are purified by dissolving them in ether; this is pentachloruretted thymol, $C^{20}H^9Cl^5O^2$. This body has the same crystalline form as thymol itself. It fuses without alteration at $98^\circ C.$ ($204^\circ F.$) and decomposes at about $200^\circ C.$ ($392^\circ F.$) disengaging a great deal of hydrochloric acid, mixed with a new hy-

drocarbon; a solid product condenses at the same time, a voluminous residue of charcoal.

The carburetted gas which results from this decomposition is obtained pure by collecting it on water, which dissolves the hydrochloric acid; it is likewise disengaged at the commencement of the reaction. Pure hydrochloric acid is afterwards obtained, and it is only at the end of the distillation that the solid products appear in the neck of the retort, at the same time there is a very abundant disengagement of oxide of carbon. This gas burns with a beautiful white flame, slightly fuliginous, it combines with equal volumes of chlorine. These properties may cause it to be confounded with olefiant gas, but it is distinguished from it by its slight odor of garlic. Its density at zero (32° F.) is 1.15; 20 degrees of cold (-4° F.) will not render it liquid. It detonates violently with oxygen, of which it absorbs four volumes giving rise to two volumes of carbonic acid. It is consequently formed of one volume of vapor of carbon and four volumes of hydrogen, condensed into one. Its chemical formula is $C^4 H^8$. This gas is, consequently, to marsh gas what olefiant gas is to methylene; I shall call it *biformene*.

The solid body, which condenses in the neck of the retort at the decomposition of pentachloruretted thymol, is represented by $H^4 Cl^4 O^4$. It is derived from the body $C^{14} H^8 O^2$, isomer of alcohol, and homologue of thymol and phenol. It is a substance which at 150° C. (302° F.), crystallising in silky needles in alcohol which may be called quadrichloruretted *benzoinol*. When we prepare several quarts of biformene, it is not necessary to prepare pentachloruretted thymol, the preparation of which is always long and difficult; it is sufficient to introduce thymol in a tubulated retort, and to pass an excess of chlorine over it. On distilling the viscid product thus obtained, and exposure to solar light, we may obtain four or five quarts of gas. The products of the decomposition are then different. One part is formed of the body $C^{14} N^5 Cl^3 O^2$, or trichloruretted alcohol, fusible at 95° C. (203° F.), distilling without alteration at 518° F. There is likewise condensed a considerable quantity of liquid, boiling at 865° C. (689° F.), the formula of which is $C^{14} H^8 Cl^4 O^4$, and possessing, consequently, the composition of which is explained by the following equations:—



By operating on a determined weight of pentachloruretted thymol, the products are obtained in the proportion of the operation, is always four parts of oxide of carbon. The crude product is considered as quadrichloruretted

reaction is very powerful in diffused light, the absorption of the gas very rapid, and the disengagement of heat considerable. A liquid is collected which is denser than water, with a more powerful ethereal odor than that of Dutch liquor, and boiling at 102°C . (215°F .). An alcoholic solution of potassa decomposes it instantly, chloride of potassium is precipitated, and water separates a very volatile liquid which floats upon it. This is monochloruretted biformene $\text{C}^4\text{H}^7\text{Cl}$, boiling at 50°C . (122°F .), of an odor resembling at once ether and garlic. By causing dry chlorine to act on these two products, we obtain successively, more and more chloruretted bodies. We thus arrive at the two compounds $\text{C}^4\text{H}^3\text{Cl}^5$, Cl^5H and $\text{C}^4\text{H}^3\text{Cl}^5$. If the action of the chlorine is prolonged in the solar light, the two bodies change suddenly, without any intermediate products, into sesquichloride of carbon.

Bromine behaves like chlorine. We obtain monobromuretted hydrobromide of biformene, of dibromide of biformene, $\text{C}^4\text{H}^3\text{Br}^2$, boiling at 141°C . (286°F .) with a very peculiar odor, and the monobromuretted biformene $\text{C}^4\text{H}^7\text{Br}$, boiling at 75°C . (167°F .).

Iodine likewise absorbs biformene, at the ordinary temperature. The operation is more rapid at 50°C . (122°F .). A very dense liquid compound is formed, which is unstable, having for its formula $\text{C}^4\text{H}^3\text{I}^2$. In contact with dry oxide of silver it decomposes suddenly, with a considerable disengagement of heat. If the reaction takes place under a layer of water the action is more moderate, iodide of silver is formed without any disengagement of gas, and the water holds in solution an inflammable liquid, with an odor resembling that of wood spirit, and which may be isolated by distillation and carbonate of potassa. The small quantity which I was able to collect did not enable me to make a complete study of it. Analysis gave numbers which agree well with the formula $\text{C}^4\text{H}^3\text{O}^2$. This is dimethylic alcohol. I succeeded in obtaining the same body by treating monobromuretted biformene with oxalate of silver. The reaction is performed partly at a temperature of 100°C . (212°F .) in a closed tube. It is formed of bromide of silver and the oxalic ether of the new alcohol. This ether, separated by distillation from the undecomposed bromuretted biformene, gave with aqueous ammonia an abundant precipitate of oxamide, and the alcohol remained in solution.

The ether of the dimethylic alcohol is obtained more easily, by causing an alcoholic solution of potassa to react on monobromuretted biformene. The reaction is quickly performed at a temperature of 100°C . (212°F .) in a closed tube. It is precipitated in abundance from the bromide of potassium, and on opening the cooled tube there is gradually disengaged a very dense gas, sparingly soluble in water, burning with a very smoky white flame with a powerful odor, at once resembling ether and garlic. It becomes liquid at a temperature but

little below the freezing point. I takes 1 volumes of oxygen, and produces 1 volume thence deduce the formula $C^4 H^7 O$ in two vol sents dimethylic ether. The alcohol does not it is not a mixed ether, which would have as would require only six volumes and a half of

Sulphuric acid readily dissolves biformene absorb 200 times its volume. It takes, at the tint, similar to that of a mixture of alcohol and does not separate the gas, and when saturated a copulate salt is produced, very soluble in water lised in absolute alcohol. Its formula is $2 S O$ to say, a composition similar to that of the sulpholefiant gas. When biformylsulphuric acid is distilled several times, one portion of the acid water retains in solution a small quantity of which combines with sulphuric acid to reconstitute in consequence of the small quantity of the I could not make an exact analysis of it or distilling biformylsulphate of baryta with acet few drops of an ethereal liquid, which when engaged a peculiar odor, and reproduced acet

These experiments show that biformene, lilpylene may give rise to two alcohols, either by hydration. We are thus led to think, that series of monoatomic alcohols, in which the constant, whereas the proportion of hydrogen mula would be $C^k H^{2k} O^2$, in which k would series and n variable. Thus we should have,

$C^4 H^4 O^2$ derived from olefiant gas by oxid

$C^4 H^6 O^2$ derived from olefiant gas by hyd

$C^4 H^8 O^2$ derived from biformene by oxida

$C^4 H^{10} O^2$ derived from biformene by hydra

Acrylic alcohol $C^6 H^6 O^2$ and propionic alcohol a similar series.

I have also ascertained the action of the salt of biformene, and it is probable that by considerable quantity of matter a double alcohol analogous to the glycol recently discovered by

Comptes Rendus, No. 7, August 18th, 1856.

NOTE on the ACTION of PERCHLORIDE of PHOSPHORUS on the ACIDS, giving rise to the PYROGENIC ACIDS.

By M. LIES BODART.

THE beautiful studies of M. Gerhardt, on the acids, have induced me to study the action of phosphorus on the fixed acids, which mucic acid is easily obtained

This composition corresponds to the form



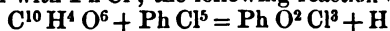
which gives

C	.	.	.	.	.	.
H	.	.	.	.	.	.
Cl	.	.	.	.	.	.
O	.	.	.	.	.	.
Ag	.	.	.	.	.	.

I likewise treated pyromucic acid with p
I obtained chloride of pyromucyle as occurs
as M. Gerhardt found.

This body is liquid, boiling at 170° C. (3
very powerfully; its odor resembles that of
it excites weeping, but not cough.

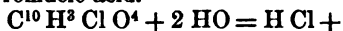
The formula of pyromucic acid being (C
treated with Ph Cl⁵, the following reaction (



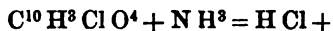
Its composition is—

C	.	.	.	.	.	.
H	.	.	.	.	.	.
Cl	.	.	.	.	.	.
O	.	.	.	.	.	.

This chloride placed under a bell-glass
water evaporates gradually, and we find,
capsule, pyromucic acid.



Put in contact with ammonia, it imme-
mucamide



I tried the action of Ph Cl⁵ on malic ac-
mucic acid, I obtained an acid containing n
composition C⁸H⁴O⁸. This body is not m
melt at 130° C. (266° F.), and does not vol
At 200° C. (392° F.) it disengages white fu
its capacity of saturation with the lime
rhombohedra, and does not possess the rotat
dried at 100° C. (212° F.); I raised the tempe
there was a loss of 0.043 gr.; at 150° C. (3
ther loss of 0.002 gr. 0.712 gr. — 0.045 gr =
with sulphuric acid; I obtained 0.579 gr. of
ing 0.170 gr. of calcium, or 25.9 per cent.
have not carried my investigations further.
hereafter.

Comptes Rendus, No. 7, August 18, 1856.

*On the CHEMICAL TYPE — AMMONIA.**

By Dr. A. W. HOFMANN, F.R.S.

THE great laws which govern chemical combination, have been mostly recognised and elaborated by the study of *mineral compounds*, the examination of which at a very early period attracted the attention of inquirers. It was only much later:—In fact, at a comparatively recent epoch, that the *vegetable* and the *animal world* were drawn into the circle of chemical observation. The progress made in the study of *vegetable* and *animal* substances was based, in the commencement at least, entirely and exclusively upon the knowledge which chemists possessed of *mineral* bodies. The experience, the ideas, gained in the examination of *mineral* substances, reflected themselves, if I may use this expression, in whatever views were brought forward regarding the nature of *vegetable* and *animal* compounds. *Organic Chemistry* was but a reproduction, in another form, of *Mineral* or *Inorganic Chemistry*.

This aspect, however, of the relative position of the two departments of the science is rapidly changing. The amount of material accumulated by the indomitable perseverance of so many cultivators of Organic Chemistry (a chaotic and almost inaccessible labyrinth, but a few years ago), is rapidly assuming shape and order. The study of *organic* bodies has led to the observation of general laws, which could have never been discovered by the examination of *mineral* substances alone, but which begin to react in a most powerful manner upon our ideas regarding the constitution of these very *mineral* substances. The progress of our knowledge of *organic* bodies has opened new points of view, from which the constitution of *mineral* substances appears to us in a brighter light, in a simpler and more intelligible form. In one word, organic chemistry is beginning to repay, and I venture to say, with interest, the debt of gratitude which it owes to her elder sister, Mineral Chemistry.

It is my task, this evening, to bring under your notice some especial examples in elucidation of the idea which I have endeavored to delineate to you. Illustrations of this kind might be taken from widely different departments of the science. In consequence of special studies and predilections of my own, I have selected as material of illustration a class of substances, of which the well-known compound ammonia is the type.

The four elements—Nitrogen, Phosphorus, Antimony, and Arsenic, although essentially differing in many of their physical properties, exhibit, nevertheless, an extraordinary similarity in their chemical character, and especially in their combining tendencies. With oxygen these four bodies produce teroxides and pentoxides which, in combination with water, have all decidedly acid properties.

* The substance of a Lecture delivered at the Royal Institution, Friday Evening, May 16, 1850.

Nitrous acid .	NO^3	Nitric acid .	NO^5
Phosphorus acid .	PO^3	Phosphoric acid .	PO^5
Antimonious acid	SbO^3	Antimonic acid .	SbO^5
Arsenious acid .	AsO^3	Arsenic acid .	AsO^5

The latter acids, moreover, appear to be all tribasic; in phosphorus and arsenic acids the tribasic character is well marked; with nitric acid it is less pronounced; and nitric acid is generally considered as a monobasic acid, but the progress of science will, I have no doubt, confirm our suspicion that the nitrogen acid is likewise of a tribasic character. The chlorides and bromides, corresponding to those of nitrogen, phosphorus, antimony, and arsenic also exhibit certain limits, similar analogies.*

Again, these four elements unite with hydrogen, and the compounds thus produced have a similar composition; they are all terhy-

Ammonia	N^1H^3
Phosphoretted hydrogen	P^1H^3
Antimonetted hydrogen	Sb^1H^3
Arsenetted hydrogen	As^1H^3

So far the analogy appears to be complete. Extraordinary peculiarities, however, are observed in the properties of these compounds, for although they are all gases at the common temperature, although they all possess a marked odor, and are not inflammable, we find that ammonia is *soluble* in water, and has a *strongly alkaline character* to this solution; while the three phosphoretted, antimonetted, and arsenetted hydrogens are *insoluble* in water, and *without the slightest alkaline reaction* ammonia when coming in contact with acids, absorbs with the greatest avidity, producing a series of well marked crystalline compounds, which are called salts of ammoniacal salts, and of which sal ammoniac and sulphate of ammoniac are familiar illustrations. Antimonetted and arsenetted hydrogens, on the other hand, have never been combined with acids; and phosphoretted hydrogen, only one salt-like compound, of phosphoretted hydrogen is known, which latter exhibits considerable analogies with the salts of ammonia.

Sulphate of ammonia	$\text{N}^1\text{H}^3\text{S}^2\text{O}^6$
Hydriodate of ammonia	$\text{N}^1\text{H}^3\text{I}^3$
Hydriodate of phosphoretted hydrogen	$\text{P}^1\text{H}^3\text{I}^3$

The want of similarity observed in the general properties of ammonia and the hydrogen compounds of phosphorus, antimony, and arsenic, has always been an obstacle in the way of regarding these four elements in question as members of the same natural

* For the purpose of illustration specimens of *nitrogen* and *arsenic*, and of their oxides, chlorides, and bromides were used.

† The preparation and the principal properties of *ammonia*, *phosphoretted*, and *arsenetted* hydrogen were experimentally determined.

The modern progress of organic chemistry has removed those difficulties.

Organic chemistry deals with compound molecules, consisting of carbon and hydrogen, occasionally associated with nitrogen and oxygen. These compound molecules, often called compound radicals, simulate the deportment and exercise the functions of elementary substances. One of the most familiar illustrations of organic radicals is the radical ethyl, consisting of four equivalents of carbon, and five of hydrogen, $C^4H^5 = E$, and which chemists assume to exist in *alcohol* and *ether*, the derivation of which from water becomes obvious by a glance at the following formulæ:—

Water	{	HO
		HO
Alcohol	{	EO
		HO
Ether	{	EO
		EO

Alcohol may be regarded as water, in which *one* equivalent of hydrogen is replaced by ethyl, *ether*, as water for the *two* hydrogen equivalents of which ethyl has been substituted. The general characters of these three compounds greatly differ from one another; but some of the fundamental properties of water, its neutral character for instance, are retained in the two substitution products.*

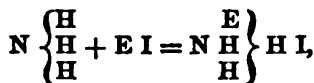
Recent researches have proved that in ammonia likewise, the hydrogen equivalents are replaceable by ethyl. Three new compounds are thus produced, which have received the names *ethylamine*, *diethylamine*, and *triethylamine*: and the composition of which is illustrated by the following formulæ:—

ture of ammoniacal salts was, moreover, illustrated by a series of painted diagrams, and a set of large and beautiful specimens furnished by Messrs. Simpson, Maule, and Nicholson, Kensington. Ammonia was evolved by the action of lime upon sal-ammoniac, phosphoretted hydrogen by that of hydrochloric acid upon phosphide of calcium, antimonetted and arsenetted hydrogen lastly, by introducing antimony- and arsenic-solutions into flasks from which hydrogen was being evolved. Cylinders were filled with the four gases over mercury. The phosphoretted hydrogen evolved proved to be spontaneously inflammable. Antimonetted and arsenetted hydrogen were inflamed by a taper, and ammonia was shown to be capable of combustion by directing the current of the gas through a gas-flame. It was demonstrated that phosphoretted, antimonetted, and arsenetted hydrogens are *not* absorbed by water or even acids, and that they exhibit no alkaline reaction with vegetable colors; whilst ammonia is absorbed by water and acids, and possesses the character of a strong alkali.

* Specimens of water, alcohol, and ether placed in juxtaposition. Ethyl gas, obtained by the action of zinc upon the iodide of ethyl in a strong copper digester, exhibited and burnt.

Ammonia	N	$\begin{Bmatrix} \text{H} \\ \text{H} \\ \text{H} \end{Bmatrix}$
Ethylamine	N	$\begin{Bmatrix} \text{E} \\ \text{H} \\ \text{H} \end{Bmatrix}$
Diethylamine	N	$\begin{Bmatrix} \text{E} \\ \text{E} \\ \text{H} \end{Bmatrix}$
Triethylamine	N	$\begin{Bmatrix} \text{E} \\ \text{E} \\ \text{E} \end{Bmatrix}$

The three ethylated derivatives fully retain the fundamental character of ammonia; they are powerful bases, capable of uniting with the acids, and of forming very definite, well-crystallising salts. Owing to the diminution of volatility with the progress of ethylation, the ethylated bases appear to be even more powerfully basic than the type itself. This development of basic power, as will be presently seen, deserves especial consideration. The substitution of ethyl for hydrogen presents no difficulty, it may be effected by several methods, one of the commonest processes consisting in the action of iodide of ethyl upon the body to be ethylated. Thus ammonia and iodide of ethyl produce ethylamine and hydriodic acid, which unite and rise to the formation of hydriodate of ethylamine.†



In consequence of the ethylated derivatives of ammonia retaining the basic character of the type, and exhibiting it, under certain conditions, even in a higher degree, the question naturally suggested itself, What would be the effect of introducing ethyl into phosphorated, antimonetted, and arsenetted hydrogen?

The ethylation of these hydrogen compounds presents difficulties not experienced with the nitrogen series, and has been accomplished only by roundabout processes. Nor have all the terms, the existence of which theory suggests, as yet been obtained; compounds corresponding to ethylamine and to diethylamine are wanting at present, but the substances which correspond to triethylamine are known.

The following table exhibits the compounds belonging to this group which are known.

† Ammonia, ethylamine, diethylamine, and triethylamine placed side by side; the alkaline properties of these four substances experimentally demonstrated. Iodide of ethyl, and its action on ammonia exhibited.

Nitrogen-Series.		Phosphorus-Series.		Antimony-Series.		Arsenic-Series.	
Ammonia. . . N	$\begin{Bmatrix} H \\ H \\ H \end{Bmatrix}$	Phosphoretted Hydrogen P	$\begin{Bmatrix} H \\ H \\ H \end{Bmatrix}$	Antimonetted Hydrogen Sb	$\begin{Bmatrix} H \\ H \\ H \end{Bmatrix}$	Arsenietted Hydrogen As	$\begin{Bmatrix} H \\ H \\ H \end{Bmatrix}$
Ethylamine. . . N	$\begin{Bmatrix} E \\ H \\ H \end{Bmatrix}$	Unknown.		Unknown.		Unknown.	
Diethylamine . N	$\begin{Bmatrix} E \\ E \\ H \end{Bmatrix}$	Unknown.		Unknown.		Unknown.	
Triethylamine . N	$\begin{Bmatrix} E \\ E \\ E \end{Bmatrix}$	Triethylphosphine. . . P	$\begin{Bmatrix} E \\ E \\ E \end{Bmatrix}$	Triethylstibine Sb	$\begin{Bmatrix} E \\ E \\ E \end{Bmatrix}$	Triethylarsine As	$\begin{Bmatrix} E \\ E \\ E \end{Bmatrix}$

Now triethylphosphine,* triethylstibine, and triethylarsine, are substances exhibiting, although in a less prominent degree, all the fundamental characters of triethylamine, and consequently of ammonia itself. They are well defined and powerful bases, capable of uniting with the acids and of producing a series of remarkable, mostly crystalline salts, in which we find all the properties of the ammoniacal salts. Chemists have thus succeeded in rendering visible to the mental eye, if I may say so, the true nature of phosphoretted, antimonetted, and arsenetted hydrogen. By the conversion of these *mineral* substances into *organic* compounds by the simple process of ethylation, their alkaline disposition, not to use the term character, has been unmistakeably brought to light. The formation of alkaline bodies similar to ammonia by the substitution of ethyl for the hydrogen in phosphoretted, antimonetted, and arsenetted hydrogen, leaves no doubt regarding the analogy of these substances with ammonia, and thus we see *that researches carried out exclusively in the field of ORGANIC CHEMISTRY have lent most valuable assistance in deciding a question of considerable importance regarding the classification of MINERAL SUBSTANCES.* These researches have furnished the last argument which was wanting to prove that nitrogen, phosphorus, antimony, and arsenic form a natural group of elements, the chemical history of which presents analogies not less prominent than those which are observed with the elements chlorine, bromine, and iodine.

The type ammonia offers another interesting illustration of the influence which the progress of organic chemistry exerts upon the mineral department of the science, and of the unexpected support which some of the mineral theories have received from the developement of our ideas regarding the constitution of organic substances.

* Specimens of triethylphosphine exhibited, and its alkaline characters demonstrated. A small quantity of triethylphosphine was poured into a test-tube filled with oxygen, and placed in hot water, when the phosphorus compound exploded with great violence. Another portion introduced into chlorine gas, gave rise to a brilliant flash of light, the carbon of the substance being set free. To show the combining power of triethylphosphine, the substance was mixed with iodide of methyl, when a white crystalline compound was immediately formed, with evolution of much heat.

Soon after Sir Humphry Davy's immortal discoveries of the alkali-metals, chemists were led by the extraordinary analogy of the salts of these metals with those of ammonia, to assume in the latter a metallic substance similar to potassium and sodium.* Numerous experiments were made to isolate this metallic principle from the ammoniacal salts and the resources of electricity, which had exhibited such wonderful powers in the hands of Sir H. Davy, were not appealed to without result. The metal itself, it is true, was not isolated; but a compound or alloy was obtained, containing nitrogen and hydrogen, and the metallic character of which was indisputable. If the electric current be passed into a solution of ammonia floating upon a layer of mercury in such a manner, that the positive pole of the battery merely dips into the ammonia, while the negative pole is immersed in the mercury, a very remarkable phenomenon is observed; the mercury begins to swell up and is gradually converted into a mass of buttery consistence, but retaining a perfect *metallic* lustre, while pure nitrogen gas arising from the oxidation of ammonia makes its appearance at the positive pole.† Removed from the influence of the battery, the altered mercury soon resumes its original appearance, losing at the same time hydrogen and ammonia in the proportion of one equivalent of the former (H) to one of the latter (NH^3). It was therefore argued that the mercury owed the alteration of its properties to its being associated with hydrogen and ammonia, that is with NH^4 , and since mercury in its combinations never retains any metallic appearance, except in its alloys called amalgams, that is in its combination with metallic substances, chemists considered themselves entitled to attribute *metallic* characters also to the hypothetical association of nitrogen and hydrogen, represented by the formula $\text{NH}^4 = \text{Am}$, for which, forthwith, the name of *ammonium* was proposed.

By assuming the existence of this hypothetical metal in the salts of ammonia, the nature of these substances, their analogy with the salts of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, became at once intelligible. The following table exhibits the ammoniacal salts (represented firstly, as combinations of *ammonia*, with hydrated acids; and, secondly, as *ammonium* compounds;) in juxta-position with the corresponding terms of the potassium series.

* The analogy of the salts of ammonia and those of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, was illustrated by numerous specimens upon the Lecture-table. Ammonia-alum, and potassa-alum, sulphate of ammonia and sulphate of potassa, chloride of ammonium and chloride of potassium, &c. &c.

† The ammonium-amalgam was produced on a small scale by the action of the electric current.

	Ammonia-Compounds.	Ammonium-Compounds.		Potassium-Compounds.
Oxide. .	NH^3, HO	$\text{NH}^4 \text{ O}$ or	Am O	K O
Chloride .	NH^3, HCl	$\text{NH}^4 \text{ Cl}$ or	Am Cl	K Cl
Sulphate .	$\text{NH}^3, \text{HSO}_4$	$\text{NH}^4 \text{ SO}^4$ or	Am SO^4	K SO^4
Nitrate .	$\text{NH}^3, \text{HNO}^3$	$\text{NH}^4 \text{ NO}^6$ or	Am NO^6	K NO^6

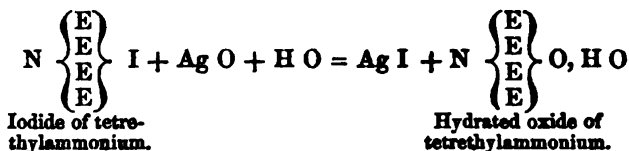
The analogy of chloride, sulphate, and nitrate of ammonium, with the corresponding potassium compounds, is complete, but the analogy begins to fail when we compare the *oxides* of ammonium and potassium. Oxide of potassium, potash, is a perfectly definite body, the properties of which, especially in its hydrated condition, are well known. On the other hand, all attempts to isolate the oxide of ammonium or its hydrate have been hitherto abortive. Liberated from one of the ammonium compounds it splits at once into ammonia gas and water, even at the common temperature. The impossibility of producing the oxide of ammonium has been always adduced as an argument against the ammonium theory.

This difficulty disappears entirely if we examine the deportment of some of the compounds briefly described in the preceding part of this lecture. By again submitting triethylamine, that is ammonia containing three equivalents of ethyl in the place of three of hydrogen, to the action of iodide of ethyl, a beautiful crystalline compound is obtained, the composition of which is represented by the formula—



that is iodide of ammonium, in which the four hydrogen equivalents are replaced by a corresponding number of equivalents of ethyl, or *iodide of tetrethylammonium*. If this compound be treated with freshly precipitated oxide of silver, a decomposition takes place, which gives rise to the formation of iodide of silver, separating as a precipitate and of the hydrated ammonium oxide, corresponding to the above-mentioned iodide or *hydrated oxide of tetrethylammonium*,* which remains in solution. The following equation elucidates this change—

* In three beakers, boiling solutions of iodide of tetrethylammonium (1), of iodide of ammonium (2), and of iodide of potassium (3), were mixed with freshly precipitated oxide of silver. The beakers were then covered with reddened litmus paper and glass plates; after a few moments the paper covering solution (2), was found to have become intensely blue, in consequence of the volatilised ammonia gas; whilst the papers covering solutions (1) and (3) did not exhibit the slightest alteration, the liberated oxides of tetrethylammonium and of potassium not being volatile. On *dipping*, however, reddened litmus paper into these two solutions, they were found to be powerfully alkaline.



The solution of this compound oxide of ammonium may be evaporated to dryness without decomposition ; a crystalline substance is thus obtained of a most powerfully alkaline character, resembling in every respect hydrated potassa itself. A concentrated solution of this substance not only burns the tongue, but acts upon the epidermis, which it destroys like potassa or soda. On rubbing the solution between the fingers, the well-known soapy sensation produced by the fixed alkalies under the same circumstances is felt. Moreover the same peculiar odor is perceived. Oxide of tetrethylammonium saponifies the fats without difficulty ; beautiful soft soaps are thus obtained, possessing all the properties of ordinary potassa soap. All the chemical effects produced by potassa or soda, are likewise produced by oxide of tetrethylammonium, and in its deportment with the salts of the metals especially, the compound oxide of ammonium can scarcely be distinguished from the fixed caustic alkalies.

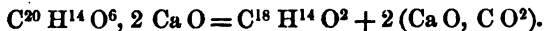
The formation of oxide of tetrethylammonium is certainly one of the most powerful props which the ammonium theory could have received, and since this support has been entirely and exclusively furnished by researches performed in the domain of *organic chemistry*, it must be admitted that the researches made in this department of the science, and especially the elaboration of the compound derivatives of the type ammonia begin to react most powerfully and beneficially upon the progress of *mineral chemistry*.†

NOTE on PHORONE. By M. LIES BODART.

IN 1849, M. Gerhardt and I, published a note on the dry distillation of camphorate of lime. We obtained an oil, to which we gave the name of phorone, the formula of which in M. Gerhardt's notation is:



in the ordinary notation. Phorone is camphorate of lime, less carbonate of lime,



Two years ago, I wished to ascertain if the fruit of the serb tree, at complete maturity, towards the middle of November, still contained malic acid. I found by Liebig's process, only a very small quantity.

† At the conclusion of the lecture, the ammonium amalgam was prepared on a large scale by the action of potassium amalgam upon a solution of ammonium chloride of

of this acid; but on ~~mixing~~ the liquors, I found an abundant syrup, which I distilled with lime. An oil passed over which had the odor of phorone; I analysed it, and found that it had the same composition:—

C	.	.	.	.	77.92	} mean of several analyses.
H	.	.	.	.	10.20	
O	.	.	.	.	11.88	
					100.00	

Phorone gives—

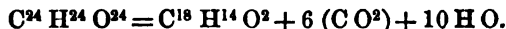
C ¹⁸	108	} or in hundredths	{	78.26
H ¹⁴	14			10.15
O ²	16			11.59
				100.00

Last year, I collected the fruit at the same period, in order to seek for the substance which, in my syrups, had given rise to phorone; but my researches were in vain. Was it the serbine of M. Pelouze? or, was it uncrystallisable sugar? I took glucose and made a thick syrup with it, and operated in the same circumstances as with the serb. An oil passed over in abundance, which had the same odor of which I have spoken. By distillation, I separated this oil into two distinct portions, A and B; A, which was of a light yellow, began to boil at 86° C. (186° 8' F.). B, turned brown rapidly in the air, and began to boil at 208° C. (406° 4' F.); these oils did not deviate the plane of polarisation. The mean of several analyses of the oil B gave the following results:—

C	.	.	.	.	.	.	.	77.90
H	.	.	.	.	.	.	.	10.27
O	.	.	.	.	.	.	.	11.83
								100.00

There is no doubt, therefore, that phorone owes its production to sugar.

This reaction is thus explained:—

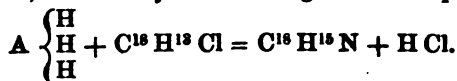


By treating the product C¹⁸ H¹⁴ O² with Ph Cl⁵, we obtain chloride of phoryle C¹⁸ H¹³ Cl; it is an oil lighter than water, of a very agreeable odor, and boiling towards 175° C. (357° F.); it has the following composition:—

		Found.	Calculated.
C	.	68.83	69.04
H	.	8.07	8.30
Cl	.	28.10	22.66
		100.00	100.00

The chloride dissolved in alcohol and saturated with ammonia, was heated under pressure in a tube sealed in the lamp. A crystalline

organic body was produced in the bottom. Having obtained it only in small quantity, I estimated only the chlorine, which was in rather larger quantity than the hydrochlorate of the nitrogenous product $C^{18}H^{15}N$, HCl , obtained by the following reaction requires.



Finally, I distilled the product B over anhydrous phosphoric acid; there passed over a limpid, refringent oil, of agreeable odor, and boiling between 150° and $160^{\circ}C$. (302 and $320^{\circ}F$). It is cumene, or an isomer of cumene. Its composition is:—

	Found.	Calculated.
C	89.57	90
H	10.28	10

The oil has the composition

	Found.	Calculated.
C	73.26	73.47
H	10.47	10.20
O	16.27	16.33
	100.00	100.00

It is the metacetone of M. Fremy, C^6H^5O .

It occurred to me that the formula of this body would rather be $C^{18}H^{15}O^3$. The reaction would be easily explained—



It may be conceived, that distillation with lime being effected at high temperature, more or less water should be eliminated. Moreover by distilling the oil over anhydrous phosphoric acid, we obtain, after a first distillation, a product which has the composition:—

C	77.7
H	10.4
O	11.9

this is phorone. On treating the oil B with potassium, no reaction took place cold; but on the temperature being raised, hydrogen was disengaged. The new product is probably $C^{18}H^{13}K$. In contact with the chloride $C^{18}H^{13}Cl$, would it give the $C^{18}H^{13}\} O^2 + KCl^2$? I did not try this last reaction, as I had no more chloride.

Be it as it may, phorone, from its principle reaction, might be regarded as an alcohol having cumene as a hydrocarbon. The metamorphosis of phorone into cumene connects sugar with the benzoic series. that Mr. Abel has converted this hydrocarbon into benzoic acid. *Comptes Rendus*, No. 7, August 18th, 1856.

On the REDUCTION of the NITRIC DERIVATIVES of the HOMOLOGOUS ACIDS of BENZOIC ACID. By M. BOULLET.

In a memoir entitled "Researches on Pyroxyline" (see THE CHEMIST, New Series, vol. iii., 1855-6, p. 200), M. Béchamp announced, that as a continuation of his work on the nitric derivatives reduced by the action of the ferruginous salts, I was engaged in analogous experiments, and that I had arrived at important results. This announcement appeared to me sufficient for securing priority, until my work could be completed and submitted to the Academy. At any rate, I regarded the question in this light; but a publication recently made by M. Voit, in Liebig's *Annalen*, relating to the subject with which I am occupied, compels me to protect my rights, if not to priority, at least in the property of my researches, by immediately addressing a summary of them to the Academy, until I have the honor of submitting to it a more complete work.

On treating nitro- and binitro-cuminic, nitro-cinnamic, and nitro- and binitro-benzoic acids, with a suitable weight of iron filings and acetic acid, a very brisk reaction is set up, without disengagement of nitrous gas. The temperature rises; when it begins to abate perceptibly, it is heated for some time in a sand bath. The residue is taken up with a slight excess of pure carbonate of soda at a gentle heat. It is filtered, and the excess of carbonate of soda employed is accurately saturated with acetic acid. It is precipitated with acetate of lead. The precipitate obtained is then decomposed with sulphuretted hydrogen, and a distinctly acid solution is obtained, which contains, according to the product reduced:—

- | | | | | |
|------------------------|---|---|---|---------------------------|
| 1. Amidocuminic acid | . | . | . | $C^{20} H^{18} N O^4$; |
| 2. Biamidocuminic acid | . | . | . | $C^{20} H^{14} N^2 O^4$; |
| 3. Amidocinnamic acid | . | . | . | $C^{18} H^9 N O^4$; |
| 4. Amidobenzoic acid | . | . | . | $C^{14} H^7 N O^4$; |
| 5. Biamidobenzoic acid | . | . | . | $C^{14} H^5 N^2 O^4$. |

Amidobenzoic acid is known by the name of benzamic acid.

All these bodies are crystallisable, and capable of combining with bases.

The same mixture of iron and acetic acid reacts on the ethers of the nitrated acids. I have reason to think, that this method of reduction will be an indirect mode of generating the ethers of the amiduretted acids in question. In the memoir which I shall have the honor of presenting to the Academy, I shall develop the theoretical views which appear to me to flow from these results.

Comptes Rendus, No. 7, August 18, 1856.

ANALYTICAL CHEMISTRY.

ESTIMATION of the HYPOSULPHITES of COMMERCE.

By M. SCHLAGDENHAUFFEN.

WE find in commerce hyposulphites of soda and potassa which, from their mode of formation contained sulphates and small quantities of sulphites. It is important to know how to estimate these salts and to find their impurities, on account of the frequent employment of hyposulphite of soda in photography.

When we desire to ascertain the quantity of sulphate contained in solution in a hyposulphite or sulphite, we may easily do so by means of a metrical liquor of chloride of barium. We take a certain quantity of dry salt, dissolve it in water, and pour into this solution the baryta salt so long as any precipitate is formed. This done, we read in the burette the number of divisions employed. We shall then know the weight of the baryta employed, and, consequently, we shall have that of the sulphuric acid, of the sulphate of baryta, and thus that of the sulphuric acid existing in the state of sulphate in the mixture.

It is sufficient then to subtract this weight from that of the three salts, and the difference will be that of the sulphite and hyposulphite. We do not yet know the particular weight of each of the last two salts; and it is for the purpose of determining it that I have devised the following method, based on the facility with which sulphuric acid decomposes the sulphites and hyposulphites.

1. Let us take at first the case of a mixture of sulphite and hyposulphite. The mixture is dried with care, and a certain quantity of it is weighed, it is dissolved in a capsule so as to have 10 grains in 50 cubic centimetres of water. A metrical test liquor of sulphuric acid is poured into the solution. There will immediately be a disengagement of sulphurous acid and a deposition of sulphur, and that will continue until the sulphite and hyposulphite are completely decomposed. Care must be taken to heat the solution of salt which is being decomposed to 60° C. (140° F.), while the sulphuric acid is being poured in, in order that the sulphurous acid may be more rapidly disengaged: by means of starched and iodised paper, we shall readily know when the operation is terminated. When the iodised paper remains colorless after the addition of a fresh quantity of sulphuric acid, it is certain that no more hyposulphite and sulphite remain in the liquor, and that the two salts are converted into sulphates. We read on the burette the number of divisions employed, and inasmuch as we have the weight of sulphuric acid necessary for forming the sulphate of soda, thus also, by means of a proportion, we shall know the weight of the soda combined with this sulphuric acid.

This done, I solve the problem in the following manner:—

Let x be the weight of the sulphite of soda;

And y that of the hyposulphite of soda.

Since we know, from what I have just said, the weight P' of the soda of the mixture, we shall be able by means of two equations to determine x and y .

The first equation is given by the weight of the two salts, whose known weight is P :—

$$x + y = P \quad (1).$$

To obtain the second equation, it suffices to find a relation between the equivalents of the salts employed and their weights. This equation is furnished by the proportions :—

$$\begin{aligned} \text{Eq. S O}^2 \text{ Na O} : \text{Eq. Na O} &:: x : x' \\ \text{Eq. S}^2 \text{ O}^2 \text{ Na O} : \text{Eq. Na O} &:: y : y'. \end{aligned}$$

In these proportions x' and y' designate the weight of the soda which is found in the quantity x of sulphite, and y of hyposulphite; now, from what I have said, we may know the weight P' of all the soda, then $x' + y' = P'$ and the equation becomes :—

$$\frac{\text{Eq. Na O}}{\text{Eq. SO}^2 \text{ Na O}} x + \frac{\text{Eq. Na O}}{\text{Eq. S}^2 \text{ O}^2 \text{ Na O}} y = P' \quad (2).$$

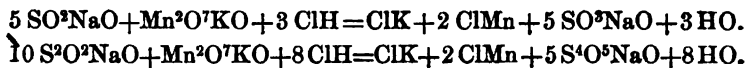
The solution of these two equations enables us to determine x and y , that is to say, the quantities of sulphite and hyposulphite which are found in the mixture.

The problem is not much more complicated when the solution contains sulphate; indeed, it suffices to take as above the weight of the salts in the dry state, to dissolve them in water, and to pour into the liquor the metrical solution of the salt of baryta, until no more precipitate of sulphate of baryta is formed. The number of divisions and consequently the weight of chloride of barium employed, enable us to obtain by a proportion the weight of the sulphuric acid, of the sulphate of baryta, and, consequently, the weight of the sulphate of soda existing in the mixture. The weight p of the sulphate of soda will be subtracted from the weight w of the three salts, and the equation $x + p = w - p$, will express the weight of the hyposulphite and sulphite.

The second equation will remain the same as equation 2, since the sulphite of soda does not prevent the reaction of sulphuric acid on the sulphite and hyposulphite.

2. We may detect and estimate a sulphite in presence of a hyposulphite, by means of another method based on the facility with which the sulphite and hyposulphite are oxidised in presence of hypermanganate of potassa.

The following equations account for this oxidation :—



By gently pouring the hypermanganate, acidified with hydrochloric acid, into the mixture of the two salts, they are oxidised and converted into sulphate and dithionate, without disengagement of sulphurous acid or deposition of sulphur. If, then, we take a metrical solution of

this hypermanganate, and pour a certain number of cubic centimetres into the mixture of the two salts, we shall know from the number of divisions employed the weight of hypermanganate necessary for effecting the transformation.

After that, as in the foregoing case, I call x and y the two weights of sulphite and hyposulphite of the mixture, it is evident that we may lay down the following two proportions between the equivalents of the salts and the weight of the hypermanganate :—

$$5 \text{ Eq. SO}^3\text{NaO} : \text{Eq. Mn}^2\text{O}^7\text{KO} :: 5x : x'.$$

$$10 \text{ Eq. S}^2\text{O}^3\text{NaO} : \text{Eq. Mn}^2\text{O}^7\text{KO} :: 10y : y'.$$

The weights $x' + y'$, express the weight of hypermanganate employed for oxidising the sulphite and hyposulphite; then we shall have a first equation between the unknown numbers, which is :—

$$\frac{\text{Eq. Mn}^2\text{O}^7\text{KO}}{\text{Eq. SO}^3\text{NaO}} x + \frac{\text{Eq. Mn}^2\text{O}^7\text{KO}}{\text{Eq. S}^2\text{O}^3\text{NaO}} y = P' \quad (2).$$

Designating by P' the weight of the hypermanganate. On the other hand, if we weigh the weight of the salts on which this transformation is operated, we shall have, indicating this weight by P' ,

$$x + y = P' \quad (1).$$

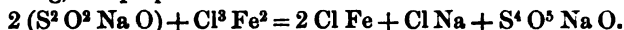
These two equations serve for finding the value of the unknown numbers.

When the sulphite and hyposulphite are accompanied by sulphate, a preliminary test must always be made with a portion of the liquor, by means of the metrical liquor of the baryta salt, in order to obtain the weight of the sulphate of soda which exists in the mixture. If p designates this weight, it will be subtracted from the weight w of the three mixed salts, and equation 1 will become, as in the foregoing case :—

$$x + y = w - P'.$$

3. When the hyposulphite is free from sulphate and contains no other impurity than the sulphite, we may very quickly arrive at a knowledge of the real quantity of hyposulphite which is found in it, by employing a metrical liquor of chloride of iron.

M. Richard, formerly preparator of the Ecole de Pharmacy at Strasbourg, has proposed this method: it is based on the relation—

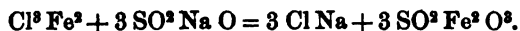


On pouring the chloride of iron into the hyposulphite, a very distinct violet coloration is obtained; as soon as this coloration no longer appears, on the addition of a new quantity of the salt of iron, we are certain that the hyposulphite is transformed. We read on the graduated burette the number of divisions of the salt of iron, and, consequently, the weight of salt employed. By a simple proportion we then know the weight of hyposulphite which has been converted.

$$\text{Eq. Cl}^3\text{Fe}^2 : 2 \text{ Eq. S}^2\text{O}^3\text{NaO} :: p : 2x.$$

In this proportion p expresses the weight of the chloride of iron, and x that of the real hyposulphite of the solution.

It is needless to add, that the presence of sulphate of soda does not interfere with this reaction. On the other hand, this method is not applicable when this salt is accompanied by sulphite, since in this case sulphite of iron would be formed, as seen in the following equation:—



This sulphite is insoluble in concentrated liquors, and dissolves with a yellow color in dilute liquors. The color, therefore, prevents us from seeing what is produced by the reaction of the ferruginous salt on the hyposulphite, and the moment at which the violet coloration disappears, that is to say, the period at which the reduction of the salt of iron takes place.

M. Richard remarks that, when a mixture of sulphate and hyposulphite has to be analysed, it is much more advantageous to estimate the hyposulphite by means of the salt of iron, than to estimate the sulphate by means of chloride of barium, since the operation indicated in the first place is very rapidly effected, whilst in employing a metrical liquor of chloride of barium, we are obliged to wait for a long time, until the precipitate of sulphate of baryta is entirely deposited.

The first two methods indicated may be very well turned to account, when it is required to estimate the hyposulphite alone in presence of a sulphate: we have only to read each time on the burette the quantity of metrical sulphuric acid or hypermanganate, and we have by a proportion the weight of the hyposulphite of the solution.

I shall not indicate here the series of numbers obtained in my experiments; suffice it to say, that in operating with mixtures composed of sulphate, sulphite and hyposulphite, the comparison of the two methods furnished me almost identical results; I attribute the slight differences to the difficulty of obtaining the precipitate of $\text{SO}^2 \text{Ba O}$ by means of the metrical solution of baryta salt, so that there might be some error arising from the equation $x + y = w - p$.

By using only a mixture of sulphate and hyposulphite, the three methods furnished perfectly concordant results.

The various samples of commercial hyposulphites which I have examined, contained from 5 to 13 and even 15 per cent. of sulphate, and 2 to 8 per cent. of sulphite.

Journal de Pharmacie, August, 1856.

RESEARCH for IODINE in MINERAL WATERS. By Professor LIEBIG.

WHEN the quantity of iodine contained in a liquor is so small, that nitric acid and starch furnish only a doubtful coloration, we obtain a very intense reaction by adding a trace of alkaline iodate and a little hydrochloric acid, the hydriodic and iodic acids react on each other in the nascent state, and liberate a much larger proportion of iodine than that contained in the iodide.

Liebig has proved, moreover, that with certain mother-liquors of

mineral waters, starch water and hydrochloric acid alone, give a blue coloration as deep as that which is obtained by means of chlorated water and hyponitric acid.

Annales de Chimie.

INORGANIC CHEMISTRY.

On the EQUIVALENT of ANTIMONY. By M. SCHNEIDER.

THE method which has been chosen for the determination of the equivalent of antimony is as follows; native sulphuret of antimony perfectly free from arsenic and other metals, was reduced by hydrogen; the comparison of the weight of the sulphuret and of the reduced metal has enabled us to fix the equivalent of antimony, that of sulphur being known.

The sulphuret of antimony used in these experiments was obtained from Arnsberg; the only impurity which it contained was a trace of quartz (about one-third per cent.), which is easily allowed for.

The reduction of sulphuret of antimony by pure dry hydrogen was performed in a refracting bulbed tube of glass, and was completed at a temperature which only volatilised traces of sulphuret of antimony. Care was taken to condense them by passing the gases through washing bottles.

By using the precaution not to cause the current of gas to pass through too quickly, the quantity of sulphuret of antimony lost never exceeded one milligramme, even when operating on six or eight grammes of sulphuret.

It is very difficult to remove the last traces of sulphur from the reduced antimony. To estimate the quantity, the metal obtained was dissolved in *aqua regia* (the quartz remained as residue, and was collected and weighed). The solution was evaporated to dryness, the residue was heated to drive off the greater part of the free acid, and, after treating it with boiling water, it was digested with pure carbonate of soda. The oxide of antimony having been separated by filtration, the sulphuric acid in the filtered liquor was next estimated. The quantity of sulphur deduced from the weight of the sulphate of baryta obtained, never exceeded one thousandth of the weight of the antimony.

By means of this method eight experiments were made, which agreed sufficiently, and which gave the following result as an average: 100 parts of sulphate of antimony, containing 71.48 of antimony and 28.52 of sulphur. Whence we deduce as the equivalent of antimony, the figures 1508.80 or 120.3 for $H = 1$.

The last experiments of Berzelius had fixed the equivalent of antimony at 1612.9, or about 129 for $H = 1$.

It is important to observe, that the arithmetical mean of the new equivalent of antimony (1503.8) and of the equivalent of phosphorus (387.5, Schrötter) is 9.456, which nearly approaches that of arsenic,

(937·5, Pelouze). According to this it would appear, that there exists between the equivalents of phosphorus, arsenic, and antimony, a relation analogous to those observed between the equivalents of calcium, strontium, and barium, and between those of chlorine, bromine, and iodine.

Poggendorff's Annalen, June, 1856.

On the ACTION of the OXYGENOUS COMPOUNDS of NITROGEN on IODIDE of POTASSIUM in the PRESENCE of WATER.

By M. A. BECHAMP.

M. BINEAU has said, that he does not believe that the nitric acid produced by storms remains long unneutralised, either by ammonia or by the lime of the dust which floats in the air, neither does he believe that this acid is found outside the drops of rain, the influence of which M. Schonbein advises so careful an avoidance of.

The result of this passage appeared to me, that the two authors attribute to nitric acid the property of displacing the iodine from iodide of potassium, without heat and in the presence of water. For some years, I have shown in my lessons in analysis, at the upper *Ecole de Pharmacie*, that pure nitric acid does not turn a mixture of iodide of potassium and starch blue, in circumstances analogous to those which M. Bineau supposes. Thus :—

Let us suppose a solution of iodide of potassium of 200^c, in which starch paste has been mixed; to a given volume of this mixture, let us add : (a) a drop of nitric acid, such as is kept in laboratories, the mixture turns blue instantly; (b) a great excess of the same acid diluted with water, into which has been passed, in the shade, a current of carbonic acid to drive off the nitrous gas; the mixture does not turn blue : (c) a great quantity of pure nitrate of potassa and dilute sulphuric acid, the mixture remains colorless : (d) but, if nitrate of potassa, whether fused and recrystallised or not, or nitrate of potassa which has been in contact with organic matters, operating as with c, the mixture becomes blue ; (e) the same will occur if we add traces of nitrite of potassa to the mixtures which did not turn blue.

We get the same results by steeping the ozonometrical paper of M. Schonbein in the same mixtures of water and nitric acid, or of water, nitrate, and dilute sulphuric acid.

M. Millon mentioned long ago, "as proof of the absolute absence of nitrous acid in nitric acid, that without heat it does not precipitate the iodine of the iodides, or the sulphur of the hydrosulphates, &c.;" I only recall these facts to those gentlemen who employ themselves with ozonometry. Nitric acid and the nitrates perhaps owe to nitrous acid the property of displacing the iodine from the iodide of potassium. But what would be the action of the nitrous acid on this binary compound? This I have not found anywhere, and this I now lay before the Academy.

When the preceding experiments are made with dilute liquors the iodine becomes free, without any disengagement of gas being perceived; the same thing occurs when nitrous acid is made to act on a solution of hydrosulphuric acid; the sulphur is deposited alone, and the nitrogen is found in the state of ammonia in the liquor; that is to say, we have—



By analogy we might admit the same explanation in the case of hydriodic acid, namely—



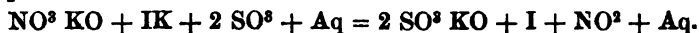
But it is clear that the reaction might also be interpreted in the following manner, admitting the reduction of NO^3 into NO^2 ,



To submit these two hypotheses to the test of experiment, I introduced into an appropriate apparatus, an equivalent of iodide of potassium and an equivalent of nitrite of potassa, dissolved in a weight of water equal to 20 times that of the first salt. I drove all the air out of the apparatus by a current of carbonic acid, and I added by degrees an excess of dilute sulphuric acid; the first drops of this acid caused a powerful reaction, and a considerable disengagement of a gas which became red in contact with the air. This gas was evidently binoxide of nitrogen, and as the apparatus contained no air, it was clear that the gas disengaged could only proceed from the reaction of the nitrous acid on the iodide; for if this binoxide had had for origin the decomposition of the nitrous acid according to the equation—



red vapors would have been seen in the apparatus itself; or there would have been none. Moreover, I ascertained that this gas was really pure binoxide of nitrogen, completely absorbable by a solution of sulphate of iron. The second hypothesis is, therefore, the real expression of the reaction, and we have—



Indeed, the volume of binoxide of nitrogen obtained in an experiment, would be equal to that which this equation enables us to calculate, namely, that we should obtain about 1300 cubic centimetres for ten grammes of iodine and five grammes of nitrite of potassa. The iodine is so completely separated, that the liquor is not more colored than an aqueous solution of this metalloid. As for the binoxide of nitrogen, it has no influence on iodide of potassium in the conditions of this experiment. Indeed, having modified the apparatus so as to pass this gas sheltered from the air into a solution of iodide and starch, I observed no coloration; but as soon as I admitted oxygen the blue coloration manifested itself, and extended more and more by the transformation of NO^2 into NO^4 . The protoxide of nitrogen is likewise without action on iodide of potassium; for having left ozonometric paper in this humid gas it underwent no appreciable change.

To sum up:—

1st. Ozonometric paper does not turn blue under the influence of pure dilute nitric acid; but the coloration may be attributed to the nitrous acid which always accompanies nitric acid, when it has been long prepared and is concentrated.

2nd. Nitric and hydriodic acids may exist together without heat in a dilute liquor, as takes place with the first acid and sulphuretted hydrogen.

3rd. Iodide of potassium reduces the nitrous acid in binoxide of nitrogen; which constitutes a new and very practicable process for the preparation of this gas.

4th. Carbonic acid does not displace the nitrous acid from nitrite of potassa.

5th. The binoxide and protoxide of nitrogen do not displace the iodine from iodide of potassium in the presence of water.

Comptes Rendus, No. 7, August 18th, 1856.

INDUSTRIAL CHEMISTRY.

On an IMPROVED PROCESS of MANUFACTURING SODA and SULPHURIC ACID. By M. E. KOPP.

THE manufacture of soda and sulphuric acid constituting one of the most important branches of industry, it is natural that it should have been, of late years especially, the subject of numerous labors and interesting improvements; however, it is easy to prove that the old process of Leblanc is still, with the exception of some slight modification, that which is followed by the largest manufacturers.

This fact arises from various causes, the principal of which are: the high degree of perfection to which the manufacture of sulphuric acid has attained; the very perfect condensation of the hydrochloric acid, which is effected by means of condensing apparatus and towers, and the numerous applications of this acid; the low price of the materials (lime and fuel) and the simplicity of the apparatus required for the conversion of sulphate of soda (salt cake) into carbonate of soda; and, finally, the circumstance, that the manufacture of sulphuric acid, in connection with that of soda and chloride of lime, forms a very complete whole, in which almost all the products are turned to account, either by direct sale, or by the reaction of some of them on each other.

However, Leblanc's process presents several disadvantages, which are universally admitted; the principal is, that it involves the almost complete loss of the sulphur contained in the sulphate of soda; again, it gives rise, in enormous quantities, to a product which has very few applications, and which, from its injurious qualities, is often the cause of great difficulties, from which it is sometimes very difficult to get free; and, finally, the treatment of matters in the calcination fur-

naces, for the preparation of crude soda, is very difficult, and success depends greatly on the skill and attention of the workmen.

The most important improvements which have been proposed in the manufacture of soda, by means of common salt, have been with the view of remedying the principal defect of Leblanc's system, namely, the total loss of the sulphur. Sometimes, various reactions have been tried with the object of passing by completely the previous transformation of the common salt into sulphate of soda, by means of sulphuric acid; sometimes, sulphates of little value have been substituted for this acid: finally, endeavors have been made to disengage the sulphur from the oxysulphuret of calcium in the state of sulphuretted hydrogen, which, on being burned, should afterwards give rise to sulphurous acid; other improvements have had for their object, the more complete condensation of the hydrochloric acid, the employment of native pyrites instead of sulphur, a more complete saturation of the soda with carbonic acid, the elimination or distinction of the sulphuret of sodium in the sodaic lixiviations, and, finally, a greater richness in pure carbonate of soda, in the commercial salts of soda.

If the inventions and improvements have not been more generally adopted in practice, it is because they often derange that economy and symmetrical combination of operations which we have already mentioned, and because they required the employment of more costly substances or apparatus, than those used in the ordinary process of manufacture; or, finally, because they were applicable only to certain circumstances or to particular localities. The new process about to be described appears to be exempt from the several inconveniences which have been mentioned.

Indeed, it presents the following advantages:—

1. It admits of our operating with the apparatus at present employed in the ordinary process of manufacturing soda and sulphuric acid.
2. It dispenses with the employment of lime and chalk, and consequently, obviates completely the production of oxysulphuret of calcium, the source of loss and numerous inconveniences in most manufactories.
3. It enables us to recover all the soda contained in the sulphate of soda, and to reconvert it into sulphuric acid with great facility.
4. It renders all loss of alkali impossible, since there is no longer any residue which we are obliged to throw away.
5. Finally, it renders the preparation of soda more independent of the skill of the workman, and enables us to operate, in the same time, and with the same apparatus, on more considerable quantities of matter.

The principle of the invention consists in the manufacture, by means of sulphate of soda, of a metalliferous crude soda, instead of the ordinary calcareous crude soda.

This new metalliferous crude soda, treated in a certain manner, produces, on one hand, an aqueous solution of carbonate of soda, and on the other hand, an insoluble residue of metallic sulphurets; these

metallic sulphurets roasted in a suitable manner furnish sulphurous gas and a residue of oxides, which is converted into sulphuric acid in the ordinary leaden chambers. This sulphuric acid serves for converting common salt into hydrochloric acid gas and sulphate of soda, and by means of the sulphate of soda and metallic oxides, a fresh quantity of metalliferous crude soda is produced.

From this it results, that in our manufactories, supposing no loss to occur, unlimited quantities of salt may be converted into carbonate of soda and hydrochloric acid, requiring only the fuel and the quantity of metal of soda necessary for converting the sulphurous acid into sulphuric acid.

The following details will suffice to give an idea of the course of the operations, and I shall confine myself to describing the manufacture of *ferruginous crude soda*, iron being of all metals that which presents the greatest advantages, both, on account of its low price and on account of the chemical properties of its oxides and sulphuret.

The proportions indicated, are those which are employed in a large manufactory in Lancashire (that of Messrs. Blythe and Benson, at Church, near Manchester), which produces several thousand tons per annum by this new process.

First operation—mixture of the materials—

125 kilogrammes of dry sulphate of soda are very intimately mixed with 80 kilogrammes of peroxide of iron and 55 kilogrammes of charcoal.

These proportions are not absolute ; they may be varied according to the purity of the sulphate of soda, the peroxide of iron, and the charcoal.

The following observations will suffice for guiding the manufacturer with regard to the matters to be mixed, the proportions and the total weight of the mixture.

With regard to the materials—

The sulphate of soda may, without inconvenience, contain more or less common salt, only, in this case, the peroxide of iron and charcoal, must be added only in proportionate quantity to the pure and dry sulphate of soda contained in the crude sulphate. This sulphate is used just as it comes from the calcination furnaces, as the residue of the action of sulphuric acid on common salt, care being taken to break up the larger pieces.

The peroxide of iron should be weighed dry and in very fine powder ; it is important that it should be as pure as possible.

For the first operation, instead of the artificial or native peroxide (hæmatite, oligist iron), we may use carbonate of iron (spathic iron), magnetic oxide of iron, iron scales, or even granulated cast iron, or metallic iron ; but in the latter case, the quantity of charcoal must be diminished, since metallic iron has a reducing action on sulphate of soda.

As we shall see further on, whatever may be the compound of iron employed, and even when metallic iron is used, in the first operation, we shall afterwards have always to operate on peroxide of iron.

The mixture of sulphate of soda and peroxide of iron, which is obtained as a residue in the process, which consists in decomposing common salt with sulphate of iron, is admirably adapted to my new process of manufacturing soda, since, if the proportions have been properly adjusted, we have only to add the quantity of charcoal required.

As regards the charcoal, we may make use, either of wood charcoal, or of coke, or of small coal, of lignite or anthracite, or even of any other organic reducing substance, only the quantity of it must be increased in proportion as the reducing power diminishes. In England small coal of ordinary quality is used.

With regard to the proportions:—

It must be observed, that it is indispensable to employ such a preparation of peroxide of iron, that the iron which it contains may combine with all the sulphur of the sulphate of soda, in order to form the combination— $S Fe$ (200 of sulphur + 350 of iron); to 9 parts of pure and dry sulphate of soda, it is proper therefore, not to employ less than 5 of pure and dry peroxide of iron; it is advantageous to use a slight excess of oxide of iron.

When the peroxide of iron contains lime, it must be previously freed from it by treating it cold or at a gentle heat with very dilute hydrochloric acid, and afterwards washing carefully; for the lime, in the course of the operations, would be successively converted into sulphuret of calcium, then into sulphate, and then again into sulphuret of calcium, &c., uselessly augmenting the volume of matters to be operated on, and consuming in utter waste charcoal and heat.

The charcoal should not be employed in excess, first because it favors the presence of sulphuret of sodium in the salt of soda, and that despite the employment of a proportionate quantity of oxide of iron, and also because the excess of charcoal remaining in contact with the sulphate of iron, we should afterwards obtain, in roasting the latter, sulphurous acid mixed with carbonic acid.

It is proper therefore to diminish the proportion of charcoal until we discover in the blocks of ferruginous crude soda, the presence of a very minute quantity of undecomposed sulphate of soda.

With regard to the total weight of the mixture to be introduced into the calcination furnace, this must depend principally on the dimension of the furnace, I shall only observe that we may operate on a charge even twice as large as that which is now used in the preparation of ordinary crude soda; this is owing to the ferruginous crude soda being much more easy to work, the mixture becoming much more rapidly pasty, and finally almost liquid.

The foregoing detail enables the manufacturer to compose a mixture suitable and appropriate to the furnaces and apparatus already in use.

Second Operation.—Calcination.

The sulphate of soda, peroxide of iron, and charcoal, having been intimately mixed, are introduced into the calcination furnace, which is exactly similar to the furnace used in the preparation of ordinary crude soda.

In order to turn the heat completely to account, the furnace should have two, or even three stages, the lower stage being that nearest to the heat. The furnace contains, in the latter case, at one time, three charges which are brought down in succession, and which are replaced as the completely calcined charges are drawn.

The treatment of the mixture in the furnace is similar to that of the mixture for preparing ordinary crude soda, and the phenomena observed are almost identical, the matter softens, becomes pasty, then more and more fluid, in proportion as it is moved about and brought nearer to the fire; yellowish flames are abundantly disengaged from all points of the surface, producing a kind of ebullition; when the reaction, which has been very brisk, begins to abate, when the flames become less voluminous and less abundant, and when the mass sinks down and becomes tranquil, presenting the appearance of a thin, homogeneous, semi-liquid paste, the operation is complete, and the mass must be quickly withdrawn from the furnace, and run while still red-hot into a kind of iron waggon, in which it is allowed to cool and solidify, after being covered, in order to keep it as much as possible from contact with the air.

After cooling, the crude ferruginous soda is presented under the form of a parallelopiped block, of blackish appearance, more or less porous, very hard, and of very great density. When the operation has been well performed, the block is very dense, its surface has a coppery reflection, and, when it is broken, the fracture presents an uniform crystalline appearance, with a very brilliant greenish metallic reflection.

(To be continued.)

NOTE on the PRECIPITATION of VARIOUS SALTS from their SOLUTION.

By M. F. MARGUERITE.

WHEN liquid hydrochloric acid is mixed with a solution of chloride of sodium, the salt is immediately, if not completely precipitated. But if we drive a current of hydrochloric acid into a saline solution until no more can be absorbed, the whole of the salt is precipitated with the exception of a few thousandths, and the liquid hydrochloric acid is pure enough to be sold in commerce.

When we operate on a mixed solution of chloride of sodium and chloride of potassium, the former salt is precipitated first, so that by dividing the operation we may effect, to a certain extent, the separation of the two salts. The insolubility of the chlorides of sodium and potassium in hydrochloric acid is such, that under its influence the sulphates of soda and potassa are decomposed into insoluble chlorides, and into free sulphuric acid eliminated in the liquor. This decomposition may go very far. Thus, hydrochloric acid being passed to saturation into a saturated solution of sulphate of potassa, converts nearly 70 per cent. of the latter into chloride of potassium, eliminating in the liquor a corresponding quantity of sulphuric acid.

The double sulphate of potassa and magnesia undergoes a similar decomposition. The sulphate of magnesia does not participate in this reaction, and, like the chloride of magnesium, it is not precipitated by hydrochloric acid, except in particular conditions of concentration.

Sulphate of soda in the same circumstances, is decomposed more completely, owing to the greater insolubility of common salt in hydrochloric acid.

The precipitation of common salt and chloride of sodium, appears to me susceptible of an industrial application.

1. In the preparation of a quality of salts intended for particular uses.
2. In the production of crude salt.
3. In the separation of chloride of potassium from the mother liquor of salt marshes.

The peculiar state affected by the salt precipitated by hydrochloric acid, its state of extreme division, its whiteness, its brilliancy, its perfect purity, and the simplicity of its preparation, render it superior for use at table to every kind of salt obtained, either by trituration or by evaporation in a boiler. It is obtained perfectly pure, indeed, by operating on natural salt water, and, better still, on redissolved crude salt; the acid liquor is decanted, and the precipitate is dried in an oven properly heated. The salt is thus purified from the hydrochloric acid with which it is impregnated. After having dried the precipitate, it may be washed with the saline solution, which has afterwards to be precipitated. This washing removes almost all the hydrochloric acid. Finally, the remaining excess of acid may be saturated with carbonate of soda, the expense of which would not be greater than that necessary for heating the oven to desiccation; this enables the salt to be spread and dried simply in the open air.

The current of hydrochloric acid keeping the liquor in a state of continual agitation, causes the precipitation of the salt in exceedingly fine grains; but the salt may be obtained in large crystals when the hydrochloric acid is disengaged, and dissolved on the surface of the liquid in a state of repose, as ordinarily occurs in the condensation of this acid.

As regards the separation of chloride of potassium from the mother liquors of salt marshes, it possesses a certain degree of interest, in as much as it is more prompt, more complete, and less expensive than that at present in use, which consists in concentrating the mother liquors with the aid of heat, and abandoning them to crystallisation in conditions of temperature which are not met with in a certain manner. Moreover, the product which is obtained is a triple chloride of sodium, potassium and magnesium; a compound which requires to be purified by successive crystallisations, which occasion loss of time, fuel, and matter.

By the action of hydrochloric acid on the mother liquors, we obtain almost the whole of the chlorides of sodium and potassium which they contain. The chloride of magnesium is not precipitated, and remains in the solution of hydrochloric acid, which may be employed in the manufacture of chloride of lime.

The chloride of potassium is separated from the chloride of sodium without difficulty, either by dividing the precipitation by hydrochloric acid, which first eliminates the chloride of sodium, or by the difference of solubility of the two salts, hot and cold.

As regards the production of crude salt by this means, the advantages do not appear doubtful. Near to salt springs there are generally manufactories of sulphate of soda, which are a permanent source of hydrochloric acid. This acid, in the south, for example, is not entirely collected, and in many places it is difficult to find a sale for it, because of the expense of conveyance. Supposing all the hydrochloric acid produced to be sold, 100 kilogrammes of salt converted into sulphate of soda would reproduce rather more than 88 per cent. of their weight of chloride of sodium. Supposing it were found more advantageous to apply all the hydrochloric acid in precipitation, a much larger quantity of salt would be regenerated in this manner. Indeed, 100 kilogrammes of chloride of sodium decomposed by sulphuric acid give 62.59 kilogrammes of hydrochloric acid gas, which, to give a solution of the density of the acid of commerce (1.18) require 109.1 kilogrammes of water. This quantity of water, at the ordinary temperature, would dissolve, and consequently precipitate 38.18 kilogrammes of salt.

The solution containing hydrochloric acid, if not cold, on being gently heated, will give 43.2 kilogrammes of acid, which may precipitate a fresh quantity of salt amounting to 26.45 kilogrammes. The solution deprived of all the gas which it can yield, will represent the acid with 16 equivalents of water, which will be useful for some purposes.

In these two operations, the quantity of salt precipitated will be 64.65 kilogrammes. These are the theoretical numbers. If we take the quantity of salt reproduced at 50 per cent., instead of 64.65 kilogrammes, this estimate cannot be regarded as exaggerated, when we consider the efficacy of the means and the simplicity of the operation.

It is to be expected, that the application of this process would produce advantageous results in the salt manufactories of the east, where the preparation of this salt is effected by means of heat, and in the manufactories of the south of France, where the production of hydrochloric acid is far from finding a satisfactory outlet.

The principle of this operation consists in the employment of a volatile agent, which, after having served for precipitating the salt, may be driven off by heat without leaving any impurity behind. By virtue of the same principle, various other salts may be eliminated from their solution, but less completely. Carbonate of soda may be precipitated by ammonia in a state of great purity, from a solution of crude soda. The crystalline salt which is obtained, dried in an oven, does not retain the slightest trace of ammonia. It may be conceived, besides, that the same quantity of ammonia may be used indefinitely, since it is sufficient to heat the ammoniacal liquor to make it give up all the gas which it has dissolved. The ferrocyanide of potassium and other salts also, are precipitated by ammonia; but I content myself

with mentioning those examples which may present some interest in an industrial point of view.

Comptes Rendus, No. 1, July 7th, 1856.

GENERAL OBSERVATIONS *on the MATERIALS EMPLOYED in CONSTRUCTIONS in the SEA.* By MM. CHATONEY and RIVOT.

SECOND PART.—*Summary of the most important practical facts.*

THE homogenousness and intimate mixture of the primary hydraulic matters, is an indispensable condition for the developement of their qualities, and for the formation of good mortars.

By baking the calcareous stones of Fécamp, after grinding them, to remove the heterogenousness, limes were obtained which set rapidly, and which acquired under water a hardness three or four times as great as those produced by the unprepared limestone.

Pulverised *silex*, baked with fat lime, produced a hydraulic lime of excellent quality. In the experiments made at Havre two years ago, it set under water in three or four days, and acquired a hardness which was comparable and sometimes superior to that of Portland cements, which had been submerged for one or two months.

The envelope of carbonate of lime which forms on the surface of all these mortars, is a great protector against the action of the sea-water. This natural and general means of preservation, has more or less power according to the locality. The mortars of the basin of Vauban, made with but slightly hydraulic lime, have been so well protected for 14 years by this covering of carbonate of lime, that we have no hesitation in recommending the use of this same lime as sufficient in any analogous case.

A *previous digestion* of the hydraulic materials, under the influence of moisture, prepares the chemical actions and contributes essentially to the success of all mortars. It is more or less indispensable, and should last a long time when puzzolanes are employed. We are not able to give very conclusive proofs of what we advance, as we have only lately become convinced of the great influence of digestion on the quality of mortars. Previous digestion is, moreover, not a new fact. It has not been published because its importance was not sufficiently recognised, but it must be carefully examined, following the indications of chemistry and learning by experience. The storing of Portland cement before using it is only a digestion in moist air, to which manufacturers have been led by experience.

The nature of the materials to be used in each locality should vary with the composition of the water, and the situation and nature of the works.

CONCLUSIONS.

1st.—The materials to be used in marine works should be composed

of silica, alumina, and lime, or better still silica and lime only, in such proportions as to form silicates of quite definite composition, such as are found in all mortars which have resisted the action of the sea.

They should contain a quantity of free lime, which should vary with the composition of the sea-water, and which should be sufficient to form on the surface, the protecting covering of which we shall speak again.

2nd.—A proper chemical composition of the hydraulic materials is not sufficient to form good mortars. It is quite indispensable that they should be very homogeneous so that the greater part of the materials should be useful, that the combination should be perfect and the setting regular.

3rd.—Mortars will resist well, only when protected against the penetration of the water by a compact texture and a covering of carbonate of lime.

The free lime is necessary to form, in combination with the carbonic acid of the water, the protecting covering of carbonate of lime. A covering of shells, marine plants, &c., may replace the carbonate of lime and prevent the decomposition.

4th.—The processes for the manufacture of mortars, vary with the species of materials used.

They have a considerable influence; the greatest is, perhaps, on their definitive resistance. The processes should have for their object:—To prepare the hydraulic materials in such a manner that the chemical combinations which will ultimately exist in the mortars when they have become stable, should be completed before the manufacture of the mortars, and that they should only require to be hydrated when wanted, to render the mortars very compact; and, consequently, very impermeable. This can only be attained, in most cases, by a longer or shorter digestion of the materials, and the mortars under the influence of moisture.

5th.—It is very seldom that we find natural materials of such a composition and homogeneousness, as to produce hydraulic limes capable of resisting sea-water. They may be advantageously replaced by artificial limes, made of pulverised silex, and rendered appropriate to each locality.

6th.—Cements have hitherto given good results, and are very useful for marine constructions. Their use requires peculiar precautions.

7th.—Natural puzzolanes may be used, only with the express condition of being mixed with lime, and being subjected, before being used, to a prolonged digestion. They should be mixed with fat limes in preference to hydraulic limes.

8th.—Artificial calcareous puzzolanes always present many difficulties. They should not be used in constructions in sea-water, because of the irregularity of the mortars in which they are introduced. Those puzzolanes which do not contain lime should give the most regular products.

9th.—The destructive action of sea-water differs in various localities. The composition of the mortars should be varied according to

the nature of the waters in which they are to be used, so that the quantity of free lime should be in proportion to the amount of carbonic acid, or sulphuretted hydrogen contained in the water.

10th.—The preparation of the materials and the manufacture of the mortars for constructions in the sea, will always require operations and care which must render the price high, but we can confine the use of them to those portions which are directly exposed to the action of the waters.

11th.—Complete and necessarily very lengthy experiments, made in different localities, and in the various conditions in which constructions may be required, will be necessary to solve all the questions raised in this memoir.

Comptes Rendus, No. 7, August 18th, 1856.

AGRICULTURAL CHEMISTRY.

On the PART PERFORMED by the NITRATES in the ECONOMY of PLANTS.

By M. G. VILLE.

I. AFTER having treated of the means of estimating the nitrates in presence of organic matters, it remains for me to speak of the influence of these salts on vegetation. This will be the object of the present memoir. In several former communications, I have sought to prove that certain plants cultivated in calcined sand, without any addition of nitrogenous matter, are developed by fixing the nitrogen of the air. But there are other plants which do not vegetate in these abnormal conditions.

I now submit to the Academy other experiments on a plant of this kind, which was cultivated in calcined sand with the aid of nitre. We shall see how these new experiments have led me to the same results as the former.

II. Under the influence of the nitrates, and of nitrate of potassa in particular, plants thrive in calcined sand as in good earth. From the first day following germination, the leaves present a beautiful green tint, and the vegetation continues with remarkable activity. In proportion as the duration of the experiment is prolonged, the nitrate which has been added to the soil disappears. By means of the processes which I have made known for estimating the nitrate, the precise moment of its complete disappearance may easily be seized. If the experiment be arrested at the end of this first period, the substance of the plants, exhausted with boiling water, does not give the slightest indication of the presence of a nitrate, or, if any be found, it is only feeble traces. Burned by means of soda-lime, the crop shows on the contrary, a quantity of nitrogen equal to that which the seeds and the nitre contained at the commencement of the experiment. Thus, the plants have turned to account all the nitrogen which the soil had received, and the nitre, changing in state, has served at once for the production of their nitrogenous proximate principles, and for the

internal organisation of their tissue ; but, we may remark, that up to this point the plants have not derived from the atmosphere an appreciable quantity of nitrogen.

I shall bring in support of these conclusions two experiments made at different periods, and I may mention, once for all, that the culture was effected in calcined sand.

First Experiment, 1855.—On the 22nd of July, eight seeds of winter colza were sown ; 0·50 gr. of nitre was added to the sand used as soil, or 0·0692 gr. of nitrogen ; the crop was gathered on the 6th of September :—

	gr.	Nitrogen of the seed. gr.	Nitrogen of the nitre. gr.
Seed dried at 100° C. (212° F.)	0·025	0·0013	0·0692
			Nitrogen of the crop. gr.
Crop dried at 100° C. (212° F.)		5·45	0·0708

The crop was equal to 218 times the weight of the seed.

Second Experiment, 1856.—Prepared and conducted like the foregoing, and commenced on the 2nd of April, and concluded on the 12th of June :—

	gr.	Nitrogen of the seed. gr.	Nitrogen of the nitre. gr.
Seed dried at 100° C. (212° F.)	0·025	0·0013	0·0692
			Nitrogen of the crop. gr.
Crop dried at 100° C. (212° F.)		5·02	0·068

The crop was equal to 200 times the weight of the seed. Thus, with 0·50 gr. of nitre, eight seeds of colza produced in ligneous development and foliage 200 times the weight of the seed.

I have said, that these experiments prove that plants absorb and assimilate the nitrogen of the nitrates. To this conclusion, I may now add two others. The first is, that nitre is not formed spontaneously in the sand employed as soil, and, on this point, I must refer to my memoir for the detail of the experiments which I have made in this respect. Not only have I never been able to detect the formation of a nitrate in a mixture of calcined sand and vegetable ash, but even when I have added to the sand, gelatine and seeds of white lupin, the result was always negative. M. Reiset has arrived at the same conclusion, in a remarkable and more general work, on the nature of the products which are formed during the decomposition of organic matter.

The second conclusion is that all the materials, sand, brick, and distilled water, employed during the course of the experiment were absolutely free from nitrogen. Thus, absorption of the nitrates and assimilation of the nitrogen of these salts by the plants, and the absence of spontaneous nitrification in the sand used as soil, are the

results which are deduced from this first portion of my work.

Instead of adding to the sand 0.50 of nitre, suppose that we raise the proportion to one gramme, all the other conditions of the experiment being observed; things occur quite differently from the former case. The vegetation presents more activity; the plants are better developed; this developement is not arrested at any time. In the end, the crop contains much more nitrogen than the seeds and nitre employed.

The following are two examples in support of this result; a greater number will be found in my memoir.

First Experiment, 1855.—On the 13th of July, ten seeds of winter colza were sown; one gramme of nitre was added to the sand. The crop was gathered on the 4th of October:—

	gr.	Nitrogen of the seed. gr.	Nitrogen of the nitre. gr.
Ten seeds of colza dried at 100° C. (212° F.) }	0.031	0.0015	0.01384
Crop dried at 100° C. (212° F.)		15.30	00.374
Nitrogen derived from the air			0.234.

The weight of the crop was 493 times the weight of the seed.

Second Experiment.—Commenced on the 7th of January, 1856, and ended on the 2nd of May. An accident disturbed the course of this experiment. At two different times, the proximity of the orifice of a hot air tube dried and almost wasted the leaves: the plants suffered greatly:—

	gr.	Nitrogen of the seed. gr.	Nitrogen of the nitre. gr.
Ten seeds of colza dried at 100° C. (212° F.) }	0.031	0.0013	0.1384
Crop dried at 100° C. (212° F.)		10.77	0.192
Nitrogen derived from the air			0.052

The weight of the crop was 347 times that of the seed.

Thus, we have two pots prepared in the same manner, with the same sand to which the same ash had been added, and watered with the same distilled water. Both were put into the same place. In that which received 0.50 gr. of nitre, a period arrived at which the vegetation ceased. From this moment, the crop contains all the nitrogen of the seeds and of the nitre, but it does not contain an excess. In the pot which had received one gramme of nitre, on the contrary, the vegetation follows a constantly progressive and ascending course, and, after a growth of two months and a half, the crop

begins to contain more nitrogen than the seeds and the nitre together, and this excess goes on always increasing.

The excess of nitrogen comes from the atmosphere, and the difference between the two results is due to the plants beginning to absorb the gaseous nitrogen only when they have attained to a certain development; 0.50 gr. of nitre not being able to bring eight seeds of colza to this period, there is no absorption of nitrogen, the result is negative. With one gramme of nitre, the direction of the result changes, there is an absorption of nitrogen, because the plants have attained and passed the period at which the absorption of the nitrogen is produced.

The effects which I notice here are not isolated facts, which may be looked upon as exceptions. They are, on the contrary, of the same order as those which certain seeds present, which produce in the calcined sand only rudiments of the plant, whilst other seeds, sown in the same condition produce complete plants.

A full discussion of these effects will be found in my memoir.

Thus, it results from the foregoing, that by means of nitre, we may, at will, produce or not produce an absorption of nitrogen. When it does take place, it is at the expence of the nitrogen of the air.

If it be desired to refer its origin to the traces of ammonia existing in the atmosphere, I would ask, why is the result different, according as we operate with 0.50 gr. of nitre, or with 1 gr.?

In the third part of my memoir, I enquire whether the nitre is assimilated in the state of nitre, or whether it passes to the state of ammonia previous to its assimilation? To this question I reply, that it is assimilated in the state of nitre.

Indeed, if the nitre is converted into ammonia, for an equal quantity of nitrogen, sal-ammoniac should have a greater action than nitre. Now, the contrary is the case. Nitre has a greater action than sal-ammoniac. For an equal quantity of nitrogen with nitre, the crop amounted in one case to 15.30 gr., and with sal-ammoniac it remained at 6.80 gr. The nitre, therefore, does not pass to the state of ammonia.

In the fourth and last part of my memoir I enquire, whether the absorption of the nitre is integral, whether it relates to all parts of the soil at once and in the same time, or whether the assimilation of the nitrogen is followed by a disengagement of oxygen, as occurs with carbonic acid. At present, my experiments do not enable me to answer this question with sufficient certainty. I confine myself therefore to putting it, and reserve to myself to treat of it in a special work.

Finally, on this point, I examine whether it is true, that all assimilation on the part of vegetables is the result of a reductive action, and whether, from the assimilation of carbon being the result of such a reaction, it must be admitted that it is the same as regards all other bodies. Taking nitrogen, for example, I conclude in the negative. Indeed, when the nitrogen comes from a nitrate, a reductive action may be admitted; but, when it comes from ammonia, how can such

a reaction be conceived, oxygen not being one of the constituents of ammonia?

Finally, returning to the principal subject of my memoir, I sum up in the following conclusions:—

1. A pot filled with calcined sand, to which are added a few grammes of vegetable ash, and which is left to the free action of the air, does not become the seat of a formation of nitrate. The result is also negative when gelatine and lupin seeds are added to the sand.

2. The plants absorb and assimilate directly the nitrogen of the nitrates.

3. The seeds which would give in calcined sand, only rudiments of plants, produce, on the contrary, plants which vegetate in this same sand with the aid of a nitrate, and they absorb or do not absorb the nitrogen of the air, according as the quantity of nitre employed is, or is not sufficient to make them go through a first vegetation.

4. For an equal quantity of nitrogen, nitre produces on plants a greater action than sal-ammoniac does; whence, I conclude that this nitre is not changed into ammonia, either before or after its assimilation.

Comptes Rendus, No. 2, July 14, 1856.

MEMOIR on the ORIGIN of NITRE. By M. DESMAREST.

FIRST PART.

It results, says the author, from the facts contained in this memoir.

1. That the nitrogen and oxygen of the air are not susceptible of combining, under the influence of electricity, to form nitric acid.

2. That this acid is not formed under the influence of ozone, or when aerated water is decomposed by electricity.

3. That it is not formed by the oxidation of the nitrogen of ammoniacal gas or of organic matters, at the expense of the oxygen of the air.

4. That, finally, it is formed only when nitrogen is found in presence of an excess of oxygen, that is to say, in a case which does not ordinarily occur in nature.

These conclusions, adds the author, will receive further confirmation in the second part of this work, which will be devoted to showing how the nitrification of stones is produced.

Comptes Rendus, No. 2, July 14, 1856.

SECOND PART.

An attentive study of the causes of nitrification have shown me:—

1. That it is produced in all porous bodies, of whatever nature: thus rotten-stone, plaster, chalk, brick, freestone, sand, wood and charcoal, are susceptible of being charged with nitre when they are placed in favorable conditions.

2. That in walls, the nitrified portions are entirely distinct from those which are not so.

3. That they exhibit very clearly defined stains, sometimes of great extent in ancient constructions, and differing considerably in color from the stains of simple moisture, which are not nitrous.

4. That these stains proceed ordinarily from the privies, or places into which the liquids which escape from them pass by infiltration.

5. Finally, that in pigeon-houses and poultry-houses, where there is no escape of fluid, and where, nevertheless, powerfully ammoniacal vapors are disengaged, the walls are nitrous only when they are sufficiently raised from the soil.

It evidently resulted from the whole of these facts, that nitrification is not produced by a gaseous emanation, but only by the progressive imbibition of a liquid; now, as urine is the liquid which most ordinarily escapes from privies, and as, moreover, I have ascertained that it is sufficient of itself for producing the nitrification of walls, it followed naturally that it was the principal, if not the only cause of nitrification. The question was then reduced, in fact, to determining how it is produced. The long and tedious researches to which I devoted myself with this view, led me to this unexpected conclusion, that animals have not the power of forming nitric acid, and that the nitre which is found in their urine is foreign nitre. It was necessary now to ascertain the source from which they derive it; vegetables naturally appeared to me to be the source.

I collected, in the country, at different places and at different seasons, a great number of plants, those which are most commonly known. Some of them, collected from the species which ordinarily contain nitre, such as borage and the sun-flower, had been sown expressly in difficultly accessible places, and in soils which gave no appearance of nitre. But for still greater certainty, the same species were also cultivated under a glass case, in an artificial soil composed of coarse lime, stone in powder, washed with distilled water, and to which I had added as manure, powdered sheeps' bones, or a manure composed of human urine, fermented for a very long time with horse-dung and not containing the least portion of nitre. Distilled water was always used for watering, and the most minute precautions were taken for isolating the pots which contained the plants, and to guard against the nitre which might come from without.

The experiments which I made for proving the presence of nitre in these plants, have shown me that plants which grow near dwellings, on the sides of roads, and in all other places frequented by animals, ordinarily contain nitre; but that we do not find it, on the contrary, in those which grow in the fields, in places inaccessible to animals, or in an artificial soil perfectly free from nitre, that, moreover, in these last conditions, plants which ordinarily contain nitre, such as borage, the sun-flower, and pellitory, appear to vegetate only with difficulty; that their seeds do not always spring up, and that when they produce plants, the latter are so poor that borage, for example, scarcely attains the height of ten centimetres (about four inches) and the sun-flower not more than double the height.

The natural conclusion from these results was, that vegetables have

not the power of forming nitre, but that what they contain they derive from animals. I thus found myself in a circle, out of which I long despaired to find a way; however, I did not lose courage. I recommenced the attempts I had made a hundred times, to effect the oxidation of the nitrogen of the urine; I continued also to collect new observations on the cause of the nitrification of plants. I had to wait long for success; but in the end I made an observation which gave me a clue to the solution of this difficulty; I perceived that the sunflower, which, when cultivated in the open fields grew only with difficulty, and did not give nitre, grew, on the contrary, with the greatest facility and contained a considerable proportion of this salt, when cultivated in a garden. The cause of this difference could not be in the presence of manures, since these are supplied to plants in fields as well as in gardens, and since I had proved, moreover, that well rotted dung did not contain nitre, and that none was produced by its mixture with the earth. Neither could this cause be attributed to the presence of animals, since they are ordinarily excluded from gardens, and that urine is not thrown into them; it could only reside, therefore, in the artificial watering which cannot be given to plants in fields, but which garden plants received. Herein, appeared to me, the cause of the nitrification of these plants; so that if I have correctly observed these facts, if I have not deceived myself in the conclusions which I have drawn from them, if I have proved that nitric acid is formed only by the oxidation of the nitrogen of the air or of organic matters, if I have proved also that it is not a product of either animal or vegetable, I am forced to admit, that nitre is a salt of mineral origin, that animals derive it from the earth, in the well or spring water which they drink, and which, passing into their urine, effects the nitrification of stones and earths, and consequently that of plants, an effect which may also be produced by artificial watering.

Comptes Rendus, No. 6, August 11, 1856.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

EXPERIMENTAL RESEARCHES ON ANIMAL HEAT.

By CLAUDE BERNARD.

THE remarkable property which is peculiar to warm blooded animals of perpetually producing heat to maintain their bodies, at a nearly even temperature, and to resist the cooling circumstances which surround them, has been the subject of the researches of many physicists, chemists and physiologists.

It is now generally admitted, and doubtless with good reason, that this production of heat is connected with the accomplishment of the physical and chemical actions which ordinarily accompany the vital phenomena. Still, we are far from having sufficiently exact notions as to the multiplicity of causes which must be considered before we

arrive at the solution of this complex problem, and MM. Regnault and Reiset, in their work on animal respiration, considering the production of animal heat, not only in its relations to the consumption of oxygen, but also in the whole of its physico-chemical conditions, declare that, in the present state of science, it is impossible to obtain a satisfactory account of the phenomenon which now engages our attention.

On the other hand, the organic conditions which may modify the phenomena of animal heat, are scarcely suspected. These physiological influences may, however, intervene in a powerful manner, either for the production of heat, or to regulate it, that is, to cause its partition among the various parts of the body.

I published in 1852, a remarkable experiment, in which I found that cutting certain nervous filaments was sufficient to cause a sudden considerable and persistent augmentation of the heat of the organs in which these nerves are distributed. From this time these facts, which have been confirmed by many physiologists, have not remained solitary. In studying the vital conditions of heat among animals, I have found many other phenomena of the same nature, which I shall describe in the course of the present work. I shall then endeavor to explain them, and to deduce thence some consequences which appear to me likely to throw some light on the theory of animal heat.

But, to appreciate these physiological states exactly and to understand the mechanism of the singular modifications which are produced locally, in the temperature of portions of the body, by organic influences artificially induced, it appeared to me especially indispensable to know, as a term of comparison, the temperature of these same parts in the normal or physiological state.

This led me naturally to form experimentally a kind of calorific topography of the blood and various animal tissues. It will be seen that this first work showed me some important facts, and that these results are of a nature to modify the most general opinions as to the distribution of heat in the blood, and in the tissue of the various parts of the body.

I determined at the same time as the temperature of the organs, that of the blood which circulates in their tissues. The blood may, indeed, be considered as the hot liquid charged to distribute, by means of the arterial and venous channels, heat to all parts of the animal machine. Now, as man and warm blooded animals ordinarily live in a circulating atmosphere, the temperature of which is lower than that of their bodies, experience has long since proved that the sanguineous liquid becomes cooled by parting with its heat to the tissues whose temperature has always a tendency to diminish. But as on the other hand, the temperature of the animal remains pretty nearly constant, this necessarily implies that the blood becomes warm in some other organs which may thence be considered as, in some measure, the furnaces of animal heat.

Now, which are exactly, in the ordinary physiological conditions,

the organs in which the blood cools, and which are those in which it again becomes warm? Such was the first question which I proposed at first to examine experimentally; for it must be clear that this is a first step which is indispensable, before we can enter on the other questions relative to the calorificity, that is, the production of heat in the animal fluids and tissues.

The question being thus put, the experimental process was very simple, and consists in taking simultaneously in a vessel, the temperature of the blood entering an organ and the temperature of the blood as it leaves the same organ. By comparing them, we shall ascertain about the amount of the modifications of heat which the blood has undergone in passing through the organic tissue, which is the subject of the examination.

Notwithstanding the apparent simplicity of this method of experiment, it will easily be imagined that it is surrounded by a great number of very serious difficulties. Such as, for example, the trifling differences of temperature which we have to observe, the difficulties inherent to the employment of thermometric instruments on living animals, at the same time, that these disturb the general physiological conditions of the functions and the physical state of the organs which we have sometimes to expose, &c. All these circumstances, which betray the imperfection of our present methods of investigation, are capable of producing grave causes of error, unless we take care to lessen them or avoid them by various precautions, and especially by frequently repeated comparative experiments, as we shall describe with respect to each series of experiments contained in this work.

So as to render more clear the explanation of the results of this topographical examination of temperature, in the various parts of the body, I have ranged my experiments under several heads, and I have examined successively the modifications of temperature, which the blood undergoes in passing through—

- 1st The digestive apparatus.
- 2nd The pulmonary apparatus.
- 3rd The genito-urinary apparatus.
- 4th The apparatus of relative life, &c.

I shall at this time touch only on the first portion of this list, and shall speak of the modifications which the temperature of the blood undergoes in circulating through the digestive apparatus.

I must mention that my experiments have all been made on the mammifera and chiefly on dogs.

On the Modifications which the temperature of the blood undergoes in passing through the digestive apparatus.

I am not aware that any one before myself paid attention to the modifications in the temperature of the blood which circulates in the organs of digestion. My first researches on this subject were commenced in 1848, and in 1849 I communicated to the *Société de*

Biologie my experiments on the augmentation of the temperature of the blood in passing through the tissue of the liver.

The digestive apparatus receives its blood from the ventral aorta by the coeliac trunk and the mesenteric arteries. After having circulated in the capillary vessels of the intestinal canal, pancreas and spleen ; and having aided in the accomplishment of the various secretory and other phenomena which take place in these organs, this blood arrives at the *vena porta* and so into the liver, in which it is subjected to fresh elaboration in the capillaries and finally passes out by the hepatic veins which pour it into the inferior vena cava, not far from the heart. It is thus seen that the circulation of the digestive apparatus differs from that of the other apparatus of the animal system in this, that the arterial blood, after having passed through the capillary system of the intestines and having been changed into venous blood, does not go immediately into the general venous system, but passes through another system of capillary vessels, that of the liver, which renders it again venous and new before it reaches the heart. We shall consequently have to examine in this apparatus, the temperature of the blood in three orders of vessels ; 1st—in the arteries which take the blood to the intestinal canal ; 2nd—in the *vena porta* which contains the blood which has passed through the intestine and which takes it to the liver ; 3rd—in the hepatic veins which pass to the inferior *vena cava*, and which contain the blood which has passed through the entire digestive apparatus.

To ascertain the temperature of the blood in these various vessels we must avoid causing any considerable disturbance in the local circulation, and on the other hand we must not too much uncover the organs, so as not to expose them to any causes of artificial cooling. The process which appears to me to have the fewest sources of inconvenience is as follows :—

The animal being properly supported, an oblique incision is made in the hypocondrium, from the articulation of the last rib of the vertebral column to the external border of the right abdominal muscle. On reaching the abdominal cavity the bowels must be protected and collected on the left side. At the bottom of the wound will be seen the trunk of the *vena cava* and the renal veins which enter it. The right kidney is easily attained, and is found placed in the highest angle of the wound, and as though resting on the articulation of the last rib. In the bottom of this same wound will be found, parallel to the trunk of the inferior vena cava and at the left of it, the ventral aorta, which will be readily recognised by its beating, but the calibre of which is concealed by cellular tissue and by the great number of nervous branches which surround it. At the right and at the top, below the duodenum, the principal mesaraic veins which go towards the trunk of the *vena porta*, which is situated below the liver.

To obtain, by means of this process, the temperature of the arterial blood which enters into the digestive apparatus, this temperature must not be taken in the mesenteric arteries, which is impracticable in

consequence of the smallness of the vessels, but it must be ascertained in the ventral aorta, on a level with the emergence of the celiac trunk and of the mesenteric artery. To obtain the temperature of the blood as it leaves the intestines, it must be taken in the trunk of the vena porta. Finally, to ascertain the temperature of the blood which leaves the liver, we must get into the hepatic veins, penetrating by the inferior *vena cava*.

When we have to deal with large dogs we can reach the various vessels which I have named without much disturbing the circulation. This is the way to proceed; the right kidney which is uncovered is taken, the renal vessels are carefully isolated and surrounded with ligatures to prevent the blood from flowing. The thermometer is passed into the inferior vena cava through an incision made in the renal vein, and is pushed up until it reaches the level of the entrance with the hepatic veins; the thermometer may even be made to enter into these veins by a manœuvre, which consists in inclining the body of the animal at the same moment from right to left.

Then, by means of an incision made in the renal artery, we can reach the ventral aorta very near the emergence of the celiac trunk and the mesenteric artery. There then remains only the trunk of the *vena porta*, into which we easily arrive by the duodenal vein, by means of which we can insert the thermometer into the trunk of the vein, under the liver.

In this first process thus described, the thermometer penetrates, by means of the collateral branching vessels, as far as the trunks in which the bulb of the instrument is surrounded on all sides by a sufficient quantity of blood to be isolated completely from the sides of the vessels. But when the animals (dogs) are small or of middle size, it is impossible to penetrate into the vascular trunks by means of collateral branches as we have just said, and we should be obliged to enter directly into the vena cava, vena porta, and aorta; which necessitates the application of ligatures, which intercept more or less completely, the circulation in those vessels. These ligatures may, however, be so placed, as to have but little influence on the intestinal circulation. For this purpose, I penetrate into the aorta below the emergence of the renal arteries, I then direct the bulb of the thermometer to the level of the celiac trunk and mesenteric artery, to obtain thus the temperature of the arterial blood which is going to be distributed in the digestive apparatus. It will be seen that in this manner, the circulation is only disturbed below the ligature, and that it remains free above, in the vessels which go into the digestive organs. By penetrating in the same manner into the trunk of the inferior *vena cava*, below the renal veins, we only disturb the circulation in the posterior members, but not at all in the hepatic veins. It is more difficult, in this second process, to penetrate into the trunk of the vena porta, without mischief to the intestinal circulation. Still, it can only be slightly modified, by placing the ligature of the vena porta below the junction of the splenic, stomachic and duodenal veins. There is then left, the stomach and a considerable extent of

the superior portion of the small intestine, in which the circulation takes place very well, and brings the blood into the superior part of the trunk of the *vena porta*. Finally, to lessen as far as possible these inevitable inconveniences, we must arrange so as not to take the temperature of the blood of the *vena porta*, until we have taken that of the other vessels.

In the numerous experiments which I have made by these two processes, I have acquired the certainty, that setting aside certain influences that the ligature of these great vascular trunks may have on the absolute heat of the blood, influences to which we shall return, they do not seriously modify the relative temperatures; and what proves this, is, that I have obtained the same relations between the temperatures of the various vessels of the digestive apparatus, by operating alternately by the two processes above described. It is almost useless to add, that in all these experiments, great rapidity is necessary, so as to avoid cooling the organs on which we experiment, and to avoid any modification which the organs of the animal may undergo from the fatigue of the operation. It sometimes happens, that when the animals are too much weakened by the operation, the temperature of the body diminishes; it is necessary to watch for this circumstance, so as only to compare those observations of temperature made in the different vessels at the same moment. It sometimes happens, likewise, that violent agitated movements produce such an effect on the circulation, that they lead to very evident modifications of temperature. The causes of these modifications must be considered further on, but we shall not trouble ourselves with them at present, but only give the results of our experiments, which results have been obtained while the animal was calm and the circulation as normal as possible.

I have used sometimes the mercury metastatic thermometers of M. Walferdin, and sometimes mercury thermometers constructed by M. Fastré. I have always made my comparative experiments with the same thermometer, which was plunged successively into the different vessels. I have taken this precaution because it is always preferable, in comparing results, not to have to allow for the differences which may exist between different instruments. I should, however, observe that in experiments on living animals, this method has not always the same advantages; and it would be better to have, if possible, two precisely similar thermometers which could be plunged at the same instant into the two parts the temperature of which it is wished to compare. Indeed in so mobile a medium as that constituted by the living organism, it might occur that during the time however short, (and it always requires one or two minutes), which separates two successive observations, some passing modification might take place which would perceptibly influence the temperature of the blood, according to whether the animal were calm or agitated, whether it became weaker or suffered, &c. For this reason one observation must never suffice; the same vessels must be examined several times, altering the course of the observation so as to ascertain whether the differential result were accidental.

The bulb of the thermometer which I used generally was from 4 to 5 millimetres in diameter, and from 2 to 3 centimetres in length. This was small enough not to interfere with the circulation in the vessels in which it was placed and its very thin sides gave the thermometer great sensibility. Every centesimal degree of the thermometer was divided into fifths, the half of which, or tenth of a degree, was easily ascertained. The thermometer was always plunged very deeply into such parts as might be cooled by contact with the air. Consequently the hepatic veins, the *vena porta*, the *cœliac* trunk and mesenteric artery, in which I had to observe the temperature are found equally placed against the vertebral column, near the level of the diaphragm, in precisely similar conditions of protection against external cold.

Now having shown as exactly as possible the manner in which these experiments have been made, I am about to give their results, which I have arranged in three tables, which correspond to the three orders of vessels of the digestive apparatus.

The first table gives the results of the comparison of the blood before and after the digestive apparatus, that is in the *ventral aorta* and *hepatic veins*.

The second table gives the comparative temperature of the blood before and after the intestine, namely the *aortic arterial blood* and the blood of the *vena porta*.

The third table gives the results of the comparative examination of the temperature of the blood before and after the liver, that is of the blood of the *vena porta* and of the *hepatic veins*.

To be continued.

PUBLIC HEALTH.

On the EXAMINATION of FLOUR and BREAD. By M. RIVOT.

THE remarkable work of which we are about to give a summary has been undertaken, in consequence of the ministry of agriculture and commerce, having employed M. Rivot to examine a great many specimens of flour and bread for the purpose of ascertaining their quality and relative value.

This eminent chemist has considered chiefly the practical view of the questions which he had to solve, and has commenced by describing the operations by means of which may be most quickly ascertained the quality of a specimen of bread, or the commercial value of a flour intended for the manufacture of bread.

The determination of the quality of the bread which may be made from the various kinds of flour is, in his eyes, the necessary end of all the tests by which this flour can be tried, and the result of the examination of bread can only be conclusive, when we are acquainted with

the flour used for its manufacture, and the conditions of that manufacture.

EXAMINATION OF FLOUR.

The examination of flour requires the following operations:—

- 1st. Determination of the hygrometric water;
- 2nd. Preparation and estimation of the gluten;
- 3rd. The microscopic observation of the flour itself, and of the starch separated in the preparation of the gluten;
- 4th. Estimation of the nitrogen and mineral matters.

Hygrometric Water.—Flour is a very hygrometric substance, and it is difficult to dry it completely.

It may be completely desiccated on a sand bath, but it is then a very delicate operation, and requires great practice in the management of the fire. The best apparatus is a stove in which the temperature can be kept constantly at about 115° or 120° C. (239° to 248° F.) The drying may be considered complete when the flour having been kept for twenty-four hours at a temperature above 110° C. (190° F.) it has not lost weight for several hours.

The proportion of hygrometric water in flour varies accordingly to the nature and source of the cereals, the method of grinding, the separation of the bran, and the preservation of the flour. Fine wheat flour kept for several days in a dry room at a temperature of 20° to 25° C. (68° to 77° F.) will retain only 9 to 10 per cent. of water. What is sold by the bakers in Paris contains from 16 to 17, and sometimes even 18 per cent. of water.

From 15 to 17 per cent. of water may be considered as the average in good wheat flour ground and kept in the ordinary atmospheric conditions.

Gluten.—The preparation of gluten by malaxating the flour in a small stream of water is an excellent method of ascertaining the good or bad state of preservation of the flour; but it must be executed in the same conditions, and if possible by the same operator, to give perfectly comparable results. The quality of the flour for the manufacture of bread is easily ascertained by the rapidity with which the gluten collects, and by its physical characters when separated from the starch. The same circumstances will likewise prove the presence of foreign matters if mixed with wheat flour.

In preparing gluten it is best to act on 100 grammes of flour, and to commence the washing on linen; when the gluten has assumed sufficient consistence, it is removed from the linen, and its purification is completed in the naked hand; it is completed when on squeezing the gluten strongly under a small stream of water, the latter passes through perfectly clear. The matters which escape from the linen or through the fingers may be collected on a fine sieve placed over a large capsule. The sieve collects the bran, the fragments of cellular tissue, the foreign bodies, and finally any portion of gluten which have slipped through the fingers or off the linen; the starch alone passes into the capsule.

The flour is thus divided into three portions, the gluten, the bran mixed with foreign matters, and the starch.

The gluten may be weighed moist, but it is preferable to weigh it after having dried it at a temperature of about 115° to 120° C. (239° to 248° F.) When dry it has a very regular texture in sheets, and it is hard and brittle.

In fine wheat flour the proportion of gluten varies with the source of the corn, from 9 to 11 per cent. of the weight of the flour taken in its ordinary hygrometric state. In flour altered by fermentation the gluten does not begin to collect for half an hour or an hour, when operating on 100 grammes, and passes in part through the linen cloth. When removed from the linen it is very difficult to form it into a solid mass which is much less consistent and elastic than gluten formed from a good flour. When dry it is hard and solid, and generally of a dark color. The careful observation of these characters, in a flour known as good, and in the samples to be examined, will enable us to appreciate distinctly the state of preservation of the flour, which is certainly the most important point for the manufacture of bread.

The weight of the gluten is of little value, as it is nearly the same in good flour, and in that which has become spoiled.

The mixture of other flour with wheat flour has great influence on the aspect of the gluten, and on the manner in which it collects; but it requires a practical eye to distinguish these characteristics.

The incineration of the gluten in general gives no useful indication as to the value of flour; M. Rivot has obtained from various glutens of wheat flour, from 1.25 to 1.45 per cent. of ash, containing more than half their weight of phosphate of lime.

Starch.—The starch carried through the sieve by the water in the preparation of the gluten, is deposited more or less slowly. The large grains gain the bottom of the capsule, the smaller ones remain in suspension in the water which they render milky, and from which they are not separated until after prolonged repose.

Half an hour after the preparation of the gluten, the milky liquor should be decanted off and allowed to deposit slowly. As soon as it has become clear, or after having filtered it, the legumine is sought for by means of acetic acid, which precipitates it of a white color. In the opinion of several chemists this is characteristic of the leguminous vegetables, nevertheless, it does not authorise an absolute conclusion, because acetic acid precipitates other substances besides legumine, such as the soluble nitrogenous matters of certain cereals; it must therefore only be considered as a simple indication of the existence of leguminous flour in the flour examined, and other proofs must be sought for among the characteristics of the flour. In examining the inferior or badly prepared flours, the heaviest part of the starch may sometimes be found covered with a slightly colored layer, in which may be distinguished the fine parts of the bran or the cellular tissue, which have passed through the sieve; this layer is then separated, and is examined through the microscope with the coarser portion which remained upon the sieve.

The starch deposited at the bottom of the capsule has a peculiar satiny aspect when it is produced from the flour of pure wheat of good quality; if on the contrary it proceeds from an altered flour, or from a mixture of good flour with rye, maize, or millet, &c., it is sticky between the fingers, and in each case presents a peculiar aspect.

A small portion of this starch should be left in contact with the air under a thin layer of water to observe the fermentation which will be manifested the more quickly, as the flour had been more spoiled.

The smallest part of the starch should be dried at a moderate temperature, and be kept for microscopic examination. But when there is reason to suspect mixtures, we ought before drying it to dilute it with water, and subject it to several successive agitations and decantations, operating in a large glass with a foot, so as to separate, as far as possible, the different sized grains of starch. In the heaviest part are found grains of the fecula of potatoes and bean flour, in the middle portion the grains of maize.

The deposit formed slowly in the first milky liquor furnished by the preparation of gluten, should contain the smallest grains of wheat and rye, and all those of millet and oats.

Matters Stopped by the Sieve.—These matters are composed of bran, portions of cellular tissue, foreign matters and some small particles of gluten. These last should be separated with care, and united to the mass of gluten, the comparative quantity of the other matters proves the care with which the bran has been separated, and the cleanliness of the flour.

Microscopic Observations.—M. Rivot uses for his observations Chevalier's microscope, with a polarising apparatus, enlarging each grain of flour 300 diameters, he advises examining separately the flour itself, the starch in two, three, and even four sizes, and, finally, the fragments of cellular tissue. This instrument only gives reliable results when observations have been multiplied, and when we have previously familiarised ourselves with the aspect of flours of different species.

M. Rivot can distinguish clearly, with the aid of a microscope, the presence in wheat flour of a very small proportion of maize, oats, millet, beans or potatoes; he has never obtained certain results with rye; the flour of vetches and horse beans present chemical characters in contact with the vapors of nitric acid and ammonia which appear conclusive.

The following is a summary of the process to follow for the analysis of flour.

In a first operation we ascertain the presence or absence of vetches and horse beans in flour, by putting the flour on the inner sides of a small porcelain capsule, and subjecting it successively to the vapors of nitric acid and ammonia, which give a deep red color to all the particles which belong to the vetch and horse bean. The microscopical examination of the reddened matter exhibits which grains are colored, and consequently aids in the estimation of the proportion of vetch

and horse-bean flour, contained in the flour under examination.

In a second series of experiments made with the flour itself, M. Rivot sought to discover with the microscope the down of oats or rye, adherent to each grain of the cereals when put into flour, and they are easily distinguished. The same observations will indicate the form of the remains of cellular tissue, by the fragments of perisperm, the presence of rice, maize, and buckwheat; by treating flour on a piece of glass with a solution of potassa, we can distinguish the square red fragments characteristic of linseed.

In a third series of experiments, the starch obtained by the preparation of the gluten was observed, and it was divided into three portions by successive agitation and decantation.

In the lightest part millet and oats were sought alternately in ordinary light and polarised light. The round grains with a black point in the centre, becoming black with a brilliant centre in polarised light, indicate millet; the opaque grains which disappear almost entirely by polarisation of light, are characteristic of oats.

In the medium sized part of the starch, maize should be looked for first; it is very easily recognised by its round grains with a black point in the centre, becoming square with a rectangular black cross, and offering four very brilliant angles in polarised light.

In the heaviest part of the starch we must seek for fecula, haricots, rice, and buckwheat. Fecula is distinguished by the size of its grains, its peculiar aspect, and the two black hyperbolic branches which polarisation of light cause to appear.

Haricots are distinguished by their elongated grains with two small black points close to the black rectangular cross, with a black patch in the centre; by the circular black contour, and the very brilliant intervals shown by polarised light.

The presence of rice is manifested by the angular fragments of the perisperm, almost transparent in ordinary light. Prismatic agglomerations analogous to the needles of commercial starch show the presence of buckwheat. To these characteristics should be added that of the taste peculiar to each substance, which gives very valuable indications, especially when the palate is practiced in this method of appreciation.

Nitrogen and Mineral Matters.—Flour contains nitrogenous principles, which do not possess like the gluten of wheat, the property of uniting by malaxation, but too much importance must not be attached to their estimation, the good quality of bread depending principally on the absence or small proportion of any other flour than that of wheat, and on the good preservation of the latter. Now these two points are determined by the separation and estimation of the gluten. The gluten is consequently what ought to be examined with the greatest attention. As for the nitrogen, as its estimation does not prove the absence of other flours, nor the good conditions of the wheat flour, it can only be useful to be known and to combine with the others; the question of nitrogen as the principal element in nutrition is too much disputed for us to class, in the opinion of M. Rivot,

flours in accordance with the proportion of nitrogen which they contain.

The ash of wheat is composed chiefly of silica, phosphoric acid, potassa, soda, and lime, it contains very little magnesia, sulphuric acid, and carbonic acid. It can only be examined by incineration in a porcelain capsule, and under a muffle heated only to dull redness, and its fusibility renders this operation very difficult, long, and it might not give an exact result. It need not be executed without there exists a suspicion that mineral salts have been fraudulently introduced into the flour, which is extremely rare.

(To be continued).

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS

PROCESS for the PREPARATION of HYDRIODIC ETHER.

By M. E. SOUBEIRAN.

THE proposition made by M. Huette, to employ iodine under the form of fumigations with hydriodic ether, has not been put to the test by physicians; and among those who have been tempted to try the experiment, some have been deterred by the bad taste and bad odor of the product which has been sold to them. We find, for example, in the excellent work of M. Tison on the therapeutical employment of iodine, that the patients have been unable to bear fumigations with hydriodic ether on account of its bad odor, and when this skilful experimentalist showed me the hydriodic ether which he had bought, I was not surprised at this.

The action which is set up between phosphorus, iodine and alcohol, is so brisk that the operation presents very great difficulties. I now give the means of preparing hydriodic acid easily and without danger.

We select a glass matrass, say of a litre in capacity, having a neck ten centimetres long and very broad; on the other hand, we take a glass tube of 20 to 22 millimetres in diameter, and close it at one end, so as to make a sort of elongated bell, of 24 to 25 millimetres in length; in the closed end we make from eight to ten holes of two millimetres each. At the moment of operation, this tube is plunged in the matrass in such a manner as not to touch the bottom, it is maintained in its position by means of a scooped cork, which will allow free vent to the vapors. It will also be well to prepare another cork, which may be adapted to the orifice of the tube without, however, closing it exactly.

Two hundred and fifty grammes of alcohol of 90° Cartier, and 20 grammes of phosphorus, are put into the matrass; it is gently heated so as to melt the phosphorus; then the tube is adapted to the matrass, so as not to touch the phosphorus, and the addition of iodine to the alcohol is commenced. For the quantity indicated, we should weigh out beforehand 500 grammes of iodine: this is the quantity which should

be transformed. A portion of the iodine is introduced into the tube, which is necessary for forming a layer of four or five centimetres at the bottom of the tube, and we agitate. In proportion as the iodine is dissolved, it runs to the bottom of the alcohol and reacts on the phosphorus. It is agitated to prevent the action from becoming too violent. When the liquor, which has been powerfully colored by the iodine, has lost almost all its color, a fresh quantity of iodine is added to the tube, and we continue to operate thus until all the iodine has been introduced, and disappeared under the influence of the phosphorus.

If the operation is performed rather quickly, the alcohol becomes heated and boils. We should have before us a vessel filled with cold water, into which to plunge the matrass for a few minutes, in order to lower the temperature sufficiently.

The first portions of iodine which are introduced into the tube dissolve very slowly; but as soon as sufficient hydriodic acid is formed to render the liquor very acid, the solution of the iodine is rapidly effected. The liquor then heats very quickly, and it is necessary sometimes to steep the matrass in cold water.

When the operation is terminated, the liquid of the matrass has become very fuming; a very abundant layer of ether is already separated. It is mixed with a powder of a reddish green and some pieces of the same color. This matter is a sub-iodide of phosphorus mixed with phosphorus, of which it is nearly impossible to avoid the formation; it is, however, attended with no inconveniences, for the iodine equally changes the sub-iodide into phosphoric and hydriodic acids. That which is left from one operation is used in the succeeding one, only as the action exerted by iodine on the sub-iodide is much less vivid than that produced by phosphorus, the operation must be commenced with the sub-iodide alone; there will then be less fear of raising the temperature. We should only take the precaution, when the sub-iodide has disappeared, to allow the liquid to cool before adding to it the phosphorus necessary for completing the operation, if we would avoid a reaction so violent as not to be without danger.

The matter left in the matrass is introduced into a glass retort, and it is distilled to remove four-fifths of the liquid. This liquid is washed with water to separate from it the alcohol, and the hydriodic ether is separated by decantation, and this ether is dried by introducing into it a few pieces of chloride of calcium. For medical use, it does not appear to me to be necessary to submit it to a new distillation. We extract a weight of ether, sensibly equal to the weight of the iodine employed; so that in a matrass of the capacity of one litre, we easily prepare 500 grammes of ether.

The transformation of this quantity of iodine may be effected in the space of an hour, or an hour and a half. If required to prepare larger quantities of hydriodic ether, it would be better to proceed less quickly. The mixture of alcohol and phosphorus should be heated so as to fuse the latter, then the iodine should be put into the tube and the operation left to itself, without agitation. Occasionally when the iodine has

disappeared from the tube, it is recharged with a fresh portion. The best way would be to add the iodine in such a manner, as not to allow the liquor to cool sufficiently to solidify the phosphorus; the action would not cease for that, but it would be greatly prolonged.

Journal de Pharmacie, July, 1856.

IODISED GLYCERINE in VARIOUS DISEASES of the SKIN.

By Dr. RICHTER, of Vienna.

THIS solution is prepared by dissolving one part of iodide of potassium in two parts of glycerine, and pouring this liquid on one part of iodine, which it dissolves completely. This solution has the great advantage over alcoholic solutions of not drying; in consequence the surfaces remain supple, and the action and absorption of the iodine continues for a long time. To use the solution it must be spread upon the diseased parts and covered with gutta percha paper to prevent the evaporation of the iodine, and to augment the perspiration of the parts which have been touched. The apparatus is left untouched for twenty-four hours and the degree of the reaction regulates the further applications, as for example, bathing with cold water and fresh application. The iodised solution occasions pain, which varies in intensity and duration, according to the state of the diseased part and the sensitiveness of the individual. There has, however, never been any general inconvenience. On removing the apparatus, the healthy skin has become brown and the diseased portions paler colored than before. On ulcerated surfaces, no trace of iodine will be found two hours after its application. Sometimes the action has been sufficiently powerful to produce *phlyctene*.

The results of Dr. Richter's experiments is, that this tincture acts as a caustic; that it has a really heroic action in cases of lupus, that its efficacy is remarkable in non-vascular gôitre, scrofulous ulcers, constitutional syphilitic ulcers; doubtful in primitive chancres and eczema, and *nil* in psoriasis. The following is one case of lupus cured:—

A man had been troubled with hypertrophic lupus from his childhood; his face was utterly disfigured, ulcerated in several places, and in which two holes represented the eyes, and a circular opening the mouth. The skin of the neck was so much thickened that there was a straight line from the chin to the sternum. To diminish the pain of the application of the iodised solution on so extensive a surface it was divided into two portions, first the neck and lower jaw and after their cure, the rest of the face. Each application occasioned pain for about two hours, and from the very first iodine was found in the urine in large quantities. The hypertrophy diminished by degrees, the ulcers became covered with a very thin epidermis which gradually thickened and then became small flat cicatrices. Fifty-five cauterisa-

tions in the space of three months sufficed to produce a complete and unhopd for cure.

Wiener med. wochenschrift.

PROCESS of PRESERVING PLANTS in their ORDINARY FORM and with the BRILLIANCY of their FLOWERS. By M. M. REVEIL, Professor to the School of Pharmacy of Paris, and BERJOT, Pharmacien at Caen.

It has long been a desideratum to preserve plants in such a way as to retain their natural form and appearance. Processes having this object have long since been described. We will take a rapid view of these processes, before describing that which we propose, and which we have always found successful.

In 1770, M. Quer, a Spaniard, presented to the Academy of Bologna a collection of plants, dried with care and very elegant; but in the process indicated the leaves and branches were detached, and dried separately between leaves of paper in the sun, or in a gently heated oven. The flowers retained their lustre and their form, if the dessiccation was very rapid, and if very little pressure was employed; then the leaves and branches were again gummed on to the principal stem. It is easy to understand that this process must be very long, and that, moreover, it was difficult and even impossible to restore the leaves and branches to their natural position. M. Monty, who described this process in the *Observations sur la Physique et sur l'Histoire Naturelle*, 1772, page 623, ascertained that the temperature of the human body is the most suitable for this dessiccation; it was by this means that he was enabled to preserve tulips, anemones, ranunculus, &c.

The celebrated anatomist, Ruisch, gives in his work entitled *Premier Tresor*, various processes for the preservation of animal matters, some of which may be applied to plants.

But it is especially to Monty that we owe the most interesting researches. We insert them here with so much the more pleasure, as his observations completely agree with our own; we did not know his works when we commenced our experiment; if we had known them some months ago, we should have been spared much trouble and many experiments.

M. Monty endeavored, in 1772, to preserve the plants without subjecting them to the least pressure; several attempts had been made in this direction; but fruitlessly. He first tried dessiccation in millet seed; but he saw that by this means the flowers and leaves were wrinkled, and, moreover, they retained the impression of the millet seeds. He afterwards tried, with no greater success, millet deprived of its pericarp; rice and corn did not give any good result; every vegetable substance was rejected, because it took the moisture from the plants, and the dessiccation was badly and slowly effected.

M. Monty then tried yellow river sand, he was obliged to give it up because the plants retained the sand. He was induced to make use of the white sand, known by the name of freestone (the sand of

Etampes answers very well for this operation). After having sifted this sand in order to separate the coarser particles, he separated the finest particles by levigation, dried the sand and used it for enveloping the plants in boxes, which he afterwards exposed to the sun or to the heat of a baker's oven. The experiment succeeded very well; afterwards, Monty used sea-sand, which gave less satisfactory results.

Ten years ago, M. Stanislas Martin proposed, under the name of *embalment of plants*, a process of preservation, in which he likewise employed dry sand, but without pointing out the precautions to be taken, and without which the operation could not succeed.

Every one noticed at the Universal Exposition, the magnificent flowers prepared by M. Keutz Swarts. We had an idea, at this time, to investigate the means by which these plants had been preserved. One of us possessing an apparatus in which a vacuum may easily be made, we tried dessiccation out of contact with the air and at a low pressure, without obtaining very satisfactory results; we likewise tried the process by ventilation, which dries rapidly and perfectly preserves the color, but which has the serious inconvenience of deforming the corolla and mutilating the leaves; all the organs of plants become extremely friable by dessiccation; thus ventilation had to be abandoned.

We then tried dry sand, and without being aware of M. Monty's experiments, we were led by degrees to introduce into the process the modifications which he had recommended as indispensable; that is to say, the selection of the sand in small equal grains, and washing it to remove the dust; but, in our opinion, these precautions are still insufficient, and after numerous trials, we have stopped at the following process:—

We take white sand in equal grains, which we pass through a hair sieve, it is washed with a large quantity of water in order to remove the finer particles, and the washing is continued until the water goes perfectly limpid. The sand is then dried in the sun or in an oven, or, which is better still, it is raised to about 150° C. (302° F.), being kept constantly agitated in a basin. We then pour in for 25 kilogrammes of sand, a mixture of 20 grammes of stearic acid, and 20 grammes of spermaceti; it is mixed and bruised with the hand so as to cover every grain of sand with some of the grease.

We then put a layer of this sand into a box of any length and width, but of the height of about 12 centimetres (nearly five inches); the bottom of this box is let in by means of a groove, and should be capable of being easily removed. On the bottom is a grating of iron wire of very large mesh. The layer of sand being well established the plants are arranged in it, taking care to open the leaves and to mould the corolla in sand which is added with great care, the plants are covered with sand, and it is best to keep to this unique layer; we take care to put the least possible quantity of sand on the leaves and stems; we cover the box with a sheet of paper, and carry to a drying stove, or an oven heated to from 40° to 45° C. (104° to 113° F.); the dessic-

cation proceeds very rapidly ; when it is supposed to be finished, the bottom of the box is removed ; the sand passes through the iron grating, and the plants remain ; they are brushed with a badger hair-brush, and are kept as we shall presently direct.

The greased sand adheres very slightly to the plants, and it is always easy to remove it ; it is often sufficient to give gentle blows for all the sand to fall, provided, however, that the plants have not been collected while still moist ; we have likewise remarked, that it is better to collect them before the anthesis is completely operated ; it may be finished by steeping the plant by its base into a small quantity of water ; for rather fleshy plants, the vacuum singularly hastens dessiccation.

However, we must add that sand, whether greased or not, cannot be employed for preserving plants which are covered with a viscid varnish, the hyoscyamus, for example, in this case it is absolutely necessary to use millet seed or rice, as Monty did.

We may, indeed, make two layers of plants ; but it is not prudent to exceed the number. The box with a moveable bottom, has rendered great services ; in using an ordinary case, there is great danger of injuring the plants in removing them from the sand.

The brilliancy of the plants is quite retained by this process ; white flowers even preserve their dead whiteness ; it might have been feared that it would have been otherwise, since the white is the interposition of the air ; yellow and blue flowers are preserved very well ; but violet and red colors turn slightly deeper.

The dried plant, left in contact with the air, resumes a little moisture and withers ; to preserve it, it is placed in bottles, at the bottom of which is put some quick lime enclosed in Japan paper and covered with moss ; the bottle is hermetically sealed with a disc of glass, which is caused to adhere by means of a mastic of gum-lac, or caoutchouc.

This process of preserving plants may be of service for drying some flowers or plants used in medicine, such as the violet, the mallow, the stems of melissa, mint, hemlock, &c. The odor is very perfectly preserved and often increased, but it is especially for the preservation of plants intended for the Schools of Pharmacy and Medicine and for the Colleges, that this process may be useful ; it will likewise render great services to horticulturists who desire to preserve rare flowers, as well as to travelling naturalists, who will thus be able to bring home their plants with their natural appearance, which will render determinations more easy.

Journal de Pharmacie, August, 1856.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On a STANNATE of ZINC found in the SLAGS of the COPPER FURNACES.

By THORNTON J. HERAPATH.

IN the ninth volume of the *Chemical Gazette*, p. 125, Mr. J. Cameron has described a peculiar crystalline compound, which was discovered by him in the slags of copper furnaces, at certain stages of the process of copper smelting. This compound occurred in the form of quadrilateral prisms, possessed a vitreous lustre, and was found by Mr. Cameron to have a density of 2.937. It did not undergo any alteration when submitted alone to a white heat, but when reduced to a fine powder, mixed with black flux, and heated to redness on charcoal before the blowpipe, it yielded globules of metallic tin.

The same effect was produced on heating it to redness in a current of hydrogen gas, or in contact with phosphorus, potassium and sodium. It was insoluble in nitric, sulphuric and hydrochloric acids, and also in a boiling solution of caustic alkali. From the results of his analysis, Mr. Cameron was induced to believe that these peculiar crystals were composed of oxide of tin; that is to say, were either an allotropic modification of stannic acid, Sn O_2 , or more probably a higher oxide, Sn O_4 .

Having had occasion to visit the Spitty works, Mr. Cameron showed me a specimen of this curious compound, and kindly presented me with a small quantity, about 10 or 12 grains, to enable me to try some experiments upon it, and subject it to a fresh examination, his engagements having prevented him from submitting it to a complete analysis.

On examining this specimen by the microscope, I found it to consist of a number of colorless four-sided prisms, very similar in appearance to those which were discovered some few years ago, by my father and myself, in a crystalline formation that had occurred in the interior of a zinc retort, at the Patent Zinc Works at Bristol.* These crystals, it will be remembered, were found by us to consist of stannic acid and oxide of zinc, in equivalent proportions. I determined, therefore, to submit the so-called oxide of tin to the same course of

* *Quarterly Journal of the Chemical Society*, vol. i.

analysis as that followed in the case of the compound in question. I accordingly reduced some of the crystals to a fine powder in an agate mortar, and fused it for about half an hour in a silver crucible with a little caustic potash. The alkaline mass resulting from this operation was then extracted with boiling water, supersaturated with nitric acid, and evaporated to dryness. It was then redissolved in hot dilute nitric acid, and the solution was filtered to separate the white insoluble oxide of tin, which was dried and weighed. The filtered solution was then boiled with an excess of carbonate of soda, when, as I had anticipated, a white precipitate of insoluble carbonate of zinc took place. This was collected in the usual manner, heated to redness, and weighed as oxide of zinc.

I. 3.21 grs. of the powdered crystals gave 2.101 grs. of oxide of tin, and 1.09 grs. of oxide of zinc.

II. 5.76 grs. gave 3.761 grs. of oxide of tin, and 2.001 grs. of oxide of zinc.

	I.	II.	Mean.
Oxide of tin (stannic acid)	65.452	65.295	65.373
Oxide of zinc	33.956	34.739	34.347
	99.408	100.034	99.720

Now, the anhydrous proto-stannate of zinc ought to contain, by calculation,—

	In 100 parts.
Stannic acid	65.218
Oxide of zinc	34.782

It is clear, therefore, that the crystals discovered by my friend, Mr. Cameron, are identical in composition with those which were met with by us, associated with crystalline zinc-oxide, in the zinc retorts.

Casa Alta, Coquimbo, Chili.

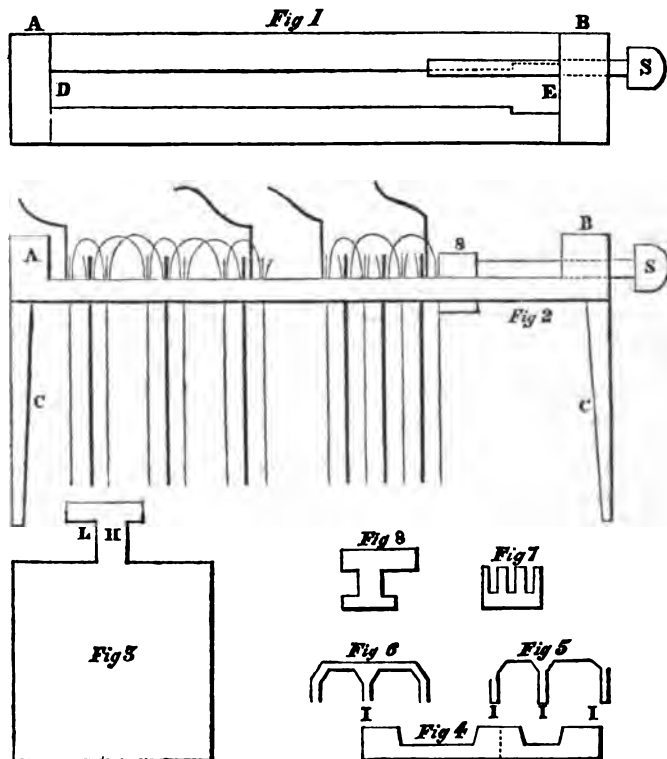
On a New Form of CAST IRON GALVANIC BATTERY.*

By Mr. W. SYMONS

A RECENT paper by the Rev. Prof. Callan, in the *Philosophical Magazine*, on a cast iron battery, which he states to be equal in power to Grove's nitric acid battery, will probably induce many persons to adopt this very cheap, but cumbrous metal. The battery exhibited at the British Association, is an improvement on one which I published in the *Pharmaceutical Journal* for February, 1853, and its recommendations I believe are, compactness, as it can be moved as one mass, and sim-

* Exhibited and described at the Cheltenham Meeting of the British Association.

plicity and economy of construction, as one wooden screw is sufficient for 10 or 12 pairs.



A B, Fig. 1 and 2, is a piece of wood half an inch thick, three or four inches broad, and from one to two feet long with four legs C C, and cross pieces attached to it at A and B; that at B especially, must be stout and very firmly fixed in it, a wooden screw, S, works; a strip seven-eighths of an inch wide is removed the whole length of the board, D E, at E, the opening should be one inch and a quarter wide. The iron and zinc plates are both cast in the shape of Fig. 3, being three-quarters of an inch wide at H. The connections are made by strips of copper Fig. 4 and 5, being three-quarters of an inch wide at I, and a quarter of an inch at the narrow parts; Fig. 4 represents the coppers as cut, and Fig. 5 as bent up for use; I, Fig. 5, fits on the plates at L, Fig. 3. It will, perhaps, be an improvement to have the copper in three separate parts as Fig. 6, although it may appear more complex to have so many pieces, yet they will be found easier to arrange, and will secure more effectual contact; for a common Wedgewood trough, they will be required about three inches and a quarter,

two inches and three-eighths, and two inches long, and of the shape of Fig. 4, divided at the dotted lines.

Fig. 2, shews two arrangements of plates, one adapted for a Wedgewood trough, with cast iron plates on each side of each zinc plate, and the other fitted up in a similar manner to a battery described by Martyn Roberts, in which he proposes to use both sides of both plates; such an arrangement may perhaps be advantageous where the resistance is very small; but as far as a few experiments with the battery shewn, has enabled me to judge it is far from economical, where the resistance is equal to that required for the decomposition of water. I have fitted up six pairs on each plan with similar plates, and with acid of the same strength; but the arrangement in which the pairs were isolated in a Wedgewood trough, liberated three or four times the amount of gases as the arrangement of Martyn Roberts.

To arrange the battery, slide on all the plates in their right order, passing them through the opening at E; in the arrangement for isolating cells, between each pair a piece of wood, Fig. 8, of the required thickness must be placed, then put on the plates at L, the connecting strips, Fig. 5, or 6, placing between each plate a thin piece of gutta percha, three-quarters of an inch square; the poles should be soldered to the end connecting pieces; then slide on a stout piece of hard wood, Fig. 8, accurately fitting the opening D, and screw the whole tightly together; some pieces of gutta percha, Fig. 7, will perhaps be required on the bottoms of the plates to keep them at an uniform distance from each other. The battery will thus be one compact mass, which can be moved readily in and out of the cells; this will probably more than compensate for the loss by the action of the acid on the iron surface not exposed to the zinc, as the battery need only be kept in the acid when actually at work, and this loss may be further prevented by covering the outside of the iron with a resinous mixture; but should the cast iron cells suggested by Callan be preferred, it is evident they can be readily cast with the addition of the piece L H, Fig. 3. As cast iron plates five inches square can be procured for twopence each at any foundry, and no binding screws are required, this battery will probably be found much cheaper and quite as efficient as any yet published, and especially adapted for experimenters who make their own apparatus, who will know how to appreciate the cheap and easy method for making the connections. For a modification of the plan adapted for malleable metals as copper, platinized silver, &c., vide *Pharmaceutical Journal*, January, 1854.

Dunster, 6th October, 1856.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

RESEARCHES on the CEREALS. By M. DUVIVIER.

THE chemists who have analysed the flour of cereals, have never examined the matters which are on the external surface of the grain, or

they have escaped their notice, being confounded with the fatty matters which these flours contain.

Proust has mentioned a yellow resin, as existing in the flour of wheat and barley.

M. Dumas, in the sixth volume of his *Treatise on Chemistry Applied to the Arts*, when enumerating the matters which constitute wheat flour, mentions some butyrous fatty matters, which are separated from gluten by means of ether.

On the other hand, MM. Payen, Poggiale, Millon and Kekulé mention in the analysis of bran, various nitrogenous products, cellulose and, moreover, a variable proportion of flour.

M. Mouries mentions in the internal pellicle of bran, a ferment having the property of converting starch into sugar, under certain circumstances.

However, the envelope of cereals contains peculiar and varied matters, according to the nature of the grain; these matters are only found in the wrinkled surface of the envelope, but they are only found there in very small proportion.

The proportion of these matters may be approximatively stated as

Wheat	.	.	.	3 to 5	per 10,000	without oil.
Rye	.	.	.	4 to 6	„ 10,000	„
Barley	.	.	.	2 to 3	„ 10,000	„
Oats	.	.	.	1 to 2	„ 10,000	„

When we treat wheat or rye with ether, entire, having carefully removed any foreign seeds, and those grains which have been broken or crushed by the flail, or the feet of horses, leaving them in contact with ether for eight hours, then decanting the liquid into a porcelain capsule and leaving it to spontaneous evaporation, we shall find as residue a lemon or orange colored matter, deposited in circular masses round the capsule, at the bottom of which will be found the dust which adhered to the grain covered with this same matter.

In this state and freshly prepared, this matter has an odor resembling that of the corn from which it proceeds: thus, that of wheat has the agreeable aromatic odor of the large masses of wheat in a granary; that of rye has the peculiar odor of that cereal.

Through a lens its aspect is granular, more or less dry, sometimes it is accompanied by another matter, roseate in wheat, reddish in rye, which is deposited below the yellow matter, or communicates to it the orange tinge of which we have spoken.

The abundance of these matters depends on the quality of the wheat and rye; and it is only the superior qualities and certain varieties of wheat and rye, in which the roseate and red matters are found; the influences of climate and atmosphere aid in the formation of this matter when they favor the act of vegetation.

We shall give the generic name of *cerealine* to one of these matters, because it is found in all cereals; the other shall bear the name of *triti-secaline*, because only wheat and rye give it, and not always those.

This crude matter is redissolved in ether or boiling alcohol, to remove the dust, and it is obtained by spontaneous evaporation, if ether is used, and with the addition of a little heat if from alcohol.

The crude cerealine of wheat, collected and put upon paper, does not stain it for a few seconds, and it is only after some time that it is surrounded by a transparent aureola, which extends for a millimetre, or at most a millimetre and a half. By this means we can ascertain whether corn has been fraudulently greased. When the paper is heated the cerealine melts, but does not extend itself as the fatty bodies do; it congeals as soon as removed from the fire, and remains brilliant like a varnish. As soon as it is collected in a mass it loses its yellow color, and when melted it becomes brown.

Cerealine is insoluble in boiling water, which softens it and renders it very tenacious; it is insoluble in cold alcohol, it dissolves in boiling alcohol, and is entirely precipitated on cooling. This is the way to purify it.

The orange colored cerealine of wheat, obtained with ether, treated with boiling alcohol of 86°, dissolves in it with the exception of the rose colored matter; this matter is separated from the liquid, and when the cerealine is deposited the alcohol is filtered or evaporated; we then obtain a small quantity of yellow oil which moistened the cerealine.

The hydrated cerealine collected is opaque, rust colored, and when dry becomes of a brown chamois color. It is rather dry than greasy to the touch; it is unctuous, but not stain paper without heat; when dissolved in alcohol and the latter evaporated to three quarters, the matter becomes when cold, a colorless jelly; if heated in the least this jelly disappears, and the cerealine unites in small brownish masses.

This sudden change of appearance struck us; we sought to ascertain its cause, and we found, much to our surprise, that the cerealine of wheat contains a salt dissolved in an excess of cerealine in small quantity in the corn; this salt is a double cerealate of lime and iron.

Caustic potassa saponifies cerealine, and the salt which it contains enters into the composition of the soap without decomposing. We can, by means of double decomposition, form cerealates with the various bases; these cerealates are always amorphous.

Cerealate of gold spread on a watch-glass and dried has a reddish metallic reflection, whereas on paper, it is a beautiful violet color. The action of caustic potassa on cerealine renders its brown color of a deeper tone; when the potassa is saturated by an acid, the liquor is immediately decolorized, and the cerealine is precipitated in the state of cerealic acid; it is in translucent granules, which become brownish on drying. Iron is found in the precipitating liquor, and the cerealic acid only retains the lime.

Cold nitric acid has no action on cerealine; when hot, it undergoes an alteration which transforms it into a beautiful orange yellow matter, forming little drops which congeal and have the consistence of wax; in consequence of this peculiarity we have given to this substance the name of *triti-cerane*.

Triti-cerane is insoluble in cold alcohol, soluble in boiling alcohol, which it colors of a golden yellow; it partly precipitates on cooling, in yellow flocks. It is obtained by evaporating the alcohol; it preserves its yellow color; it is saponifiable and the solution is limpid, of an orange color. Triti-cerانات may be obtained by double decomposition; triti-cerانات of silver is of a dark grey, that of zinc is of a pale yellow, that of iron is green and afterwards becomes yellow; triti-ceranic acid precipitated by hydrochloric acid is of a brownish color; it still contains iron and lime; when the precipitating liquor was examined iron was found. Thus, notwithstanding the transformation, triti-cerane preserves the iron and lime contained in cerealine.

The pink matter of wheat or triti-secaline, is insoluble in boiling alcohol and in caustic potassa; soluble in ether, and in essence of turpentine; it sometimes remains in the capsule quite colorless; it is only after exposure for some time to the air and light that the pink color is evident.

We have mentioned that the crude cerealine of rye has a lemon or orange color, similar to that of wheat; this appearance might induce us to think that they are identical, but they are not. This matter from rye has, however, some analogy as shown by its behavior with reagents; it at times appears to approximate that of wheat, but it soon shows such marked differences as to prevent the possibility of confounding them chemically.

The cerealine of rye put upon paper does not stain it; when heated it fuses and stains it of a dark brown, but does not spread; when it cools it congeals and remains brilliant like that of wheat. It is insoluble in boiling water, but becomes sticky in it; sparingly soluble in cold alcohol; it dissolves in boiling alcohol, and precipitates partially on cooling. By evaporating the alcohol so as to leave a little at last and letting it cool, the cerealine of rye becomes an almost colorless jelly, sprinkled with brown flocks of sub-cerealate of iron and lime; if, on the contrary, it is evaporated to dryness, the cerealine redissolves the cerealate and becomes brown and brilliant, soft to the touch; sometimes, instead of being brown, the cerealine is greenish; it preserves this color until the end, and is brilliant and green.

Hydrated and dry cerealine of rye is of a beautiful chocolate color; it almost always happens when redissolving it in alcohol that some portions will not dissolve; these portions are sub-cerealate of lime and iron.

The cerealine of rye is richer in double cerealate of lime and iron than that of wheat.

The cerealine of rye dissolves as well as the cerealate, in boiling caustic potassa; the liquor takes a deep brown color. The action of the potassa sometimes causes the appearance of a red matter, which rests on the surface of the liquid; this matter is separated by filtration, is decolorized by acids, and the color restored by alkalis.

Wishing to ascertain the stability of the soap formed, we caused the evaporation of the potassic solution; brown flocks of cerealate separated from the liquor; this, in concentrating, again dissolved them and became very brown and stringy.

We can obtain cerealates by double decomposition. These cerealates contain neither iron nor lime; thus, with silver, we form flocks of a brownish cerealate, which becomes of a dark brown in drying; with gold, a canary yellow cerealate, pulverulent, passing to a brick red. The sub-cerealate of zinc is flesh colored, which color it retains when quite dry.

The cerealates of gold and of silver are reduced by fire, and returned to the metallic state. The sub-cerealate of zinc presents a singular phenomenon; it appears to undergo no alteration when burnt, except the loss of the flesh color; instead of fusing and burning, it hardens and becomes yellow; when removed from the fire, it becomes white; it resumes its yellow color when again exposed to the fire, and *vice versa*; it is fixed.

The potassa soap decomposed by hydrochloric acid loses its reddish brown color, and becomes paler; the cerealic acid is precipitated; it is filtered and washed several times. Cerealic acid collected in the state of hydrate occupies a considerable volume, relative to the small quantity of matter; it is clotted, translucent, of a dark nankeen color; in drying it passes to a mahogany color.

Cerealic acid only dissolves in boiling alcohol; it leaves a portion of the cerealate undissolved; the acid remains in solution. By causing the alcohol to evaporate we obtain the cerealic acid, which always contains cerealates; it scarcely reddens litmus paper; when dried it is unctuous and rather sticky to the touch. When we evaporate the acid water, reagents find iron in it; they do not prove the presence of lime.

Cold nitric acid is without action on the cerealine of rye; when hot, the action of the acid has no result but to divide it; it is reduced into filaments instead of forming little drops, is scarcely altered in the brown color and finishes by becoming carbonised.

The triti-secaline cannot be obtained, like that of wheat, by boiling alcohol which separates the cerealine, for it is dissolved in this vehicle by the influence of the cerealine.

It is by means of a weak solution of caustic potassa that we are able to separate the cerealine of rye from the triti-secaline with which it is mingled.

Some rye of a superior quality, having given a very abundant quantity of an orange yellow matter, this was treated hot with potassa and alcohol; it dissolved, communicating to the liquor a beautiful reddish brown color, due to the triti-secaline which separated from it under the form of furfuraceous scales of a red color. When all was precipitated it was filtered; the triti-secaline was washed several times and dried. Like that of wheat, it is soluble in ether and in the essence of turpentine, insoluble in boiling alcohol, sparingly soluble in boiling caustic potassa.

Some rye of a superior quality gave a yellow matter, which dissolved in alcohol, took a greenish tinge, which it preserved after the complete evaporation of the alcohol. This coloration appears to us due to a peculiar oxidation of the iron. The matter treated by boiling

caustic potassa was dissolved, taking a reddish brown color; the liquid was not limpid, and made no deposit when preserved for three days; filtered after this time, all the matter remained upon the filter; the liquor, almost colorless, only retained a small quantity of cerealate of iron held in solution by the potassa. There was, consequently, in this case no combination with alkali which had been saturated with hydrochloric acid causing effervescence.

Barley, treated in the same manner with ether, gives a whitish, dry matter, of a soft aromatic odor; it does not stain paper when cold; when hot it fuses sparingly, and assumes a greyish tint. The cerealine is purified by dissolving it in boiling alcohol; it is precipitated by cooling; it is filtered and the alcohol evaporated; it gives as a residue a brownish, sticky matter of a resinous nature, containing lime.

The cerealine of barley when dry, is white, opaque and brittle; it is soluble in potassa and contains cerealate of lime.

Oats give a greenish, oily matter, with an aromatic odor which is not so agreeable as that of barley, forming small drops elongated at the base, and disposed round the capsule. This matter put on paper stains it immediately and the oil spreads more and more, leaving, when dry, a little solid matter. The crude matter from oats is treated with boiling alcohol and allowed to cool; a small quantity is precipitated; the alcohol is separated by filtration, the latter is evaporated; a greenish yellow oil is obtained of a disagreeable odor, resembling that of the cruciferae.

The cerealine of oats is of a dirty white color, cracking when dry; it is soluble in caustic potassa, and contains lime.

Vogel had already mentioned a greenish yellow oil in oats. We have not found in the envelope of this cereal the odorous principle resembling vanilla which M. Journef found in oats, by treating it as we have done, directly with ether.

When the cereals are subjected to the action of alcohol, very different results are obtained.

Thus, we cause boiling alcohol to act for some minutes on wheat, and pour the hot alcohol into a glass funnel, the mouth of which is covered with carded cotton; it is then evaporated to dryness. We obtain a matter with an agreeable odor; this matter is taken up again by boiling water which dissolves a yellow substance with the odor of wheat, sapid, attracting moisture and burning, so as to leave very little residue. This matter is united to a small quantity of chloride of calcium. The cerealine which remains in the capsule is redissolved by boiling alcohol, and the liquid is allowed to cool; a small quantity of cerealine separates in flocks; it is filtered and again evaporated; according to the evaporation of the alcohol, greenish fibrous flocks of hydrated cerealate of lime and iron are deposited; in driving off the last portions of alcohol the flocks fuse and remains brilliant. Some traces of yellow oil may be remarked on the edge of the capsule.

The cerealine thus obtained is in small quantity, but it is almost colorless and translucent, and contains but a very small quantity of double cerealate.

Rye gives an almost inodorous matter, sprinkled with dried flocks of double cerealate. When this matter is treated with boiling water the flocks detach themselves, swell, absorbing a little water, become almost white and float upon the liquid: it is easy to examine them separately by throwing on a filter the water which contains them, and which has dissolved a substance analogous to that of wheat, having the odor of rye, and likewise containing chloride of calcium. The cerealine remained in the capsule with a little double cerealate.

Barley gives a matter with a very agreeable aromatic odor; this matter is reabsorbed by water, which dissolves out the same yellowish sapid substance, containing the odorous principle and also chloride of calcium. The cerealine remains insoluble, and containing its double cerealate. Sometimes alcohol dissolves out a green, oily matter, which is deposited in little drops during the evaporation of the alcohol.

The matter of oats has an agreeable odor, in which we perceive the odor of vanilla mentioned by M. Journet. This matter, treated with boiling water, is partially dissolved, as well as a little cerealate, which renders the liquor milky. It is filtered and evaporated to dryness; we obtain a substance which has the odor of vanilla, sapid, analogous to that of the other cereals, and likewise accompanied by chloride of calcium. This cerealate collected on the filter is mixed with that remaining in the capsule with a little cerealine, which itself retains the odorous principle.

This yellowish or brownish matter, which is found in all cereals associated with chloride of calcium, appears to be the chief seat of the odorous principle; this principle impregnating the cerealine of wheat, barley and rye, aromatises it and communicates the odor, such as is found in the fresh matters extracted by ether. We can examine this matter, which is of a nitrogenous nature, by treating cereals directly with boiling alcohol; its taste is sharp and it is deliquescent, in consequence of the chloride of calcium which it contains; it is insoluble in boiling ether, which only separates traces of oil from oats; insoluble in cold alcohol, soluble in hot alcohol and in cold water. It burns, giving a puffy charcoal difficult to incinerate, and giving by reagents chlorine and lime.

By treating cereals with ether, we have seen that this agent dissolves a double cerealate of lime and iron, held in solution in cerealine, that this cerealate dried with cerealine, almost always refuses to dissolve entirely in boiling alcohol, doubtless, because the iron absorbing oxygen from the air, passes to a higher state of oxidation than the natural state. Whence we might infer, that the cerealate is less soluble than cerealine in boiling alcohol, whereas the contrary is the fact.

In treating the cereals directly with boiling alcohol it is remarkable, that it removes more cerealate than cerealine; thus, the cerealine of barley, extracted by ether only gives a very small proportion of cerealate of lime; it is the same with that of oats which gives still less; but when we treat these cereals with alcohol, we obtain very little but double cerealate. Wheat and rye likewise give very little cerealine

with boiling alcohol, which only removes a double bi-cerealate, in which the iron is in the first degree of oxidation.

Finally, we extract from the epiderm is of cereals, with cold water, various products more or less colored, with a nauseous and disagreeable odor, of which some are soluble in boiling alcohol, and some insoluble. It is well known that most of these products are nitrogenous; we merely mention them here to recall them to memory.

We now return to a substance of which we have already spoken, the triti-secaline, which has peculiar characters. When treated with concentrated caustic potassa, it is diminished in volume, leaving a quite insoluble residue; this residue, when dried, was exposed on a plate of platinum to the flame of alcohol; it fused, burnt with a sharp odor, and left as final residue a reddish matter, This matter is oxide of iron.

This experiment would appear to indicate, that the peculiar coloring matter mixed with iron in wheat and rye is different from cerealine, and may probably be an acid which we shall term secalic acid; we, however, give this opinion with great reserve

The various matters extracted from the envelope of cereals, behave in the following manner before the blow-pipe.

These matters fuse at about 58° to 60° C. (136° to 140° F.).

Cerealine from wheat fuses, inflames and burns with a white smoke, leaving a little residue, which finally incinerates. The ashes are formed of lime and iron.

Cerealic acid fuses, inflames and burns with much smoke, leaving a little carbonaceous residue, which when incinerated gives only lime.

Cerealine from rye fuses, becoming of a dark color, inflames and burns with much smoke, leaving a charcoal which is converted into a leafy matter, more abundant than that formed by wheat, and which consists of lime and iron.

Cerealic acid from rye fuses, inflames and burns with much smoke, producing a little carbon which disappears, being converted into carbonic acid, and leaving a residue of lime and iron.

Cerealine from barley and oats, extracted by ether, fuses, burns brightly without smoke, leaving a little residue of lime, whereas that obtained by alcohol fuses, burns with smoke, leaving a little carbon which disappears, and some charcoal which puffs up and which is difficult to reduce, in consequence of the nitrogenous odorous matter which it contains. Lime, iron and chlorine are found in it.

These matters diffuse, when burning, a disagreeable odor, analogous to that of burning fatty matters.

It is to the iron that must be attributed the chamois, chocolate and reddish brown color of the matters extracted from wheat and rye.

It is only by the application of heat that we can find the lime which is so intimately mixed with iron, in the state of double cerealate dissolved in cerealine.

It is not surprising, that cerealine should have escaped the notice of chemists; it is eminently proteic; it possesses properties which differ from any other known substance, and yet which so nearly ap-

proximate to them as to be liable to be confounded with them. It is at once greasy and dry to the touch; it has sometimes the aspect of a varnish, sometimes like a gelatinous substance, sometimes brilliant, sometimes dull and rough; it crushes on pressure and rolls under the fingers like wax, or divides and is reduced to powder. It dries in light flocks, which heat transforms by fusion, into a brilliant network. The cerealine of rye forms with potassa, sometimes a soap, sometimes it is merely held in suspension, and is separated from it with the greatest ease. It is a vegetable chameleon, by turns yellow, brown, chocolate, greenish, red, white, and colorless.

Pure cerealine is colorless and translucent, but we have been able to obtain but a very small quantity in that state.

The external part of the envelope of cereals is, therefore, covered with fatty matters and with odorous nitrogenous matters, having no connection with the covering sometimes seen on leaves and fruits, and which appear quite different from those contained by flour, with which it must not be confounded; and bran must no longer be considered as containing only nitrogenous matters, and variable proportions of cellulose and flour.

But it is of less consequence to know in what state these matters are found on the surface of grain, than to endeavor to ascertain what part they fill in the animal economy, as well as in the vegetable economy of cereals.

In cereals, as used for the principal base of human nourishment, they are almost entirely eliminated with the bran. Animals, on the contrary, absorb them altogether, receiving as nourishment the grain and the bran.

These matters are all assimilable; the elements of the fatty bodies, the lime and the iron, and the nitrogenous matters are destined to nourish at once the fatty parts and the bones of animals, to give normal vitality to their blood, and to develop their muscles in full power.

They form a very tenacious covering to the grain, serving as a preservative to it, at the same time that it gives a peculiar odor, *sui generis*. Wheat owes its market value to them; they heighten its color, give it the brilliancy and unctuousness to the touch known to the trade, and thus form the pivot of transactions and induce purchasers; in this point of view these matters are of the greatest interest.

This is so true, that wheat when too long kept loses this aspect, which is so preferred; it becomes dull and harsh to the touch by the dessiccation of these matters; it is then less esteemed and is considered inferior.

Fine Wheat.—This contains orange colored cerealine, cerealate of lime and iron, triti-secaline (secalate of iron?), a yellow oil, a nitrogenous matter with the odor of wheat, chloride of calcium, a nitrogenous matter soluble in water, and a nitrogenous matter soluble in alcohol.

Ordinary Wheat.—The same as above, except that the cerealine is yellow, and it contains no triti-secaline.

Barley.—Contains white cerealine, cerealate of lime, cerealate of lime and iron, a green oil, a brown resin, a nitrogenous matter with a sweet odor, chloride of calcium, a nitrogenous matter soluble in water, a nitrogenous matter soluble in alcohol.

Fine Rye.—Orange colored cerealine, cerealate of lime and iron, triti-secaline (secalate of iron?), nitrogenous matter with the odor of rye, chloride of calcium, a nitrogenous matter soluble in water, and a nitrogenous matter soluble in alcohol.

Inferior Rye.—The cerealine is yellow and it contains no triti-secaline; in all other respects it is similar to the above.

Oats.—Dirty white cerealine, cerealate of lime, double cerealate of lime and iron, greenish yellow oil, a nitrogenous matter with an odor of vanilla, a nitrogenous matter soluble in water, and a nitrogenous matter soluble in alcohol.

It is evident, that there are numerous investigations still necessary with respect to flour, so as to know whether flours should be sold sifted as at present, or whether they should not rather be sold so as to contain the whole product of the corn.

Journal de Chemie Médicale, October, 1856.

RESEARCHES on ESSENCE of TURPENTINE; NEW ACID OBTAINED by the OXIDATION of HYDRATE of TURPENTINE.

By M. PERSONNE.

THE acids hitherto obtained by the oxydation of essence of turpentine, have been produced by the action of nitric acid of various degrees of concentration. These are:—Terebic or terebilic acid $C^{14} H^9 O^7$, HO of MM. Bromeis and Rabourdin; pyroterebilic acid derived from terebilic acid; finally, terephthalic $C^{16} H^4 O^6$, 2 HO, terebenzic acid $C^{14} H^6 O^8$ HO, and terechysic $C^6 H^3 O^4$, HO of M. A. Caillot.

On comparing the formulæ of these different acids with that of essence of turpentine, it is difficult to comprehend the reaction which has produced them. This is because the oxidising action of nitric acid is very complex, and the production of these various acids is always accompanied by that of several bodies produced simultaneously or by secondary actions. The employment of an oxidising body whose elements are very stable, has led me to hope for a more simple and more distinct reaction; potassa, and still better potassa or soda lime, appeared to offer the best chances of success. It was by passing the vapor of hydrate of turpentine $C^{20} H^{16}$, 4 HO, on soda lime, heated to about 400° C. (752° F.), then treating the product with hydrochloric acid, that I obtained and isolated the acid which I am about to describe.

This acid is solid, white, with a slight odor, heavier than water; it fuses at 90° C. (194° F.), and distils at 250° C. (482° F.) During the distillation a small quantity appears to be decomposed; it is almost insoluble in cold water, more soluble in boiling water, from which it is deposited, on cooling, under the form of a white powder,

composed of small crystalline needles; very soluble in alcohol and ether. By sublimation it crystallises in small plates, which appear to be oblique prisms. Its vapor is very acrid and irritates the nostrils extremely.

The composition of this acid is represented by $C^{16}H^{10}O^4$; this formula has been determined by the analyses of the salts of lime and of silver, of the crystallised acid as well as of the distilled acid.

The analysis of the lime salt gave . $Ca\ O = 17.68$ per cent.

The analysis of the silver salt gave . $\left\{ \begin{array}{l} C = 88.92 = H\ 3.70 \\ Ag = 43.90 \end{array} \right.$

Calculation gives for lime 17.83, and for the silver salt

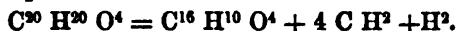
$$C = 89.18, H = 3.67, Ag = 44.08$$

and for the formula $C^{16}H^9O^3$, HO.

Lime forms, in combining with this acid, a white salt, crystallising in small silky needles, which give it the appearance of sulphate of quinine; it possesses a similar lightness. The silver salt is sparingly soluble in boiling water from which it crystallises in small quantity on cooling. Oxide of lead forms with it an uncrystallisable salt which possesses, when dry, the aspect of pieces of gum arabic. This acid etherifies alcohol with the greatest ease, and produces an ether similar to the odorous ethers produced by the volatile fatty acids; its odor is similar to that of pears and pine apples.

Terebinthilic acid $C^{16}H^{10}O^4$ is placed by its composition, between caprylic acid $C^{16}H^{16}O^4$ and toluic acid $C^{16}H^8O^4$, from which it only differs in the amount of hydrogen.

The production of this terebinthilic acid by the action of soda lime on hydrate of turpentine, is accompanied by an abundant disengagement of gas, which I recognised as a mixture of proto-carburetted hydrogen and free hydrogen; this formation may be represented by the following equation:—



Besides this gaseous mixture, there is formed a small quantity of liquid hydrate of turpentine (*terpinol*), but this product is only accidental. This formation of gaseous products occasions a fear of explosions when, for the sake of speed and to prevent waste, a closed tube is used, as M. Delalande does in making campholic acid; it is better to use a tube open at one end, and consequently to operate at the ordinary pressure.

Comptes Rendus, No. 10, September 8th, 1856.

DETERMINATION of the AMOUNT of THEINE in GUARANA.

By JOHN STENHOUSE, LL.D. F.R.S.

GUARANA, as is well known, is a species of chocolate, manufactured from the fruit of the *Paullinia Sorbilis*, by the aboriginal tribes of Para, and other districts in Brazil, and extensively employed by them,

both as a refreshing beverage, and as a remedy against dysentery and similar diseases.

The presence of theine in guarana was first discovered many years ago by Dr. Theodore Martius, of Erlangen, and his observations were subsequently corroborated by Messrs. Berthellot, Dechastelus, and C. Jobst.

As none of these gentlemen, however, made any determination of the amount of theine in guarana, I thought it might be of some interest to supply the omission, especially as I had previously determined the quantity of theine present in all the other substances known to contain that alkaloid. Through the kindness of my friend John Miers, Esq., F.R.S., the well-known Brazilian traveller, I have recently been enabled to subject guarana to a new examination.

The specimen of guarana, given me by Mr. Miers, was about seven and a-half inches long, one and a-quarter inches thick, and weighed nearly three-quarters of a pound. It had a rough corrugated surface, dark brown color, no odor, and a somewhat amygdaloidal fracture.

Twenty-five grammes of the specimen were very finely powdered, and boiled with one quart of distilled water for some time. When cold, a slight excess of basic acetate of lead was added, when a bulky brownish-red precipitate fell. This was repeatedly digested with hot water, and the clear liquid which passed through the filter was treated with a slight excess of sulphuretted hydrogen, till all the lead present in it was precipitated. The clear solution was then evaporated to dryness on the water-bath, and the residue dissolved in a small quantity of boiling alcohol and filtered. The filtrate was allowed to evaporate nearly to dryness, when yellowish-colored crystals were deposited. These were pressed between folds of bibulous paper, and recrystallised out of spirits of wine, when they were rendered perfectly colorless. After being dried on the water-bath, they were weighed, when the twenty-five grammes gave 1.260 grammes, equal to 5.04 per cent.

A second determination, in which 14 grammes were used, gave 0.715 grammes, equal to 5.1 per cent.

Average of the two determinations, 5.07 per cent.

In addition to theine, guarana contains a coloring matter, apparently analogous to the tannin in cinchona bark, and likewise a fatty matter which, like the fat of chocolate, does not appear to become rancid by keeping.

From an inspection of the subjoined table, it will be seen that guarana is the richest known source of theine :—

	Per cent. of Theine.
Guarana	5.07
Good Black Tea	2.13
Black Tea from Kemaon, E.I.	1.97
Various samples of Coffee Beans, from 0.8 to	1.00
Dried Coffee Leaves, from Sumatra	1.26
Paraguay Tea, from <i>Ilex Paraguayensis</i>	1.2

Pharmaceutical Journal.

ANALYTICAL CHEMISTRY.

REDUCTION OF SILVER by GRAPE SUGAR. By M. WICKE.

THE alloy is dissolved in nitric acid; it is evaporated to drive off the excess of acid; it is diluted with water, and an excess of carbonate of soda is added hot. The precipitates of the two metallic oxides are then boiled with a solution of grape sugar. The silver is reduced. The copper is then extracted by means of ammonia, and the silver remains pure in the metallic state.

This process is analogous to that of M. Levöl.

Annalen der Chemie und Pharmacie.

PROCESS for the DETECTION of very small QUANTITIES of COPPER and LEAD. By M. LOWENTHAL.

THE metallic solutions are evaporated to dryness; the residue is then dissolved on a sand bath, in 10 or 15 grammes of concentrated sulphuric acid. It is allowed to cool; the solution is poured into a graduated tube, and from five to eight drops of hydrochloric acid are added. If copper and lead are present, the liquid becomes turbid and whitish; but if copper exists in any large quantity the color of the turbid liquid will not be white, but of a yellowish brown. According to the author, we can thus discover a hundred thousandth of lead and a twenty five millionth of copper.

Journal de Chimie Pratique.

INDUSTRIAL CHEMISTRY.

NOTE on a GREEN DYE from CHINA. By Mr. DANIEL HANBURY.

SOME weeks since a merchant showed me a sample of a new dye, said to have been imported into Marseilles from China, and to be recommended for dyeing silk. From its dark blue color, I conjectured it might be a sort of indigo, such as the Chinese are stated to prepare from *Isatis tinctoria* L. and from *Polygonum tinctorium*, Lour., but I had no opportunity of determining whether it was indigo, by any chemical examination.

Very recently, however, a larger sample of the same dye, and a piece of cotton cloth dyed green with it, have been sent to me by my friend William Lockhart, Esq., of Shanghai. They were accompanied by a communication as to the manufacture of the dye upon the authority of the Rev. J Edkins, the substance of which is given below.

I may add, that Mr. Lockhart forwarded to me in the early part of this year, stems of the cultivated tree called *Lith-chae*, being one of

those from whose bark the dye is manufactured, but no specimens from which its botanical characters could be ascertained.

The dye, which is known as a *green* dye, and called *Lük-kaou*, consists of a dry paste, in small irregular fragments scarcely thicker than stout paper. Its color is an intense blue-black, with a little of the coppery lustre seen upon indigo. Rubbed upon paper with a moistened ivory knife, it develops a greenish blue of considerable brilliancy. It is insoluble in water, rectified spirit, or ether, whether cold or hot. With fixed alkalis, however, a solution of an intense green color is obtained. A solution prepared by boiling 10 parts of the dye with 3 parts of carbonate of potash (KO,CO^2) has a brilliant bluish-green and possesses the following characters. The addition of an acid occasions no precipitate. Solution of hypochlorite of soda (*Liquor sodæ chlorinata*, P. L.), very sparingly added, changes the green to blue, which speedily passes to purple and then to pink; a large addition of the reagent destroying the color completely. Solution of iodine produces the same changes from green to pink. Sulphurous acid alters the green to a yellowish-brown. Hydrochloric acid occasions no change in a solution of the dye; nitric acid renders it brown.

This brief and superficial examination is almost all that my small stock of the dye has permitted, but upon the arrival of a larger quantity, I trust a complete investigation will be made.

The following is the substance of the information before alluded to, as obtained by the Rev. J. Edkins.

The bark of two kinds of the tree called *Lüh-chae* literally *Green-shrub*, one kind growing wild which is known as the *white*, and another, which is cultivated; and called the *yellow*, are used to obtain the dye.

The *white* or wild *Lüh-chae* grows abundantly in the neighborhood of Kea-hing and Ningpoo. The *yellow* is produced at Tsoh-kow-pang, where about thirty men are employed in the manufacture of dye. This place is two or three miles west of Wang-teen, a town a few miles to the south of Kea-hing. The dye is also prepared in Hooman and at Ningpo, but not so good.

The process is as follows — The two barks are placed together in iron pans, and thoroughly boiled in water. The decoction is left undisturbed for three days, after which it is placed in large earthenware vessels, and cotton cloth prepared with lime is dyed with it several times. After five or six immersions, the coloring matter is washed from the cloth with water, and placed in iron pans to be again boiled. The coloring matter is taken up on cotton yarn [by dipping] several times in succession; it is then washed off and sprinkled on thin paper, when half dry, the paper is pasted on light screens and thoroughly exposed to the sun. The product is called *Lük-kau*. In dyeing cotton cloth with it, 10 parts are mixed with 3 parts of subcarbonate of potash in boiling water. It is not used to dye silk on account of the expence, since it is only a rough surface that takes it easily, and to color silk, so much of the material must be used, that it would not pay. All cotton fabrics, also grass-cloths, take the color readily. The dye does not fade with washing, which gives it a superiority over

other greens. It has long been used by painters in water-colors, but the application of it to dye cloth was first made only twenty years ago. If some method could be discovered of applying it to silk fabrics it would become still more useful.

The dye is sent from Koa-hing as far as Shantung.

Pharmaceutical Journal.

NEW PROCESS for the MANUFACTURE of STEEL.

FOR the satisfaction of a number of scientific gentlemen, engineers, and others interested in the subject, an exhibition of the new method of manufacturing steel, the discovery of Capt. Uchatius, (Engineer in Chief of the Imperial Arsenal at Vienna), was made at Messrs. Rennie and Sons, the Albion Engine Works, Holland-street, Blackfriars-road, in the presence of numerous spectators.

Mr. Charles Lenz, the partner of Capt. Uchatius, operated on the occasion, assisted by workmen from the factory of Messrs. Turton and Co. at Sheffield. Whilst the process was going on, Mr. Lenz read a paper descriptive of the invention. He commenced by explaining that he had labored under many disadvantages in being compelled to contrive substitutes for the regular furnaces and other proper appliances peculiar to steel works, but, nevertheless, an opinion could be there formed of the merits of this important invention, for all the melting operations in cast steel manufacture were necessarily a series of operations on a small scale, the size of a steel crucible limiting the magnitude of the furnaces. He proceeded to state that the method adopted in England, and all over the world, he might say, for making the best descriptions of cast steel heretofore, was to convert Swedish or Russian bar iron, by a lengthy, uncertain, and costly process, first into what is called blister steel, which product was then melted down in crucibles and cast into ingots for the manufacture of the bar steel of commerce. The invention about to be exhibited by him would render this country quite independent of Sweden and Russia for steel iron-making, as he would show the company present that East India pig iron, now very plentiful and cheap here, could be converted into fine steel in as few hours as Swedish and Russian bar-iron would take weeks to manipulate; in addition to which, he could assure those present that numerous descriptions of ordinary English pig iron would answer for this purpose equally well, if he might judge from the limited experiments in English iron he had performed. Indeed, the results of these experiments were very remarkable, and he expected that nearly all the iron-works would soon make steel as regularly as they now made iron. He begged the company to consider the importance of a discovery which would so reduce the cost of steel as to render it available for numerous purposes in engineering, now quite precluded by the price; he did not hesitate to assert that fully two-thirds might be saved in the cost of producing cast steel by using the present instead of the old process.

Mr. Lenz then proceeded to explain that the invention of Capt.

Uchatius was founded upon the well-known fact that cast iron surrounded by any oxygenised materials, and subjected to a cementing heat for a given time, would yield up a portion of its carbon, which would combine with the oxygen driven off from the surrounding materials forming carbonic oxide or carbonic acid gas. If this process were interrupted before completion, a partially decarbonised iron would result, the surface of which would have been converted into a pure iron, while the inner parts remained unchanged; or, in other words, the progress of decarbonising action would depend on the amount of metallic surface brought into contact with the oxygen yielding material with which the iron was surrounded. In order, therefore, to expedite this operation, the pig iron was first reduced to a granulated state, which was accomplished by simply running the molten iron from the cupola (a blast furnace in some cases) into cold water, agitated by mechanical means. This granulated iron was mixed with a proper proportion of pulverised oxygen yielding materials of a very cheap description, such as sparry iron ore (spathose ore) and adding, if requisite, a small quantity of manganese, which mixture was put into common crucibles and subjected to heat in a cast-steel blast furnace of ordinary construction. By thus subjecting the granules of iron, in presence of the sparry iron ore, to a melting heat, the surrounding oxides would first effect a partial decarbonisation of the granulated iron, which decarbonisation would be limited in amount according to the size of the granules operated upon, and by reason of the continued application of heat the iron would melt and separate (with the assistance of the melting residue of sparry iron ore), from the impurities with which it was mixed, and also bring down with it a portion of the iron contained in the sparry iron ore, thereby increasing the yield of cast-steel by 6 per cent. The manipulations of melting and casting were the same as those commonly employed by cast-steel manufacturers. The quality of the steel made by this process could be considerably modified. Thus, the finer the pig iron was granulated, the softer would be the steel made therefrom. The softer sorts of welding cast-steel might be obtained by an addition of good wrought iron in small pieces, and the harder qualities by adding charcoal in various proportions to the before mentioned mixture. Thus, continued Mr. Lenz, might crude iron be converted into steel ingots in the incredibly short space of about two hours.

Mr. Lenz then proceeded to exhibit the preliminary process of granulating, by running a crucible of melted pig iron into a vessel of water, when it was instantaneously converted into shot-like particles. A weight of 24 lbs. of the granulated iron was mixed with 6 lbs. of crushed ore and peroxide of manganese, in the proportion, it was said, of about 4 lbs. of ore and 2 lbs. of peroxide of manganese, to which was added a small quantity of fire-clay, and filled into the crucible in the temporary furnace, and allowed to melt in the usual manner. In the mean time the company proceeded to witness the operation of the hammering down into a bar of an ingot of this new steel, which had been made a few days since, and although the steam-hammer

used was not at all adapted for steel, nevertheless, the bar steel produced from the ingot then hammered was pronounced to be of excellent quality, and tools made by Messrs. Rennie from a fellow ingot were tried and found to possess the qualities of fine English cast steel. After two hours and three quarters had elapsed since the filling—some defect in the blast a little retarding the melting—the contents of the crucible were poured into the iron mould, from which, when opened, an ingot of steel, weighing 25 lbs., being 1 lb. more than the iron used, was exhibited to the company, which bore every external evidence of being perfect in quality. It was to be forwarded to the steel works of Messrs. Spencer and Sons, of Newcastle-on-Tyne, to be properly tilted into bar steel for further tests as to its quality and properties. The simplicity and rapidity of the new process, as well as the quality of the specimens of steel shown, elicited much admiration. The importance of the process in reducing the cost of steel can scarcely be overrated when the innumerable new uses to which it would inevitably be turned in preference to iron are considered—the expense of steel tyres, axles, piston rods, shafts, and other important working parts of machinery being estimated as not exceeding the price now paid for first-class iron.

Journal of the Society of Arts.

AGRICULTURAL CHEMISTRY.

On the PRODUCTION of CARBONIC ACID GAS by the SOIL, ORGANIC MATTERS and MANURES.

By M. B. CORENWINDER.

Since the time of Saussure we have known that organised substances, when moistened and exposed to the air, extract oxygen and partly absorb it, so that there is a production of water with their hydrogen, and of carbonic acid at the expense of their carbon.

This eminent observer has likewise remarked that damp leaves, earth, sawdust, &c., produce, by their decomposition in the air, a certain quantity of carbonic acid.

I some years ago proposed to repeat these observations, and I prepared an apparatus to ascertain the quantity of carbonic acid produced by organic matters when exposed to the action of atmospheric air.

The results which I obtained appeared to me to be in some respects interesting, and I therefore determined to publish them.

The apparatus which I used consisted of—

1. A Liebig's bulb, *a*, containing a concentrated solution of potassa; this potassa was intended to retain the carbonic acid of the atmosphere.
2. A test glass, *b*, containing baryta water, to ascertain whether all the carbonic acid of the atmosphere was taken up by the potassa in *a*.

3. A large bell glass, *c*, under which I placed the matters to be examined.

4. A test glass, *d*, containing baryta water, destined to absorb the carbonic acid produced in the bell glass *c*.

5. A second test glass, *e*, intended to ascertain whether all the carbonic acid produced was absorbed by *d*.

6. A large aspirator of about 50 litres capacity, intended to pass pure air through the whole apparatus at a regular and constant speed.

It is unnecessary to enter further into the mounting and arrangement of this apparatus; it is sufficient to say that every precaution was taken to avoid errors.

Experiments on Soils.

First Experiment.—On the 12th June, 1851, I placed under the bell glass of my apparatus, a flower-pot into which I had put garden earth. This earth was rather argillaceous, and had not been manured for a long time. Its composition was as follows:—

Water	.	.	.	.	.	.	31.90
Organic matters	.	.	.	.	.	.	13.70
Mineral matters	.	.	.	.	.	.	54.40
							100.00

The upper surface of the earth experimented on was about 50 square centimetres, and the depth of the layer about 8 centimetres.

Before putting the baryta-water into test glass *d*, I worked the aspirator for some time, so as to remove all the carbonic acid from the bell glass, and from the earth in the pot.

I then put a clear solution of baryta-water into the test glass, and I arranged the stopcock of the aspirator so as to have a constant flow amounting to 80 litres in the twenty-four hours.

After a certain time the baryta liquid turned white, became turbid, and at the end of twenty-four hours, when I closed the experiment, a considerable deposit of carbonate of baryta was in the receiving test glass. I threw it rapidly on a weighed filter, washed it with boiling water, and dried it with care. I thus obtained 0.22 grs. of carbonate of baryta, which represents about 0.05 of carbonic acid, or 25 cubic centimetres.

From this we may consider that a hectare of land of eight centimetres in thickness furnishes, in twenty-four hours, about 500 hectolitres of carbonic acid.

Second Experiment.—On the 1st July, 1851, I placed under the bell glass a pot of the same size as before, containing still more argillaceous earth. The deposit of carbonate of baryta in twenty-four hours was 0.13, which represents 14 6-10ths cubic centimetres of carbonic acid, or about 300 hectolitres per hectare.

Third Experiment.—The next day I moved the earth with a knife, so as to imitate ploughing, and to lay open fresh surfaces. I

obtained in twenty-four hours 0·17 gra. of carbonate of baryta, representing 19 3-10ths cubic centimetres of carbonic acid, or about 400 hectolitres per hectare.

It is right to add that, on continuing these experiments for several days there was but little variation in the quantity of carbonate of baryta, so that we may conclude that the carbonic acid obtained depended entirely on the alteration of the air in the organic matters contained in the earth subjected to examination.

Fourth Experiment.—I have quite recently made a fresh experiment on cultivated earth, taken from the surface of a field of beet-root following a crop of wheat.

To operate with accuracy, I took from several parts of the field a large quantity of earth which was properly mixed; I then put some into a flower-pot, and heaped it in lightly.

The surface of the earth experimented on was 63 6-10ths square centimetres, and its depth about 8 or 9 centimetres. Its composition was as follows:—

Water	.	.	.	.	.	.	12·50
Organic matters	.	.	.	.	.	.	4·80
Argil, &c.	.	.	.	.	.	.	82·70
							100·00

The field from which it was taken had not been limed for seven years. It had received farm-yard manure before the winter, and colza cakes in March, in the proportion of 3,300 kilos per hectare. The earth was argillaceous, contained no carbonate of lime, but had been well ploughed.

The cake spread on it and harrowed in was in an advanced state of decomposition, and covered with mouldiness.

The pot having been placed under the bell glass, I worked the aspirator for some time without collecting the gas.

The experiment lasted four days.

The first day I obtained in twelve hours—

A deposit of carbonate of baryta	.	.	.	.	gr.	0·505
The second day	.	.	.	.	.	0·470
The third day	.	.	.	.	.	0·450
The fourth day	.	.	.	.	.	0·400
Average in twelve hours	.	.	.	.	.	0·450
„ in twenty-four	.	.	.	.	.	0·912

During the first three days the temperature was from 20° to 25° C. (68° to 77° F.). The last day it rose to 30° to 35° C. (86° to 95° F.).

It will be observed that the carbonate of baryta decreased regularly each day. This circumstance must be attributed to the diminution of moisture of the matter experimented on, and perhaps to other causes which are difficult of appreciation.

As for the temperature, a very careful examination has proved that it does not exert any perceptible influence on this phenomenon in

these small limits; or rather, its influence is probably neutralised in part by the desiccation of the earth and of the organic matter in decomposition.

However this may be, we may conclude from the numbers obtained, that in twenty-four hours the earth in the pot gave 0.202 gr. of carbonic acid, or about 100 cubic centimetres, equivalent to 15.70 litres per square metre, or 1,570 hectolitres per hectare.

It will be observed that, in the first experiment the earth contained much more organic matter than in the fourth, and that, notwithstanding, the last gave much more carbonic acid. It results that the production of this elastic fluid, in these circumstances, depends on the case with which the fertilising agent decomposes. In the first case, the earth contained humus which decomposes slowly; in the second, it was fertilised by oil-cake, whose effects are much more active, but less durable.

Without attaching too much importance to these figures given by analysis, they show, nevertheless, how considerable is the quantity of carbonic acid produced in a well cultivated and well manured field, and prove, it appears to me, in an indubitable manner, that the mass of this elastic fluid exhaled by the earth must be assimilated by the leaves of vegetables.

We could not understand otherwise why the quantity of carbonic acid found in the air should be so trifling.

We shall find by the following experiments that manure in a state of putrefaction gives unexpected volumes of carbonic acid.

It must have been remarked that in the third experiment I obtained a more abundant disengagement of carbonic gas, on moving the earth so as to renew the surfaces, and thus imitating ploughing and harrowing. It is evident that by this operation we expose to the action of the oxygen of the air fresh molecules of organic matter, hitherto preserved from it. This practice has long been general in England.

"I never obtained better turnips," says Lord Leicester, "than when I had the earth turned up between the rows." I have myself found the good effects thus produced.

The effect of ploughing, &c., has hitherto been supposed to be only the destruction of weeds, the lightening the soil, so that the air could reach the roots, and the disaggregation of the argil, which sets at liberty the alkalis necessary for vegetation. I certainly do not deny that these effects are produced, and that they are very necessary, but I likewise think that the value of these mechanical operations is increased by the production of an abundance of carbonic acid which they occasion in cultivated earth.

Experiments on Manures.

Fifth Experiment.—On 1st August, 1851, I placed under the bell glass of my apparatus a pot-full of fresh cow-dung.

The operation continued for twenty-four hours. The weight of carbonate of baryta obtained was 0.545, which represents 62 cubic centimetres of carbonic acid from a surface of 50 cubic centimetres,

and a depth of 8 centimetres, or 12 4-10th litres per square metre, or 1,240 hectolitres for one hectare, supposing all the conditions the same.

The next day the weight of carbonate of baryta obtained was 0.525, a nearly identical result.

Finally, having kept cow-dung for four days, and subjected it to the same experiment, I obtained in twenty-four hours 1.120 grs. of carbonate of baryta (temperature about 25° C., 68° F.), which is equivalent to 126 cubic centimetres of carbonic acid, the surface of the pot being 63 6-10th square centimetres, and the depth of manure about 8 centimetres.

From this we may calculate that a layer of cow-dung in the same state of decomposition, 1 square metre in surface and 8 centimetres in thickness, would give in twenty-four hours 19.8 litres, or, in round numbers, 20 litres of carbonic acid, and consequently that one hectare would produce 2,000 hectolitres.

Sixth Experiment.—I then operated on farm-yard manure, and obtained in twenty-four hours 0.525 of carbonate of baryta, equivalent to about 1,200 hectolitres of carbonic acid on a surface of one hectare supposed to be in the same conditions of solidity, temperature, and chemical composition as in my experiment.

Seventh Experiment.—On the 10th August, at a temperature of 20° C. (68° F.), I made some experiments with fresh horse-dung; I obtained on the first day a quantity of carbonic acid equivalent to about 5 litres for a layer of 1 square metre of superficies, and 8 centimetres in depth, representing 500 hectolitres to the hectare. This fresh manure consequently gave but a moderate quantity. Not doubting that the production of this gas would become more considerable in proportion to the fermentation of the manure, I left it for four days under a bell glass. It then began to heat and to diffuse an unpleasant odor; I then commenced examining it.

After some hours the production of carbonic acid became so abundant that the baryta in the receiver was saturated. I could only continue my experiment for six hours, and I obtained 0.975 of carbonate of baryta, representing 440 cubic centimetres of carbonic acid in twenty-four hours from a surface of 50 square centimetres. This quantity is equivalent to 88 litres per square metre, or the enormous proportion of 8,800 hectolitres per hectare.

Experiments on various Organic Matters.

To complete these researches I made experiments with a certain number of organic matters of different species, such as cotton, flax, unbleached and spun, guano, sugar, and even wood charcoal.

Neither dry and pure sugar, nor moist wood charcoal produce carbonic acid. Guano steeped in water only gives a small quantity, which is explained by its chemical composition and the nature of its elements.

Eighth Experiment.—Unbleached spun flax steeped in water, and lightly wrung out, produced a considerable quantity of carbonic acid. A skein of 50 grammes put under the bell glass of the apparatus

gave the first day, in twenty-four hours, the temperature being from 25° to 30° C. (77° to 86° F.), 4 grs. of carbonate of baryta, representing about 9 litres of carbonic acid for 1 kilogramme of this thread.

After remaining several days under the bell glass, this same skein of flax gave me, in twenty-four hours, a deposit of carbonate of baryta weighing 4.272 grs., equivalent for 1 kilogramme to 11 litres of carbonic acid.

According to the above experiments we see that the quantity of carbonic acid furnished to vegetables by the fermentation of the organic matters at the surface of the earth, is more considerable than has hitherto been supposed. It is very generally admitted that the carbonic acid necessary for vegetation is due entirely to animal respiration, and a ponderation has been established between the animal and vegetable kingdoms which is perhaps too absolute, if we consider the large quantity of carbon annually fixed in the mass of vegetables which cover cultivated soils, and the comparatively very small quantity of carbonic acid given by animal respiration.

My experiments appear to prove that, if we take into consideration the carbonic acid produced by animal respiration, and the accidental combustion of fires and volcanoes, it would be more in conformity with the nature of things to attribute to the production of carbonic acid from the surface of the earth, the greater portion of that employed in the alimentation of vegetables.

It is, therefore, rational to suppose that vegetables are placed on the surface of the earth, amid incessantly renewing carbonic acid, which is more abundant as the temperature is higher and the ground more moist, circumstances that aid the decomposition of manures. The careful and accurate analyses by M. Boussegault of the air confined in the soil, justify this view, and I have only endeavored to confirm his views by my experiments.

Annales de Chimie et de Physique, October, 1856.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

On the Existence of an Organic Base in URINARY DEPOSITS.

By Dr. WILLIAM FRAZER.

LICENTATE OF THE KING AND QUEEN'S COLLEGE OF PHYSICIANS, AND ROYAL COLLEGE OF SURGEONS, IRELAND; LECTURER ON BOTANY AND NATURAL HISTORY TO THE CARMICHAEL SCHOOL OF MEDICINE.

I AM desirous of directing some degree of attention to the existence of an organic basis or substructure, to be found in calculous concretions and in urinary sediments generally, as from a number of observations I am led to think that it constitutes an important element in their composition, and one the existence of which has been almost entirely overlooked hitherto. The fact of there being sometimes found in the human body a so-named fibrinous calculus is well known,

but with this rare exception, in which the entire mass is of organic origin, and therefore different from what I now describe, I may say that the almost universal presence of organic or animal matter in these concretions is lost sight of; nor, indeed, could the ordinary modes of examination pursued in the analysis of calculi easily lead to its detection, and I was therefore obliged to employ a different mode of research for it, and to have recourse to the valuable aid of the microscope in this matter.

If we dissolve small fragments of calculus concretions, whether urinary or renal, or if we use for the same purpose some common sedimentary deposit, of course employing its suitable solvent,—in the case of the phosphatic sediments an acid, and for lithic acid, an alkaline solution, say of potash; and for this purpose very small portions indeed of the stone will answer,—and examine the resulting solution under the microscope there is no difficulty in detecting the organic residue I speak of; in some instances it forms an unusually large amount of the stone, in others it is in less quantity but by proceeding in this manner I have never failed to satisfy myself of its presence, which I believe to be invariably the rule.

I think this organic matter acts as the basis or substructure of the concretion; that it then operates, to use an illustration, very much in the manner in which a string or thread disposes the occurrence of crystallisation or deposit of sugar in making sugar candy, and that it thus gives origin to most of our crystalline sediments, and to all our culculous concretions. As an additional proof of the view thus put forward, I would instance the very common microscopic observation of a hair or thread which has accidentally fallen into some urine which is about depositing—say lithic acid—and in which we constantly see the hair or thread, after lying for some hours in the fluid, becoming encrusted over with the well developed crystals of the salt. Or I might illustrate it by the beautiful and interesting experiment of Dr. Garrod, for detecting the presence of lithic acid in the serum of the blood in gouty patients, by immersing a very fine thread in the serum, and after some hours the crystals are deposited in sufficient quantity on the thread to be recognised by the aid of a microscope.

Perhaps I may make my views plainer by stating, that I do not think urinary sediments to be mere deposits caused by a diminution in the temperature of the fluid from which they are crystallised, nor to be of the nature of chemical precipitates formed by the addition of two dissimilar substances, and a resulting insoluble product being thus evolved; but that they are slowly crystallised on an animal basis or substructure, probably in many instances epithelial, from solutions which in other cases would appear to hold them dissolved for an indefinite time; and I believe that this view of the subject will explain the not uncommon fact of a calculous concretion existing and increasing in a patient who still has no urinary sediment that can be detected on examination of the urine.

Dublin Hospital Gazette.

EXPERIMENTAL RESEARCHES ON ANIMAL HEAT.—By M. CLAUDE BERNARD.
(Continued from page 54.)

TABLE I. Temperature of the blood compared in the vena porta and hepatic veins. (Before and after the digestive apparatus.)					TABLE II. Temperature of the blood compared in the vena porta and vena porta. (Before and after the intestine.)					TABLE III. Temperature of the blood compared in the vena porta and hepatic veins. (Before and after the liver.)				
Dog	Vena Porta (Before digestive apparatus.)	Hepatic Vena (After.)	Differ-ences.	Observations.	Dog	Aorta (Before intestine.)	Vena Porta (After.)	Differ-ences.	Observations.	Dog	Vena Porta (Before liver.)	Hepatic Vena (After.)	Differ-ences.	Observations.
1	40.3	40.6	+0.3	Fasting.	1	39.6	39.8	+0.2	Digesting.	1	40.2	40.6	+0.4	Fasting.
2	40.3	40.9	+0.6	Digesting.	2	40.3	40.7	+0.4	"	2	41.3	41.5	+0.2	"
3	39.4	39.6	+0.2	Fasting 2 days.	3	39.4	39.5	-0.1	Digesting.	3	40.6	40.9	+0.3	Digesting.
4	39.6	39.9	+0.3	Digesting.	4	39.5	39.7	+0.2	"	4	40.7	40.9	-0.2	Fasting.
5	39.4	39.6	+0.2	Digesting, weakened.	5	40.9	40.6	-0.3	"	5	39.4	39.6	-0.2	"
6	38.6	38.9	+0.3	Digesting.	6	40.3	40.2	-0.1	Fasting, had drunk.	6	39.8	39.9	+0.1	Digesting, weakened.
7	41.0	41.6	+0.6	Dig. end, vigorous.	7	38.6	38.6	0.0	Digesting.	7	39.5	39.6	+0.1	Fasting.
8	40.0	40.2	+0.2	Digestion beginning.	8	39.9	39.5	-0.4	Dig. commenced.	8	38.6	38.9	+0.3	Digesting, weakened.
9	39.5	40.0	+0.5	"	9	39.4	39.3	-0.1	Fasting, 2 days.	9	39.5	40.2	+0.7	Digesting.
10	40.0	40.7	+0.7	Fasting, vigorous.	10	40.0	40.2	+0.2	"	10	39.6	39.8	+0.2	"
11	41.0	41.6	+0.6	Digesting, vigorous.	11	39.0	39.4	+0.4	"	11	40.2	40.7	+0.5	Fasting.
12	39.0	39.6	+0.6	Fasting, alcohol.	12	37.6	38.0	+0.4	Digestion finished.	12	39.7	40.3	+0.6	Digesting, vigorous.
13	37.6	38.4	+0.8	Fast, 4 days, ether	13	38.7	39.2	+0.5	"	13	38.4	39.6	+1.2	Fasting 4 days.
14	38.7	39.6	+0.9	Digesting, vigorous.	14	37.5	37.8	+0.3	"	14	37.8	38.4	+0.6	"
15	38.4	39.4	+1.0	Small.	15	38.7	38.8	+0.1	Fasting.	15	39.8	39.8	0.0	Digesting, febrile.
16	37.5	38.2	+0.7	Weakened.						16	39.6	39.7	+0.1	Digesting.
17	38.1	38.7	+0.6	Weakened.						17	38.0	38.2	+0.2	After death art. res.
18	38.7	38.9	+0.2	Fasting, vigorous.						18	37.8	38.2	+0.4	"
										19	38.8	38.9	+0.1	Fasting.

By inspecting these three tables, we find that there is a constant increase of the temperature of the blood, during its transit through the digestive apparatus, and at the same time we may see the part which the intestine and the liver play in this increase.

It would be illusory, and even erroneous to draw averages from the numbers mentioned in these tables, because the animals which have been examined, were frequently in different conditions, and not comparable to each other. The only operations which should be compared, are those made on the different vessels of the same animal, and these are the comparisons from which we should deduce our consequences.

We see by the results contained in the first table, *that the blood when coming from the digestive apparatus, after having passed through the two sanguineous capillary systems, that of the intestine, and that of the liver, is always warmer than when it enters.*

The state of abstinence, or digestion which at first appeared so important to compare, do not appear to occasion any great modifications in the constant result which we have just mentioned, and indeed the increase of heat is frequently found to be greater in animals when fasting.

The second table gives the changes of temperature of the blood, in passing through only the capillary system of the intestine. We see in a general manner, that the temperature increases. Still there are some observations in which there is apparent an equality, and even a lowering of the temperature.

These peculiarities are easily understood from the fact that the mucous membrane of the intestine, is in reality an exterior surface, on which may be found, in consequence of alimentary ingestion, some substances from without, and which may occasion a cooling, which is necessarily communicated to the blood which circulates through it.

The third table shows the modifications of temperature which the blood undergoes, in traversing only the hepatic capillary tissue. There is here a constant increase in the temperature of the sanguineous fluid, and on examining the numbers of the differences, we see that this increase is relatively very great.

To sum up, the experiments whose results are contained in this work prove:—

1. That the digestive apparatus causes the sanguineous fluid to become constantly hotter, so that in this apparatus the venous blood is warmer than the arterial.

2. The blood which passes out of the digestive apparatus by the hepatic veins, is a constant source of calorification to the blood which passes to the heart by the *vena cava inferior*. We may, henceforth, say that it is the principal source; for in no part of the system is the blood so hot as in the hepatic veins, and our tables of experiments show that in the most vigorous animals this temperature may reach $41^{\circ} 6$ C. ($106^{\circ} 9$ F.)

3. Among the organs which aid in heating the blood in the digestive apparatus, the liver occupies the first rank. Whence it results

that this organ should be considered as one of the principal furnaces of animal heat.

On the Modification which the Temperature of the Blood undergoes in traversing the Pulmonary Apparatus.

I have said above that I was the first person who paid much attention to the modification of temperature which the blood undergoes in traversing the digestive apparatus, but this is not the case with respect to the respiratory. As far back as we have any knowledge of, philosophers, have endeavored to ascertain whether the lungs cooled or heated the blood, but it is especially since the celebrated theory of Lavoisier on the respiration of animals, that this subject has been the object of very numerous experimental researches.

The ancients considered that the lungs cooled the blood. But the genius of Lavoisier, embracing at one view, the part played by oxygen in inorganic and living beings, regarded the lungs, which absorb oxygen and emit carbonic acid, as an organ in which a combustion is operated by the direct fixation of the oxygen of the air on the carbonated elements of the blood. This combustion being necessarily accompanied by the disengagement of heat, it resulted that the lungs were the principal furnace of animal heat, and that the blood which passes out from the respiratory apparatus into the left cavities of the heart would be hotter than the blood which is sent into the pulmonary organs from the right cavities of the heart.

Experiments on animals appeared at first to confirm the ideas of this illustrious chemist, Saissy, Davy, Nasse, &c. supported the theory of Lavoisier, by concluding from their experiments, that the blood was hotter in the left than in the right heart. Saissy saw that the blood was warmer in the left ventricle by 0.5° C. Davy obtained about the same difference, and Nasse found 1° C. of heat more in the blood of the left ventricle, than in that of the right. Other experiments, those of Crawford, Krimmer, Scudamore, Davy, Becquerel, and Breschet, still confirmed the same theory, by ascertaining that the blood is hotter in the arteries than in the veins.

But if the theory of Lavoisier has always continued accurate with regard to the general phenomena which constitute this important function, still the works of Lagrange, Edwards, and Magnus, have have led to a different interpretation of the use of the lungs. These authors have shown that instead of a direct combustion in the lungs, there was in these organs only a simple change between the oxygen of the air, and the carbonic acid of the blood.

The respiratory act being from that time a general phenomenon taking place in all the capillaries of the body, there was no longer a theoretical necessity for admitting that the pulmonary apparatus was the furnace in which the blood was warmed again in passing from the state of venous into that of arterial blood. Still the habit continued of admitting that the blood should be hotter in the left ventricle than in the right. It is true that if experiments on animals had unanimously proved that it was so, we ought to admit the result, even if it were

opposed to theory. But this was not the case, and there was reason for making fresh experiments. Opposed to the above experimenters who had found the blood hotter in the left side than in the right, were others who stated they had found the blood hotter in the right ventricle than in the left. Autenrieth found that the blood was half a degree warmer in the right ventricle. Berger obtained exactly the same result. Collard de Martigny and Malgaigne mentioned a difference in the same direction. Afterwards Herring, profiting by the exceptional case of a calf which continued alive with ectopy of the heart, plunged the thermometer directly into the cavities of the heart, thus exposed naturally in a mammifera. Herring found in this remarkable experiment, that the blood in the right ventricle was 1.53° C. hotter than in the left ventricle. More recently M. George Liebig has published an excellent work on the temperature of the blood in which he arrives at the same conclusion, that the blood in the right cavities of the heart have a higher temperature than that in the left cavities.

When we thus find in physiology contradictory results on the same subject, we may at once affirm that all the experiments have not been tried in precisely similar conditions, or that some cause of error has existed in some which has not existed in the others. It will, therefore, be necessary in this case to ascertain what causes have led observers to erroneous results.

One of the principal causes of error, which have led to the opinion that the blood of the left heart is hotter than that of the right heart, proceeds from these experiments having been made on animals which were just killed, and the chests of which have been opened to expose the heart. In these circumstances, the circulation is disturbed or completely stopped, and the heart cools in the air. But the cooling of the two ventricles is unequal, and is much more considerable with respect to what is in the right ventricle, than for that in the left. This difference in the cooling proceeds from the different thickness of the substance of the ventricles of the heart; for, as is well known, the substance of the right ventricle is two or three times thinner than that of the left. It will, therefore, be evident that if a thermometer be placed in the two ventricles of a heart in these conditions, an excess of temperature will be found for the left ventricle, merely because it cools more slowly than the right.

This inequality in loss of heat by a liquid contained in the two ventricles of the heart, which may easily be imagined *a priori*, may be proved by a very simple experiment. Extract the heart of a dead animal, and place in the ventricles two thermometers, as nearly comparable as possible. Fill the cavities of the heart with hot water, and close the opening of the large vessels by means of ligatures which will serve to fix the thermometers, and at the same time for suspending the heart. Thus prepared, steep the heart containing the two thermometers in a bath of hot water, and leave it in it until all parts of the heart have obtained the same temperature, which may be ascertained by both thermometers marking the same degree of heat; the heart is then removed from the water, and by means of the string used as

a ligature it is suspended to cool in contact with the air. Very soon, the two thermometers which were of one accord in the bath, disagree completely in the air. For example, the two thermometers marking 35° C. (95° F.) when taken out of the water the ambient temperature being about 10° or 12° C. (50° or 53° F.) five minutes after the exposure of the heart to the air, the thermometer of the left heart will mark 34.9°, and that of the right heart only 33.8° proving that the latter ventricle cools much more quickly than the first.

M. George Liebig has likewise mentioned this cause of error, and has shown its importance by direct experiment. On the other hand, by comparing the details of the contradictory experiments of the authors which we have compared above, we see that it is chiefly from defective experiments of this nature, that these varying results have arisen.

I shall not dwell longer on these causes of error, which merely require to be mentioned. I will merely mention, that the only manner of avoiding them is to operate on the living animal in such a manner that the heart shall not be exposed, and that the blood shall be constantly renewed in its cavities. All the experiments which I am about to describe, have been made in these physiological conditions.

Experiments made to ascertain the Temperature of the Blood in the Cavities of the Heart, before and after the Respiratory Apparatus.

To obtain the temperature of the blood, before and after the respiratory apparatus, it is naturally indicated to take the temperature of the blood in the right and left ventricles of the heart. It would be impossible in the living animal to ascertain the temperature of the blood in the pulmonary artery, which takes this liquid to the lungs, and in the pulmonary veins which take it back after traversing these organs. But the blood of the right heart is in reality the blood which is about to enter the lungs, as the blood of the left ventricle represents that which has just passed through the respiratory apparatus.

My first experiments were made on dogs in the following manner. The animal being properly placed on its back, I made an incision at the lower part of the right side of the neck, and I successively isolated the right jugular vein, and the carotid artery of the same side, as low as was possible without entering the chest. I tied these two vessels, and placed a string under them; then through an incision in both artery and vein, I introduced the thermometer into the right and into the left ventricle, placing on the vessel and round the thermometer a moderately tight ligature, only intended to impede the entrance of air into the right heart, and the flow of blood from the left. We know that we have entered the ventricle, when the thermometer will descend no further, and when it is agitated by the contractions of the heart. The right ventricle is easily entered through the jugular vein and the auricle. For the left ventricle it is necessary in dogs, to penetrate through the carotid, then by the brachio-cephalic trunk, and thus reach the end of the aorta to penetrate directly by it into the left ventricle. It sometimes happens that we thus tear the sigmoid valves

of the aorta, and this accident may produce some perturbations in the circulation, to which we shall return when we proceed to study the modifications produced in the temperature of the blood by perturbations of the circulation. At present we shall speak only of results obtained in calm animals, and when the circulation retained as far as possible its normal conditions. I must, likewise, add that it is necessary to operate on large dogs, if we wish to penetrate into the left ventricle with a thermometer of the dimensions of that described in our first communication. When the dogs are small, it is necessary to have very small thermometers, which are so fragile that the least movement of the animal will break them. These remarks do not apply to the right ventricle, which is always easily penetrated. In my experiments I have always used one thermometer, which I introduced first into one ventricle, and then into the other, alternating the experiments several times to verify the same facts. I obtained the following results with dogs.

	Right Heart. (Before the Lungs.)	Left Heart. (After the Lungs.)	Difference.	Observations.
	°C.	°C.		
1st Dog.	38.2	38.0	-0.2	Dig. com. weak.
2nd "	39.5	39.3	-0.2	Fasting.
	39.2	39.1	-0.1	
3rd "	38.8	38.6	-0.2	Fasting, atmos.
	38.7	38.5	-0.2	28° C. (82° F.)
4th "	38.8	38.6	-0.2	Fasting, vigorous.
	39.2	39.1	-0.1	Next day, digesting.
5th "	38.9	38.7	-0.2	Fasting, vigorous.
6th "	38.9	38.8	-0.1	Digesting, vigorous.
	39.4	39.2	-0.2	Next day, dig. fin.

According to these results, we see that the blood in the right ventricle, when about to pass into the lungs, has at all times a higher temperature than the blood of the left ventricle which has passed through the lungs. The difference of temperature never exceeded two tenths of a degree, but it was always evident. According to these experiments, the state of digestion or abstinence appears to have a distinct influence on the temperature of the blood in the heart. When the animal is fasting, the differences of temperature are ordinarily more considerable, and they become slighter during the progress of digestion. But at the same time, the entire temperature of the blood appears to rise. This is evident when we compare the experiments. But I have succeeded in proving this very distinctly in the same animal. In the fourth dog, the temperature when fasting was 38° 8' in the right heart; 38° 6' in the left heart, the difference consequently was two tenths. The next day, the same animal being in full digestion, the temperature being 39° 2' in the right ventricle, and 39° 1' in the left, there was only one tenth difference. In this case, therefore, we see that the difference of temperature had diminished, but the absolute temperature had increased. I will remark in

conclusion, that my experiments were made on very healthy animals, since not only did they not die under the operation, but they survived well, preserved their appetites, and could thus be observed several times in various conditions.

Still I would not trust these experiments only; and as these results were of the greatest importance with regard to the theory of animal heat, I determined to prove them on other animals, taking every imaginable precaution to avoid error. I therefore requested M. Walferdin, whose competence on thermometric questions is well known, and whose instruments are so very accurate, to aid me in the determination of the temperature of living animals.

We made experiments at the slaughter-house, at Grenelle, on sheep, and obtained results which agreed perfectly with those obtained from dogs. The temperatures were taken with the mercury metastatic thermometer of M. Walferdin, divided into a certain number of parts, each representing the hundredth of a degree, which may thus be read directly on the instrument.

The process of operating is peculiarly easy with the sheep. This is because the neck of the sheep is very long, consequently we can go very near the origin of the vessels of the heart, without opening the chest. I make a longitudinal incision at the middle of the lower part of the neck, which should be prolonged to the sternum. The sterno-hyoidian and sterno-thyroidian muscle of the right side, are moved aside, and immediately underneath is the right carotid artery at the head of the brachio-cephalic trunk. The jugular vein is found forwarder. Sometimes the vessels may be a little concealed. The thermometer was always introduced into the left heart by the brachio-cephalic trunk, and into the right by the jugular vein, following the precautions which we have already mentioned. The observations were made with the same thermometer which was introduced successively into the right and left side, always verifying each result several times. These results are from calm animals, whose circulation was regular.

Dates.	Right Heart.	Left Heart.	Difference.	Observations.
	°C.	°C.		
June 9, 1853.	40·374	40·122	—0·252	Atmos. 24° C.
16, "	40·032	39·924	—0·108	" 22° C.
" "	39·600	39·582	—0·012	" "
" "	40·392	40·248	—0·144	" "
" "	39·870	39·582	—0·288	" "
March 9, 1854.	40·481	40·392	—0·089	" 14° C.
" "	40·481	40·392	—0·089	" "
" "	40·392	40·212	—0·180	" "
" "	39·942	39·852	—0·092	" "

These experiments on sheep consequently lead to the same conclusions as those on dogs, namely, that the blood of the right ventricle is hotter than that in the left ventricle.

We shall find that the excess of temperature is brought to the right ventricle, by the *vena cava inferior*, and not by the *vena cava superior*, in which it is cooler; but we shall at present merely state that the mixture of all the venous blood of the body, which is made in the right ventricle, is, when about to pass into the lungs through the pulmonary artery, hotter than when it passes out from those organs, and arrives in the state of arterial blood in the left ventricle. Whence we are forced to admit that the blood is cooled by its contact with the air at the surface of the lungs, which in this respect may be looked upon as an external surface.

To sum up, from all the experiments contained in this work, which have been made on living animals, and I think with all the accuracy of which such experiments are at present susceptible, I think that we may legitimately conclude.

1st. That the circulation of the blood through the pulmonary apparatus, causes this liquid to cool.

2nd. We cannot, therefore, any longer consider the lungs as a source of animal heat.

3rd. That the transformation of the venous blood into arterial blood in the living animal, does not coincide with an augmentation of heat in this liquid, but on the contrary, with a diminution of its temperature.

In another communication I shall proceed to examine the modification of heat, to which the blood is subjected in traversing the genito-urinary apparatus.

Comptes Rendus, Nos. 7 and 11, Aug. 18th, and Sep. 15th, 1856.

On PHYSIOLOGICAL GLUCOSURIA in LYING-IN WOMEN, NURSES, and some PREGNANT WOMEN. By M. BLOT.

THE presence of sugar in the urine, has hitherto been considered as the sign of a most serious malady, namely, *diabetes*. The numerous investigations which I have made, have given me results which must in future remove a portion of the diagnostic value of this symptom. In fact my researches show that sugar exists normally in the urine of all lying-in women, of all nurses, and of many pregnant women.

In my investigations, I have been assisted by M. Reveil, and sometimes by M. Berthelot, whose labors in organic chemistry are so well known.

To demonstrate this fact, I have had recourse to all the general means for ascertaining the presence of sugar in a liquid; and I have thus proved in the most indubitable manner, that the urines in question present these four properties, belonging only to sugars, 1st, of reducing the cupreopotassic liquor; 2nd, they render brown the caustic alkaline solutions of potassa and lime; 3rd, they give by fermentation, alcohol and carbonic acid; 4th, finally, they deviate polarised light to the right.

Let us now show in what conditions this glucosuria is present. In all lying-in women without exception, from the moment that the

secretion of milk commences, sugar exists in the urine in sufficient quantity to be estimated. With many women it only appears at that time, with some it is found before, but generally in small quantity. If the lacteal secretion continues, the sugar continues to pass into the urine with daily variations in quantity which are at present inexplicable. When the secretion of milk is very abundant, the proportion of sugar is usually great; if it is only slight, the urine is but slightly saccharine. Consequently an examination of urine might aid, up to a certain point, in deciding the value of a nurse. If the secretion of milk is diminished or injured in any way, and especially by the development of any morbid state, the sugar diminishes or completely disappears; if the disease is removed and the secretion of milk is re-established, the sugar re-appears. In fact, the urine contains sugar, as long as the lacteal secretion continues. I have found very notable proportions (8 grammes in 100 grammes of urine) in a nurse who had suckled for twenty-two months. I need scarcely add that none of these women, whether pregnant, lying-in, or nursing, had any symptom of diabetes. On the contrary, their urine was all the richer in sugar in proportion as their health was better, and as it the more nearly approached the normal or physiological state.

When lactation ceases, the sugar disappears from the urine in a short time, varying in different subjects; more quickly in women who do not suckle, more slowly in those who after having suckled begin weaning. Among these latter the disappearance of the sugar offers many peculiarities; sometimes I have found some one day, there has been none the next, and again I have found it on the third day. But it always occurs that the sugar disappears almost entirely, the moment that the tumefaction of the breast, consequent on weaning, ceases.

From these observations it appears to me impossible not to admit that this physiological glucosuria has a relation to the lacteal secretion.

I have observed that the quantity of sugar varies in different subjects, and at different epochs of lactation; I must likewise mention that it is generally much less than in diabetes. The quantity which I found varied from 1 to 2 grammes up to 12 grammes in 1000 grammes of urine. I will only observe in this respect, that these quantities were found in the morning urine, which is probably the least rich.

In pregnant women sugar was found in half the subjects examined. I think, but am not quite positive, that this peculiarity is found when the sympathetic phenomena of pregnancy in the breast are much developed; and is wanting when the breasts remain, so to speak, indifferent to what is passing in the uterus.

This glucosuria being ascertained in women, it was natural to suppose that, in the same conditions, it would exist in female animals of the different species of mammifera, I am pursuing my investigations in this direction, and hope soon to arrive at certain results. I am even now in a condition to state that this phenomenon is found in the cow; for in nine observations on those animals I have never failed to find it.

TOXICOLOGY.

POISONING by HELOELLA ESCULENTA.

THE family of a forester had eaten for supper a large quantity of morils, collected by the father. They had been boiled in water, and fried in fat. In the night, the father, mother, and six children, were seized with all the symptoms of poisoning by poisonous champignons. Despite all the efforts of two physicians, a boy of three and two girls of five and six years of age died on the second day, and the rest of the family were saved only after very severe symptoms. The physicians ascertained that no other cause of poisoning existed; the pan was well tinned, the morils were fresh, and well known to the father, and the family had frequently consumed them under the same circumstances. The physicians were shown the place where they were gathered; it was a sandy place in which no other kind of champignon existed; moreover, the form was so well characterised, that error was impossible, and this heloella is eaten in large quantity throughout the country. There could be no doubt as to this champignon having caused the poisoning. However, all authors regard it as innocent. Must we not conclude, therefore, that under certain influences yet unknown, eatable champignons may become poisonous, and that those which are poisonous may lose their poisonous principle? This might explain the differences between authors and consumers, as to some champignons, which the former regards as eatable, and the latter as dangerous.

Deutsche Klinik.

PUBLIC HEALTH.

On the CHEMICAL COMPOSITION and NUTRITIVE EQUIVALENTS *of the*
FOOD of MAN. By M. POGGIALE.

THESE investigations form the first part of a work which I have undertaken with the view of determining the chemical composition and the nutritive equivalents of the principal aliments of man. I have assisted, during some years, in the labors of various commissions charged to study the composition, the nutritive value, and the substitutes for aliments; I have consequently made very numerous analyses, and have tried to ascertain by experiment the solution of these questions, which are of the highest importance to the human race.

During the last twenty years many very important memoirs have been published as to the use of the plastic aliments and respiratory aliments, on the formation of fat and of sugar, on the proportion of nitrogen contained in the alimentary substances, and on the connection existing between the aliments and the transmutations of the organism. It results from these great works, which we owe chiefly to MM. Dumas, Liebig, Boussingault, Payen, Persoz, Bernard, &c., that the alimentary substances of the human race are divided into two great classes,—nitrogenous aliments and non-nitrogenous aliments;

that the first nourish our organs, and that the others are burned in the system, being transformed into water and carbonic acid, and thus producing animal heat. It likewise appears from the researches of modern chemists and physiologists that neither man nor animals can be nourished by food which does not contain nitrogen; that all vegetables contain nitrogenous principles; that their nutritive power is generally in proportion to the nitrogenous matter contained in them; and that again being subordinate to the proportions of albuminoid substances, fatty matters, hydrates of carbon, and salts which they contain.

But to know, even approximately, the nutritive value of alimentary substances, it is indispensable to have exact ideas of the elements which form them. Unfortunately, we do not yet know the composition of the principal aliments, and we have consequently no exact data for the establishment of a scale of nutrition. On the other hand, physiologists have singularly exaggerated the importance of the nitrogenous substances, by seeking in the proportion of these principles the measure of the nutritive value of aliments. In fact, if observation has proved that matters devoid of nitrogen are unfit to support life, we likewise know that nitrogenous aliments alone will not suffice for nutrition, and that they require the aid of respiratory aliments and mineral salts.

The estimation of the fatty matters in starch, sugar, and similar substances, not having been made with the degree of precision which such questions deserve, chemists such as M. Liebig, in arranging comparative tables, have been obliged to neglect the fatty matters, or to represent them by a certain quantity of an amylaceous substance. Thus, 24 parts of starch would be equivalent to 10 parts of fat. But, supposing that these figures show exactly the relation between these principles with respect to the amount of heat which they produced, it is certain that none of them fulfil the same functions in the system, and the slightest observation will demonstrate that fat cannot be entirely replaced by these amylaceous and saccharine matters. It is necessary that the carbon which enters the system should do so in the form of fat, as well as in the state of glucose. These questions consequently require more investigations, and for this purpose I have undertaken the present work.

Analytical Processes Employed.—The analytical methods employed by chemists to isolate the substances which compose aliments are all attended with many difficulties. I have done all I could to overcome these, by taking the greatest care in the analyses which I made, or by modifying the usual processes. Those who have made these investigations know how long and troublesome they are, but I have persevered in the hope of being useful, and from the conviction that to study the physiological use of aliments, it was necessary, in the first place, to be well acquainted with their composition.

The aliments furnished by the cereals and leguminous plants, which form the principal subject of this first Memoir, contain water, mineral salts, cellulose, nitrogenous matters both soluble and insoluble in

water, fatty substances, and, especially in the leguminous plants, peculiar products,—such as tannin, and some resiniform matters, which have not hitherto been thoroughly examined, and which have but little importance in the light of aliments.

The following is a summary of the methods which I employed to separate these principles from each other.

Determination of the Water.—The quantity of water was estimated by drying 10 grammes of matter in an oven, with a current of air heated to 120° C. (248° F.) The substance was weighed until its weight remained unchanged. We shall see further on, that in the graminaceous and leguminous vegetables the proportion of water varies between 12 and 16 per cent.

Fixed Matters.—I have determined the weight of the inorganic substances by calcining in a platinum crucible a known quantity of the aliment, and weighing the residue, which was generally formed of carbonate of lime and magnesia, phosphate of potassa, sulphate of potassa, and chloride of potassium. Among these salts, those which it is the most important to estimate are the phosphates.

Cellulose.—The process hitherto employed for the determination of the cellulose contained in aliments furnished by vegetables are defective; indeed, they consist in treating them successively with acids and alkalies, diluted with boiling water, alcohol, and ether, and weighing the residue which resists the action of these solvents: the but slightly aggregated cellulose existing in the interior of the grain is dissolved, as I have shown in my work on the chemical composition of bran, and in my researches on the lignine of corn. The process which I followed, and which depends chiefly on the employment of diastase will be described further on.

Nitrogenous Matters.—If the nutritive value of aliments is not represented only by the nitrogenous matters which they contain, we must admit that they play a very important part in alimentation, and that animals which are fed with non-nitrogenous aliments alone are sure to die.

The estimation of the nitrogen is therefore most interesting and should be made with the greatest care.

The albuminoid matters found in aliments, and which are known as fibrine, albumen, caseine, legumine, &c., have various physical characters, but always the same chemical composition. The proportion of nitrogen is about 16 per cent., and consequently the quantity of nitrogen found by experiment gives by a simple calculation the quantity of albuminoid matter. In wheat the nitrogenous matter takes two forms, those of albumen and gluten. In practice it is best to separate them by washing; so as to examine their physical characters. In fact, if wheat occupies the first rank among alimentary substances, and other cereals are inferior to it, especially with respect to purification, it owes this advantage to gluten; but, by this process, it is impossible to obtain all the gluten, and, moreover, all the albumen passes away in the washing waters.

I have ascertained the proportion of nitrogen by the excellent

method of MM. Will and Warrentrapp, as modified by M. Peligot. This process consists in transforming all the nitrogen of the organic matter into ammonia, causing the ammoniacal vapors to pass into a known volume of estimated sulphuric acid, and determining, by means of an alkaline liquor, the quantity of ammonia combined with the sulphuric acid.

I have, in some measure, modified M. Peligot's process; thus, instead of introducing oxalic acid at the bottom of the tube, which gives hydrogen towards the end of the operation, I have found it better to drive off the gasses by a current of air, by means of an aspirator in connection with the Liebig's tube of the apparatus.

M. Peligot has proposed preparing the alkaline liquor by triturating slaked lime with a dilute solution of sugar; but this solution changes by degrees, especially in summer, and requires frequent estimation. I use a solution of potassa lime, suitably diluted with water, which has not this inconvenience.

The estimation of the nitrogen of all the alimentary substances which I have examined was executed by this process. The old process of separating the nitrogen in the gaseous state is very difficult, and usually gives an excess of gas produced by the air of the tube, and the hydrogen of bin-oxide of nitrogen furnished by the substance analysed.

Some physiologists, MM. Schlossberger and Kempt, have estimated the quantity of nitrogen contained in aliments by the original process of MM. Will and Warrentrapp, which consists in precipitating the ammonia with bichloride of platinum, washing the ammonio-platinic precipitate with a mixture of alcohol and ether, which dissolves the bichloride of platinum in excess, and does not dissolve the double chloride of platinum and ammonia. But the execution of this process is long and difficult, and the results which it furnishes are never exact.

Starch.—The proportion of starch contained in the cereals and leguminous vegetables may be ascertained with sufficient exactness by transforming it into sugar by means of diastase. This is performed at a temperature of 60° C. (140° F.), and the contact is continued until the starch has completely disappeared. The mixture is thrown on a filter, washed, and, as a portion of the starch is still in the liquid in the form of dextrine, this is converted into sugar by boiling in presence of sulphuric acid. The sugar is then estimated by means of a known cupro-potassic solution. The sugar and dextrine existing naturally in these aliments, and which raise the amount of starch too high, are estimated directly, and subtracted from the latter.

The process of estimation with acids, which is recommended by several authors, offers great difficulties, and gives very inaccurate results; indeed, I shall show in another part, that even very dilute acids will transform the cellulose into sugar.

Fatty Matters.—I have estimated the fatty matters by treating a determined quantity of the dried aliment several times with pure ether. The proportion of fatty matters contained in alimentary substances varies from 1 to 63 per cent., as will be seen as we proceed.

Observation shows that the alimentation of man is only sufficient and easy, when it contains proper proportions of the plastic substances, fatty matters, salts, and alimentary hydrocarbons. If the respiratory aliments predominate, the nutrition is insufficient, and the digestive functions suffer; if, on the contrary, man receives too much nitrogenous substance, he requires a great deal of exercise to burn the quantity of carbon necessary for the support of animal heat, and the excess of nitrogen injures the preservation of the vital functions. Thus, the most nutritious aliments are those which contain the four principles above mentioned in proper proportions, recognised both scientifically and practically. To understand the nutritive value of aliments, we must first determine the proportions of saline, fatty, nitrogenous, and hydrochloruretted matters which they contain. I shall therefore refrain from establishing a scale of nutrition until I have shown the results given by analysis.

I must explain my opinions on the theory of nutritive equivalents, on the substitution of aliments, and on the duties of chemistry in questions of alimentation. The chemical and physiological researches made during the last twenty years, have shewn that an adult laboring man consumes each day about 350 grammes of carbon necessary for the support of animal heat, and 180 grammes of nitrogenous matters charged with the regeneration of the tissues, and which are rejected by the system, particularly under the form of urea and uric acid; his nourishment is not complete, unless it is formed of one part of nitrogenous aliments, and of four parts respiratory aliments; and, finally, his health suffers, if these proportions are much modified. Moreover, when our aliments are not sufficiently nitrogenous, we instinctively augment the proportion of plastic aliments. It is thus that, according to M. Boussingault, in Alsace, the peasantry add curdled milk to their potatoes, and the Indians, in the higher regions of the Andes, eat cooked potatoes, and a large proportion of cheese. The alimentary rations of man must therefore contain the amount of saline matters, carbon and nitrogen, emitted during the twenty-fours for him to retain his health and strength. These are the fundamental principles of human nourishment. But is it sufficient for ascertaining the nutritive value of aliments to determine the proportion of mineral, fatty proteic, and hydro-carburetted matters? I think not. The theory of the nutritive equivalents is correct, but with the condition that it must not be applied in two systematic a manner, and that the theoretical results are always controlled by the practical. Indeed, the nutritive power of the alimentary substances depends likewise on their strength, their cohesion, and their digestibility; and frequently, a substance rich in alimentary principles, but difficult of digestion, nourishes less, and produces less strength than another matter easily dissolved by the digestive juices. Thus, meat nourishes much more than cooked white of egg, although these two substances possess the same composition. If we add to flour one third of its weight of water, it is evident that we shall not produce the same physiological effects as with bread which contains the same principles. Gelatine is more nitrogenous

than meat, and yet numerous experiments have proved that dogs die after several weeks of this exclusive regimen, whereas, with meat alone, they live much longer. If the nutritive power of aliments generally increases with the proportion of nitrogen they contain, we must still admit that all nitrogenous matters cannot be considered as nutritious for man, and that it is necessary that they should be absorbed in the proteic form. Thus, the nitrogenous principles of the cortical layers of corn are refractory to the action of the digestive organs of man, such as the straw of wheat, rye, barley, oats, &c. In most scales of nutrition the starting point is always one of the constituent principles of aliments, nitrogen; theory likewise has given indications which are frequently uncertain, and in opposition to practice.

These observations being made, we will give the name of *alimentary equivalents* to the quantities of the various aliments of man and animals which, like the chemical equivalents of bodies, mutually replace each other, produce the same physiological effects, and which may, consequently, be substituted one for the other. Those authors who have occupied themselves with nutritive equivalents have given numbers which differ much from each other. Thus, without mentioning other examples, the equivalent of wheat would be 49, according to M. Boussingault, and according to Bloch, 27. The latter gives for the equivalent of rye, 33; and M. Boussingault, 51; Einhoff places the equivalent of oats at 83, Bloch at 39, and Boussingault at 54.

We should consider in each aliment separately the nitrogenous matters, the carbonaceous principle, the fatty substances, the mineral matters, and form a separate table for each of these principles. This is the only way of rendering alimentary equivalents useful. Thus, we shall find, that for one part of nitrogenous substance, beans, haricots, peas, and lentils, contain about two parts of non-nitrogenous principles, corn five parts and rice ten; meat contains about three times as much nitrogenous principle as bread. But, if we only take as a point of comparison one of the essential principles, nitrogen, alimentation would become impossible. Thus, if we wished to substitute rice or dry haricots for meat, we should require, if we considered only the nitrogen, for 250 grammes of meat, 190 grammes of haricots, and 550 grammes of rice. These quantities contain the same proportion of nitrogen; but the ration of haricots, and especially that of rice, would contain too much carbonaceous matter. Such a regimen would evidently be injurious to health, and could only be possible as a very rare exception. On the other hand, if we wished to substitute 250 grammes of meat for 550 grammes of rice, or 750 grammes of bread, the man would soon die of inanition. He would not receive nearly the quantity of carbon necessary for the support of animal heat.

A good table of alimentary equivalents is indispensable for the formation of fixed rations, and to prove this, let us take for example, bread and meat, which usually form the principal bases of human nourishment. If we admit that 100 grammes of bread contain seven grammes of nitrogenous matter, and that to make up for what a man loses in 24 hours, the aliments must furnish to the system 130 gram-

mes of nitrogenous matters, it would be necessary to take 1850 grammes of bread, if the alimentary ration is to be formed of that one aliment alone. But this quantity of bread contains an excess of carbon which is a loss to the consumer, and which fatigues the digestive organs. On the other hand, if the regimen is composed entirely of meat, to furnish the necessary quantity of carbon the man would require about three kilogrammes of meat; but this quantity would contain such an excess of nitrogen as to injure the regularity of the physical functions. A mixed regimen must, therefore, be determined on of meat and bread, so that it shall contain one part of nitrogenous principle and about four parts of respiratory aliments. Moreover, instinct guides us in the choice of our food, and when it is too nitrogenous we increase the amount of respiratory aliments by fat, or the amylaceous bodies.

(To be continued.)

CHEMICAL REPORT on the MODE of DETECTING VEGETABLE SUBSTANCES MIXED with COFFEE for the PURPOSE of ADULTERATION.

By Professor GRAHAM, Dr. STENHOUSE, and Dr. D. CAMPBELL.

Addressed to the Chairman of the Board of Inland Revenue.

THE adulteration of coffee in the condition of the original bean, unground and unroasted, could only be effected by the substitution of a different seed, and would form the subject of an inquiry entirely botanical. But it is proper to remark that the coffee-bean is liable to be rendered entirely worthless, without any inquiry as to its structure, when kept in a wet or damp state for some time, apparently from the readiness with which the soluble constituents of the bean spontaneously ferment. Coffee damaged by sea-water has been found to retain neither the aroma nor bitter flavor of the seed, and to have lost the whole of its characteristic principle, caffeine. The entire soluble matter which can be extracted from the damaged seeds by boiling water is greatly reduced, and does not exceed twelve per cent. of their weight, while the presence of the salts of sea-water is always sufficiently obvious.

The coffee-bean, in its fresh, unprepared state, is tough, and ground with difficulty. It yields an infusion without aroma, which is bitter, and is said to act more powerfully on the nerves than roasted coffee. This seed, however, is always roasted before being made use of, and it is in that state, and with its structure more or less obliterated by grinding, that it must be identified, and its purity established by chemical means of investigation.

In consequence of torrefaction, coffee is materially altered, and acquires new properties. The woody tissue of the fresh bean is horny and differs from ordinary wood fibre in its composition, and is also said not to yield sugar when treated with sulphuric acid. By the roasting, this woody tissue undergoes a partial decomposition, and becomes friable, and the difficulty of pulverising the seed, and

exhausting it by water, is removed. Then it produces at the same time a soluble brown bitter matter, due in part to a gummy substance pre-existing in the coffee, altered like starch by torrefaction, but principally to the conversion into caramel of a quantity of sugar in the coffee-bean, amounting to six or seven per cent of its weight.

A still more characteristic product of the roasting of coffee, is that which gives it aroma. This principle, when separated from the infusion of coffee by distillation, is found to be a brown liquid oil heavier than water, soluble in ether, and has received the name of *Caffeone* (Boutron and Frémy). *Caffeone* is slightly soluble in boiling water; a quantity of *caffeone* which is almost insensible will aromatise two or three pints of water.

In common with all the valuable constituents of coffee, *caffeone* is found to come from the soluble portion of the roasted seed.

The caffeic acid of the green coffee is also changed by the roasting into an acid of different properties.

Of the crystallisable *cafféine*, a small portion may be lost from its volatility, in roasting.

No seed appears to be known, which roasted and pulverised, forms a true equivalent and sufficient substitute for coffee, either in the physiological properties or chemical composition of its soluble extract. A great variety of seeds were tried in France, as substitutes for coffee, during the continuance of the Continental blockade, including in addition to maize, barley, oats, and other cereals, the seeds of the yellow flag (*Iris pseudo-acorus*), grey pea (*Cicer arietinum*), the milk vetch, or Andalusian astragalus (*Astragalus boeoticus*), the *Hibiscus esculentus*, holly, Spanish broom, acorns, chesnuts, the small lupin (*Lupinus angustifolia*), peas, haricots, horse-beans, sunflower, pips of the gooseberry and grape, eglantine (*Rosa villosa*), and the capsules of box (*Buxas sempervirens*). Of the seeds enumerated, the yellow flag, a common marsh plant in England, appears to have offered the only similarity to coffee; but it is doubtful whether the resemblance extended beyond the aroma of this seed when roasted, which is certainly suggestive of coffee.

The search made among the seeds of other plants for a substitute for the coffee-berry may then be said to have entirely failed.

The divergence of the root-substitute from true coffee is still greater in every property except one. The roots which have been most used are those of chicory (*Cichorium intybus*), carrot, beat, rush-nut (*Cyperus esculentus*), earth-nut (*Arachis hypogaea*), scratch-weed (*Gallium aparine*), fern (*Polypodium filix mas*), and butchers' broom (*Ruscus aculeatus*).

The roots of chicory, and of beet and carrot, which are all extensively used in Germany, are similarly prepared, being cut into thin slices, dried in a stove, and then passed through a coffee-roaster—generally with the addition of two per cent. of butter, and sometimes of red powder, to give the color of coffee.

It is to be remarked of the roots, that they are generally used rather as an addition to coffee than as a substitute for it. In one property these roots all agree, and we have no doubt that it has led to this common application of them; chicory, beat, carrot, &c., are all

remarkable for containing a large quantity of sugar, easily caramelised by heat. They acquire, when roasted, the bitter of burnt sugar, with a somewhat similar aroma. Now the taste of this bitter appears to be one of the strongest and most general of our gustatory preferences. It equally recommends toast-water, and the varieties of brown beer or porter, in the preparation of which a portion of malt is used with its sugar caramelised by heat. The caramel bitter is, in fact, a stock flavor, which we find modified by the most various accessories in different beverages, and even in solid articles of diet in a cooked state. It is not surprising, therefore, that the chicory root, containing as it does about thirty per cent. of sugar, more than one-half of which is caramelised in roasting, should obtain extensive favor as an addition to coffee. Fresh chicory has a certain bitterness, or rather acidity, but this is overpowered in the torrefied root by the caramel bitter, and this root may be adequately replaced by the bland beet or carrot, similarly prepared. No one of these roots contains any constituent which associates it with coffee, except sugar; in other respects they are entirely different.

The preparation of roasted chicory appears to have originated in Holland upwards of a century ago, but remained secret till 1801. It is now prepared on a great scale, both on the Continent and in England. The quantity of chicory-powder consumed annually in France is known to amount to 6,000,000 kilogrammes.

In the chemical examination of ground coffee, with the view to discover if it is mixed with the vegetable substances which have been named, or with others, the characteristic constituents of the coffee are less immediately available than certain properties of the infusion of a physical character. This arises from the circumstance, that although it is easy to discover the presence of caffeic acid and caffeine, yet the determination of the exact quantity of these substances in an infusion is both difficult and tedious.

There is reason also to believe that the proportion of caffeic acid and caffeine, varies considerably in different samples of coffee, so that the quantities of these substances (when found) could not show exactly the proportion of pure coffee in a mixture. It will be most advantageous to discuss here the general properties of the coffee infusion, in the first instance, as they are most easily observed; and a single character of this kind in some instances, and two or three in others, will generally be sufficient to establish adulteration when it has been practised. The higher chemical inquiries will then follow.

1. When hot water is applied to the powder of chicory and other roots, it softens immediately, from the facility with which water is imbibed, while the grains of coffee remain hard and gritty in the same circumstances. Ground chicory is highly hygroscopic.

Roasted grain, such as wheat and barley, gives an infusion with hot water, which is mucilaginous and thick, while the infusion of coffee is remarkably thin and limpid. The grain infusion generally contains starch, and gives, when cool, a blue coloration with iodine, while the infusions of both coffee and chicory appear to be entirely destitute of starch.

2. The more deep and rapid coloration of water by chicory and the allied roots than by coffee, affords a useful indication in a preliminary examination. The roasted grains also appear to color water more deeply than coffee does. The relative coloring power of coffee, chicory, and a variety of other vegetable substances used in the adulteration of coffee, was determined with considerable precision by infusing equal quantities of each in water, as in the preparation of coffee, filtering the infusion through paper, and observing the color in glass tubes of equal diameter—about one inch. The solutions required to be very dilute. It was also necessary to have a standard of comparison, and for this purpose caramel, carefully prepared from cane-sugar, was had recourse to. The standard solution of caramel consisted of one grain of that substance dissolved in 2000 grains of water. To produce the same intensity of color as the standard solution, a larger proportion than one grain of all the other substances required to be dissolved in 2000 grains of water. The proportion necessary is expressed in Table I. The substances are all roasted, as they would be used to mix with coffee.

TABLE I.

Weight of Substance (roasted) dissolved in 2000 parts of water, to produce an equal depth of color.

Caramel	1	Red beet	3.33
Mangold-wurzel	1.66	Bread raspings	3.64
Bonka (a coffee substitute)	1.66	Acorns	5.00
Spark's vinegar coloring	1.74	Over-roasted coffee	5.46
Black malt	1.82	Highly-roasted coffee	5.77
White turnips	2.0	Medium-roasted coffee	6.95
Carrots	2.0	Another specimen of coffee	6.66
Chicory (darkest Yorkshire)	2.22	White lupin seed	10.00
Parsnips	2.5	Peas	13.33
Maize	2.86	Beans	13.33
Rye	2.86	Spent tan	33.00
Dandelion root	3.33	Brown malt	40

It will be seen from the preceding table, that 2.22 parts of chicory have the same coloring power as 5.77 grains of highly-roasted, and 6.95 grains of medium-roasted coffee, or of 13.33 grains of roasted peas, and forty parts of brown malt.

The same results are given in a different form in Table II.

TABLE II.

Coloring power of various Substances (roasted) dissolved in an equal quantity of Water.

Caramel	1000	Red beet	300.3
Mangold-wurzel	602.4	Bread raspings	274.72
Bonka (a coffee substitute)	602.4	Acorns	200
Spark's vinegar coloring	574.71	Over-roasted coffee	183.15
Black malt	549.45	Highly-roasted coffee	173.81
White turnips	500	Medium-roasted coffee	143.88
Carrots	500	Another specimen of coffee	150.15
Chicory (darkest Yorkshire)	450.45	White lupin seed	100
Parsnips	400	Peas	73.18
Maize	350	Beans	75.18
Rye	350	Spent tan	30
Dandelion root	300.3	Brown malt	25

The number for chicory (450) indicates that that substance possesses nearly half the coloring power of caramel (1000); while highly-roasted coffee (173) is about one-sixth of caramel. Maize and rye, with probably all the other cereals, rise to 350, and have therefore a high coloring power, quite double that of coffee; while peas and beans (75) are on the other side, and possess only about half the coloring power of coffee for equal weights of the substances compared.

The preceding solutions were prepared at 212°, and chicory then exhibits about three times the coloring power of coffee. But when the solutions are prepared without heat, the disparity is still greater. In cold water chicory exceeded coffee about four and a half times in coloring power.

When a few grains of roasted chicory, or any other sweet root, are dropped into a glass of cold water, without being stirred, a yellowish brown color diffuses rapidly through the liquid, while the pure coffee gives no sensible color to the water in similar circumstances.

3. Another property of infusions, which is still more precise and valuable, is their specific gravity.

The proportion of substance found most suitable for an extensive comparison was one in ten of water. The substances were not exhausted by water, but simply placed in about a pint of cold water, in the preceding proportion by weight, and the temperature raised to 212°, and retained at that point for not more than half a minute. The infusions were then filtered through paper. The substances, as usual, are roasted and ground equally fine, with the exception of the last three in the table.

TABLE III.

Specific gravity of Solutions at 60° F., one part of Substance to ten parts of Water.

Spent tan	1002·14	English chicory (Yorkshire) ,	1019·1
Lupin seed	1005·7	Black malt	1021·2
Acorns	1007·3	Turnips , ...	1021·4
Peas	1007·3	Rye meal	1021·6
Mocha coffee	1008·0	English chicory	1021·7
Beans	1008·4	Dandelion root	1021·9
Neigherry coffee	1008·4	Red beet	1022·1
Plantation Ceylon coffee	1000·7	Foreign chicory	1022·6
Java coffee	1008·7	Guernsey chicory	1023·2
Jamaica coffee	1008·7	Mangold-wurzel	1023·5
Costa Rica coffee	1008·98	Maize , ...	1025·3
Native Ceylon coffee	1009·0	Bread raspings	1026·3
Costa Rica coffee	1009·5	British gum	1037·9
Brown malt	1010·9	Gum arabic	1038·6
Parsnips	1014·3	Cane sugar	1040·9
Carrots	1017·1	Starch sugar	1042·8
Bouka	1018·5		

The leguminous seeds, it appears, give a low specific gravity—peas 1007·3, and beans 1008·4. The coffees are also remarkably low, varying from Mocha coffee 1008·0, to Costa Rica 1002·9; while chicory rises greatly, ranging in different samples between 1019·1 and 1023·2. The cereals are equally high, or still higher, in the scale of

gravity ; rye meal being 1021.6 and maize 1025.8. The low gravity of the coffee infusion, therefore, distinguishes it sharply from the two most important classes of adulterating substances,—the roots and the cereals.

4. The action of other solvents, besides water, may be shortly referred to.

Agitated four times successively in ten times their weight of ether, the substances to be mentioned gave different proportions of matter soluble in that menstruum :—

Roasted beans . . .	1.81 per cent. of oil and resin.
„ maize . . .	5.15 „ „ „
„ chicory (Yorkshire) .	6.83 „ „ „
„ coffee (Mocha) .	15.93 „ „ „

including probably nearly one per cent. of caffeine.

It thus appears that coffee yields much more soluble matter to ether than beans, maize, or chicory, which represent the three classes of leguminous seeds, cereals, and sweet roots. The fat obtained from chicory was no doubt principally composed of the sweet American or Australian tallow, added by English manufacturers to the root, in roasting, to prevent burning. The experiment with ether is easily made, and may in particular circumstances prove valuable.

The solubility of the same substances in proof spirit was observed, the substance being exhausted four times in succession by ten times its weight of proof spirit, at the boiling temperature.

Roasted beans gave 17.5 per cent. of a dry, blackish, lustrous extract.

Roasted maize gave 50.2 per cent. of an extract much like the preceding.

Roasted chicory (Yorkshire) gave 67.76 per cent. of extract of a lighter color than the preceding, but otherwise very similar.

Roasted coffee (Mocha) gave 26.35 per cent. of extract, much like the first two in external appearance.

These operations are remarkably tedious, while the experiments with ether, on the contrary, are easy and simple. The results with proof spirit are not sufficiently characteristic to recommend its application.

5. Fermentation by means of yeast gives a decisive proof of the adulteration of coffee by many vegetable substances, particularly by chicory and the other saccharine roots.

In our fermentation experiments, 2000 grains of the coffee or other substances were weighed out, and treated successively with one pint and a half of cold water, one pint and a half of water about 174°, and a little additional water for washing the solid residue upon a filter of fine calico. About three pints of infusion were thus obtained, which were mixed with 250 grains of brewers' yeast, weighed after being pressed in a calico bag. The fermentation was continued for 48 hours, at a temperature of from 80° to 90°. The fermented liquor was afterwards distilled, and about 6000 grains brought over. The last vinous

liquor was rectified a second time, about 3000 grains being now collected; and the alcohol was inferred from the density of this distillate.

The substances were examined for sugar, both before and after being roasted, as it was interesting to observe the extent to which the sugar is caramelised, by torrefaction, in the different substances.

The sugar of coffee is found to be reduced by roasting from 6 to 7 per cent. in raw coffee, to from 0, to 1.12 per cent. in the roasted,—or to be almost entirely destroyed; while in other substances the sugar is more generally reduced by precisely similar treatment, to from one-half to one-third of its original proportion. It is difficult to account for this dissimilarity, unless a portion of the sugar of raw coffee exists in a state of conjugate combination, like the sugar in tannin, amygdalin, salicin, &c. Our attempts to isolate such a conjugate body, however, from raw coffee, have not yet proved successful, and its existence is therefore hypothetical. We succeeded, on the other hand, in crystallising out cane-sugar from an infusion of raw coffee. It was decided by proper experiments that the fermentation of sugar was not interfered with by the empyreumatic products, or essential oil of roasted coffee.

The sugar was determined which exists in the most dissimilar varieties of coffee, the wild and cultivated beans, the beans from Ceylon and the West Indies, from Arabia, and the Neilgherry Hills. Twelve different samples were examined, each both before and after roasting.

TABLE IV.

Sugar found in Coffee, before and after Torrefication.

	Sugar per cent.			Sugar per cent.	
	Raw.	Roasted.		Raw.	Roasted.
1. Plantation Ceylon .	7.52	1.14	7. Costa Rica . . .	6.72	0.49
2. " " .	7.48	0.63	8. " " . . .	6.87	0.40
3. " " .	7.70	0.0	9. Jamaica . . .	7.78	0.0
4. " " .	7.10	0.0	10. Mocha . . .	7.40	0.50
5. Native Ceylon . .	5.70	0.46	11. " " . . .	6.40	0.0
6. Java . . .	6.78	0.48	12. Neilgherry . . .	6.20	0.0

The sugar in coffee appears to be increased by cultivation,—the proportion in Native Ceylon being 5.7 per cent., and in Plantation Ceylon from 7.1 to 7.7 per cent.

The proportion of sugar in the dried roots is high at first, and continues after roasting still very notable.

TABLE V.

Sugar in Chicory and other Sweet Roots, before and after Torrefication.

	Sugar per cent.			Sugar per cent.	
	Raw.	Roasted.		Raw.	Roasted.
Foreign chicory . .	23.76	11.98	Turnips	30.48	9.65
Guernsey chicory . .	30.49	15.96	Beet root (red) . .	24.06	17.24
English chicory . .	35.23	17.98	Dandelion root . .	21.96	9.08
" " (Yorksh.) . .	32.06	9.86	Parsnips	21.70	6.98
Mangold-wurzel . .	23.68	9.96	Bouka (a coffee substitute	—	5.82
Carrots (ordinary) .	31.98	11.53			

It thus appears that roasted chicory, as it is sold for mixing with coffee, retains from 9·86 to 17·98 per cent. of undecomposed sugar. In none of the other sweet roots which are occasionally substituted for chicory, such as mangold-wurzel, beet, turnip, or carrot, does the proportion of sugar retained after torrefaction fall under 9 per cent., with the exception of parsnip, in which the sugar falls to 6·98 per cent. The coffee substitute, "Bouka," falls into this class, and appears to be a mixture of true coffee with a torrefied sweet root.

The last group is composed of the leguminous and certain other seeds, with the cereals. The sugar was determined for most of these substances in the roasted form of the grain only, which alone affects the question of adulteration.

TABLE VI.

Sugar in various Seeds, before and after Torrefication.

	Sugar per cent.			Sugar per cent.	
	Raw.	Roasted.		Raw.	Roasted.
Acorns	8·64	2·70	Bread-raspings	—	1·78
Horse-beans	—	1·62	Lupin-seed	—	0·74
Peas (grey)	—	1·08	Brown malt	8·48	—
Maise	—	0·82	Black malt	—	1·66
Rye-meal	—	1·96			

In the roasted seeds enumerated, the proportion of sugar is sensible, but not sufficiently considerable to give the means of distinguishing leguminous seeds and cereals from coffee. In roasted acorns, the sugar rises to 2·7 per cent., and black malt, it will be observed, still retains 1·66 per cent. of fermentable matter.

The fermentation test, on the other hand, is adapted to detect adulteration by chicory, and the sweet roots, and will, we believe, from its certainty and facility of application, prove eminently useful for that purpose.

To be continued.

On the EXAMINATION of FLOUR and BREAD. By M. RIVOT.

(Concluded from page 59.)

EXAMINATION OF BREAD.

UNDER ordinary circumstances we examine bread taken at the baker's, without knowing exactly the nature of the flour used, the mode of manufacture, the duration of the baking, or even the time which has elapsed since it was removed from the oven; the consequence is, that the results obtained are generally incomplete, and connected with the state of the loaf, at the moment in which it is experimented on.

The operations necessary to ascertain the quality of bread are the following:—

1. The examination of the external characters, the degree of baking, the odor, the taste, the amount of hardening from spontaneous evaporation.

2. The determination of the hygrometric water in the crumb, and in the crust, and, consequently, the amount of dry matters contained in the bread.

3. The incineration of these two parts separately, and the calculation of the dry flour employed to produce 100 parts of bread. When we know the flour employed, and its proportions of water, we can calculate the amount of its product in bread.

4. The analysis of the ash, the estimation of the salts, the search for any mineral matters which may be introduced during the panification.

5. The search for any other descriptions of flour which may have been mixed with the wheat flour.

EXTERNAL CHARACTERS.

Bread should be examined from twelve to eighteen hours after its removal from the oven,—namely, in the state in which it is most usually sold by the baker.

The loaf is weighed entire, and then cut into two equal portions; one is intended for the determination of the hygrometric water and should be weighed again; the other is used in the examination of the external characters.

We observe the aspect, the odor, the taste, and the consistence of the crumb and of the crust. Rye, haricots, buckwheat, potatoes, &c. are easily recognised by their taste and odor, even when only in small proportion; maize renders the crumb rather sticky, and gives it a very evident yellow color. Fermented wheat flour renders the crumb heavy and sticky; loaves prepared with this flour have a disagreeable and peculiar odor; their taste is sourish, and the digestion of them is difficult.

Loaves prepared with mixed flours almost always harden more rapidly than loaves made of good wheat flour; horse chesnuts, potatoes, haricots, and rice, hasten the hardening of the loaves; but they do not give any new or disagreeable taste. It is not the same with bread made from fermenting flour, which hardens rapidly, and becomes worse and worse, as it becomes dry. This bread becomes sourish, and is covered with mould in less than four days, even in a dry place.

Numerous experiments have shown M. Rivot that the crumb of bread kept in a capsule, at 18° or 20° C. (64° to 68° F.), loses weight progressively—at first quickly, afterwards very slowly—and does not arrive at a stationary hygrometric state, until the end of eight or nine days. It then retains from 9 to 10 per cent. of water, a proportion differing but little from that of wheat flour of good quality, in the same conditions; whence it appears that the portion of the flour which produces the crumb does not undergo any great alteration in becoming bread.

DETERMINATION OF THE HYGROMETRIC WATER.

For this determination we should operate on a whole loaf, or at any rate, on the larger proportion of one which should be carefully chosen

to represent the average proportion of crumb, of crust, of well baked parts, and of those which have been less heated. After dessication, the crumb and crust should always be incinerated, so as to be able to calculate, as mentioned further on, the proportion of flour employed in the manufacture of the loaf, and we must remark that the result of this calculation can be exact only when we operate on a whole, or at any rate, on half a loaf.

M. Rivot considers as crust all the parts of the bread which have undergone more or less decomposition in baking, and as crumb those parts which have lost nothing of their organic substance. The crumb and the crust separated immediately after the loaf has been weighed, should each be weighed separately, divided into very small fragments, and placed in porcelain capsules.

The proportion of crust to crumb in a loaf demonstrates in loaves of the same form the degree of baking, which depends as much on the duration as on the temperature of the fire, and in different loaves, the influence of the form on the proportion of crumb and crust, and, consequently, on the yield of the flour in bread.

In loaves of 2 kilogrammes, called bricks, the proportion of crust to crumb is from 0.43 to 0.33 per cent.

In fancy loaves of 2 kilogrammes, which are longer than the preceding, the proportion of crumb seldom exceeds 70 per cent.; it descends to 60 per cent. in loaves that are over baked. In still longer loaves the proportion of crumb does not exceed 55 per cent. and in this case the yield of bread from the flour is very small.

Round loaves approximate bricks; the proportion of crust to crumb is ordinarily comprised between 0.50 and 0.60, except in those of half a kilogramme, in which the proportion of crust is larger, and reaches 45 per cent. Finally, in manchets of 2 kilogrammes, the proportion of crust to crumb is not far from 0.50.

In loaves of a known form, the mode of baking is manifested by the comparison of their appearance with the proportion of crust which they contain. A rapid baking in a very hot oven almost always gives a burnt crust and an ill cooked crumb, which retains a great deal of water. Loaves subjected to a prolonged baking, in a moderately hot oven, have a well colored, thick crust, and a well cooked crumb, containing but little water.

Loaves made with fermented flour, mixed in large proportion with flour of good quality, can only be rendered passable by slow baking, prolonged for an hour.

The dessication of the crumb and crust should be made, as far as possible, in a hot air stove, kept at a temperature of from 110° to 115° C. (230° to 239° F.)

We know that the dessication is perfect when the crumb becomes slightly yellow on the surface, and when its weight remains stationary, in two weighings made at an interval of six hours. When operating on loaves of bad quality, we must redouble our precautions, and the temperature must never rise higher than 100° C. (212° F.), so as not to alter the bread by causing it to lose the water of combination,

which is disengaged with much greater facility from these loaves than from those which have been made with good flour. Besides this, partial decomposition is evidenced by the want of accord between the proportion of dry matters from the bread and that of the flour, calculated from the ash. It is essential to weigh the dessicated crust and crumb very rapidly, as these substances are very hygrometric.

Well baked loaves of good quality, examined eighteen hours after removal from the oven, contain from 33 to 34 per cent. of water; the crumb gives 42 to 43 per cent. and the crust 17 to 18 per cent.

In the various samples examined by M. Rivot, the extreme limits of the proportion of water were:—

Crumb	.	.	.	.	.	40 to 48 per cent.
Crust	.	.	.	.	.	17 to 27 „
Entire loaf	.	.	.	.	.	30 to 41 „

INCINERATION.

The incineration of the dried crumb and crust requires very great precaution; it can only succeed in porcelain capsules, under a large muffle, heated cautiously to a dull red. The capsules are moved by degrees to the middle of the muffle, so as slowly to produce the decomposition and inflammation of the organic matter. When the flame ceases, the matter is heated to dull redness, by placing the capsules at the bottom of the muffle, and inclining them towards the opening, so as to favor the oxidation of the charcoal which has to be burnt.

When the ashes have become very white, they are weighed very rapidly; from the weights obtained are deduced the proportion of ash in 100 parts of crust, crumb, and bread.

The proportion is rather smaller in the crumb than in the crust; in the bread itself it varies with the nature and yield of the flour employed; it is generally between 0.60 and 0.80 per cent. of bread. The determination of the ash gives the means of calculating the proportion of dry flour employed to produce the bread, with very great exactness, when the bread is of good quality.

Admitting that the paste put into the oven and transformed into loaves is perceptibly homogeneous, the relation of the two numbers which express the proportion of ash in the crust and crumb represents the proportion of crumb which would have been produced by that part of the paste which has been transformed into crust. If we multiply this relation by the proportion of crust contained in 100 parts of bread, and if we add the product of the crumb, the sum will represent the crumb which would be produced by the flour employed in 100 parts of bread.

Knowing the weight of the dry flour, it will be easy to calculate the proportion of ordinary flour and its yield of bread, if we have been able, at the same time, to determine the hygrometric state of the flour. If, for example, the flour contains 17 per cent. of water, the number obtained for the dry flour must be divided by 83, to have the proportion of flour, at 17 per cent. producing 100 parts of bread; and then divide 100 by the proportion of ordinary flour, to have the

yield in bread of 100 parts of flour. The comparison of the two numbers which represent the proportion of dry flour employed, and that of the dry matters contained in 100 parts of bread, should give for the first, an excess which represents the loss of organic matters during the baking, by the formation of the crust.

In fancy loaves of 2 kilogrammes, the loss of organic matter varies from $1\frac{1}{2}$ to 3 per cent. of the dry flour employed; in the brick loaves, it is between $\frac{1}{2}$ and 2 per cent. In split loaves and small round loaves it is from 3 to 4 per cent., and in round loaves of great diameter, and well baked manchetts, it is nearly 2 per cent.

The calculation of the yield of flour demonstrates that for loaves of good quality, equally baked, it depends chiefly on the form of the loaves. M. Rivot has deduced, from a great number of experiments, the following averages for loaves manufactured with flour of good quality, containing 17 per cent. of hygrometric water, and experimented on 18 hours after their removal from the oven;—

100 parts of flour produce

125 to 130 of brick loaves of 2 kilogrammes.

120 to 125 of fancy loaves of 2 kilogrammes

112 to 122 of very long loaves.

120 to 128 of round loaves, according to their diameter.

125 to 135 of manchetts of 2 kilogrammes, according to the baking and the thickness of the crumb.

Some separate experiments seem, moreover, to show that during the first eighteen hours after their removal from the oven, loaves lose from 3 to 5 per cent. of their weight. If this number is accurate, it results that for well baked manchetts, round loaves, and bricks, the average yield of ordinary flour is nearly 133; that for fancy loaves it amounts to 125 or 130; and for long loaves it is frequently under 120.

M. Rivot has ascertained that salt is found in bread in proportions which vary between 0.17 gra. and 0.78 gra. per kilogramme of bread, but in general the amount is 0.45 gra.

With respect to ascertaining the quality of the bread, the analysis of the ash is of small value when the proportion is comprised between the ordinary limits of 0.60 and 0.80 per cent. of bread; but when it exceeds this amount it is necessary to seek for mineral salts, or the products of decomposition of these salts, such as sulphate of copper, sulphate of magnesia and alum, which some bakers sometimes introduce into bread.

EXAMINATION OF BREAD MADE WITH MIXED FLOURS.

When bread contains very considerable proportions of the flour or rye, haricots, maize, &c., their presence is manifested by the peculiar taste and odor of the bread, and by the more or less rapid hardening which it undergoes in drying spontaneously. But when these characters are wanting, it is necessary to examine, simultaneously,

the bread and the flour employed in its manufacture, taking advantage of all the characters described in the course of this Memoir as aiding in the estimation of bread and flour.

M. Rivot concludes his Memoir with a table of all the results which he has obtained from careful examination by the methods described of twenty-one different samples of bread; and with a table of the composition of the ash of ten samples of brick loaves, bought of different bakers in Paris: as we have already given the average of these results, we shall not reproduce them here.

Journal de Pharmacie et de Chimie, September, 1856.

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS

On CAUSTICS in a CHEMICAL POINT of VIEW.

By M. E. FERRAND.

THE question of caustics having been raised by the *Société des Sciences Médicales et Naturelles*, at Brussels, and the disposition existing to substitute caustic for cutting in many surgical cases, and, finally, the absence of chemical experiments, whether direct or indirect, on the nature of caustics; all these circumstances have convinced me of the importance of a work on this subject.

Dr. Philipeaux has undertaken this task of examining, in all its relations, the very extensive question of caustics and the methods of cauterisations, and has entrusted the chemical portion of the investigation to me.

To appreciate, in a general manner, the question of caustics, to group analogies, to characterise differences, to study, with respect to knowledge, their solvent or coagulating power, and to trace their effects, in the development of the eschar; I undertook a great many experiments which had the advantage of being comparable with each other, and thus gave very accurate results. They were conducted in exactly similar conditions of medium, time, and quantity, and always carried on comparatively. The blood and its simple or conjugate elements, serum, globules, fibrine, and clots, all the tissues, both with epidermis and deprived of it, gave me, by their contact with caustics, so many subjects of study, interesting in theory and useful in practice. But how are we to arrange the numerous elements of this study, and how are we to draw useful conclusions from these experiments on dead bodies.

What are the agents employed in cauterisation? The caustics of Vienna, Recamier, Velpeau, Canquoin, Rivaillé, etc. These are chemical agents.

What is their action on our tissues and liquids? They produce

immediate or successive decompositions and combinations—in fact, new bodies in which have disappeared the properties of the matters put in contact.

They have, then, chemical actions. Consequently :—

1st. It is a powerful force which disorganises dead tissue, as well as living tissue.

2nd. It is evidently a chemical classification which should be applied to them.

And this classification is correct on several accounts.

Without introducing a new word, it borrows from a well known science, a few of its most familiar expressions, giving them their proper definition.

It forms into one group caustics which have an analogous composition, and, consequently, an analogy in their proper definition.

It separates and distinguishes those which, having a different composition, have, consequently, a dissimilar kind of action, first on the tissues, and consecutively on the rest of the economy.

From these advantages it follows that, made from a chemical view, it remains perfectly correct in a surgical point of view.

It is very simple and short, for it comprehends but three groups, alkaline, acid, and metallic caustics.

Clinical observations led M. Bonnet to this classification; experiments in the theatre and laboratory lead to the same.

If studied without preconceived theoretical ideas as to their relation to the albuminoid and fibrinous elements, caustics arranged according to their merit, from whichever point we consider them, constitute a long list, in which the alkaline caustics become associates; the acid caustics likewise unite with each other, and the metallic caustics constitute a group apart, the acid metallic caustics forming the link between these two last classes.

This result is always true, whichever begins or finishes the list.

Those experimental data are completed :—

I. By the general characters of each group.

II. By the separate study of each individual substance.

III. By a chapter on the employment of caustics.

IV. By observations on the composition and developement of the eschars.

V. By considerations on the absorption of caustics.

One single example will enable me to shew the characters of the three categories already described, and this chemical result is shewn from my numerous comparative experiments, as having a great surgical importance.

Albumen in solution gives a very fluid liquid with alkaline caustics; with acid caustics, a pasty mass, which does not dry, even after eight to ten days; with metallic caustics, an opaque clot, which hardens and dries in a short time, so as to become pulverulent under the pressure of the fingers.

LIST OF THE PRINCIPAL CAUSTICS.

Ammoniacal	Cupreous
Antimoniocal	Mercurial
Silver	Acid Mercurial
Arsenic	Nitric
Gold	Potassic
Calci-potassic (of Vienna, of Filhos)	Stannic
Chloro-bromic	Sulphuric
Chromic	Zincic

ALKALINE CAUSTICS.—AMMONIACAL, ANTIMONIACAL, AND CALCI-POTASSIC.

In general, caustic alkalies occupy the first rank with respect to the homogeneity of the result, the second place with regard to the extent of the action produced, and the last with respect to consistence.

They have a dissolving action on the elements of the blood; they attack *the skin* powerfully, and this property is characteristic; indeed, they are used in many cases, and their application at first becomes necessary, when we intend immediately to use a metallic caustic, as these last do not break *the skin*.

With Muscles.—A soft, not very distinct, eschar, 3 millimetres in thickness.

Dried as far as possible in the air, the cauterised portion weighed two, when the product of an analogous cauterisation weighed four with acid, and one with metallic caustics.

ACID CAUSTICS.—NITRIC, CHLORO-NITRIC, CHLORO-BROMIC, CHROMIC, SULPHURIC, &c.

In general, the results which they produce, occupy the first rank as regards extent, and the second for consistence; their action is essentially penetrating; their action on the elements of the blood varies; they solidify the albumen of the blood into a paste, with a greasy appearance; they coagulate the globules of the blood; hydrochloric acid destroys them, forming a gelatinous magma. They liquify the fibrine.

They attack the skin powerfully, when not deprived of epidermis, thus recalling the action of the alkaline caustics; but if the acids are not very concentrated, they wrinkle and crack it without cauterising.

With the Exposed Muscle.—A moderately firm eschar, more or less spongy; more often irregular, incomplete, and thus showing that they penetrate deeply, but not energetically. Its thickness easily becomes 3 millimetres, and we find even more deeply that it has been imbibed.

They produce abundant suppuration.

When dried, as far as possible, in the open air, the eschar weighs four, when that cauterised with alkalies weighs two, and that with metallic salts weighs one.

METALLIC CAUSTICS.—OF ANTIMONY, SILVER, ARSENIC, GOLD, MERCURY, TIN, ZINC, &c.

In general, the results which they give are combinations which, among caustics, occupy the first rank as to consistence, and the last as regards extent, and even sometimes of homogeneousness.

They solidify the elements of the blood, albumen, globule, albumenose, hematine, and fibrine, and are, moreover, characterised by their easy dessication, and the insolubility of the compound produced.

With the *skin*, the epidermis is not attacked by metallic caustics which are not dissolved in an excess of acid, or which are not decomposed in contact with it; when applied, the epidermis is only more or less wrinkled.

With the Exposed Muscle.—A dry, hard, eschar, generally well defined.

Whether obtained from dead or living beings, the eschar produced by metallic caustics may be preserved indefinitely, for they are imputrescible.

Acid Metallic Caustics.—Mercurial acid, or acid deuto-nitrate of mercury, and *gold caustic*, or Recamier's caustic, a solution of chloride of gold in *aqua regia*.

They present properties which shew necessarily the proportion and energy of the elements which constitute them. They participate in various degrees in the properties of the caustics of the second and third classes, between which, as I have already observed, they form the connecting link. They nevertheless differ from them in various important points. They differ from the purely acid caustics, because they do not dissolve fibrine, as they do; from the neutral metallic caustics, they are distinguished by the property—which the latter do not possess—of perfectly attacking the skin when quite dry, and possessing its epidermis.

OF PARTICULAR CAUSTICS.

After having given the general and characteristic properties of caustics, considered in analogous groups, we regret that our limits will not permit us to enter into the particular examination of individuals and their destructive properties: we can only mention a few.

It is necessary to have ammonia in a state of absolute concentration, to have the effect of cauterising, and even for the preparation of Goudret's vesicant pomade.

It is advisable to have a little cotton round the pastille of potassa, at the moment of its application.

Lime has a great influence in the caustics of Vienna and of Filhos.

The action of monohydrated nitric acid is so rapid that there is always reason to fear hemorrhage.

It is useful to use *aqua regia* in cases of venomous bites of animals, when other means are not at hand.

Chloride of bromium will be found to possess all the qualities of an acid caustic.

In the long memoir from which this is an extract will be found the proportions of Velpéau's caustic and black caustic, and the superiority of the latter over yellow caustic.

Chloride of antimony, becoming decomposed under the influence of water, it has an acid reaction, which enables this caustic to remove the epidermis.

There is a revivification of the metals of the last class, with their metallic lustre, as far as concerns gold and silver caustics, both on the dead and living body.

It is important to know that 100 grammes of urine decompose, and in some degree annihilate, on an average, three grammes of nitrate of silver, when it is wished to produce in the bladder, by means of injection, the modifying action of this salt.

It is highly necessary to watch, by means of analyses of the urine, the consecutive effects of arsenical caustics.

Acid nitrate of mercury has many advantages, in consequence of the double energies of its constituent elements.

Finally, it is much better to employ chloride of zinc, under the form of sparadrap or seton, with regard to the homogeneity, extent, and limitation of the rapid inflammation which this caustic causes—conditions which render it the most anti-hemorrhagic and safe caustic.

ON THE EMPLOYMENT OF CAUSTICS.

Various experiments, and especially those tried on fibrine and globules united in a clot, and on the other hand, on the muscles, have proved to me how important it is to be very careful in managing caustics.

Clots are small masses with shining surfaces, formed of tender and penetrable paste; the muscles are fibrous tissues, more or less closely united, into which corrosive liquids can easily sink, and caustics are usually either liquids or very deliquescent solids, which are thus in danger of passing through the tissues, unless all necessary precautions are taken. Chloride of zinc itself, the eschar of which so perfectly retains its form, would infallibly fuse, were it not for the precautions M. Canquoin in the preparation of his *Escharotic paste*.

Numerous substances have consequently been employed as excipients; some are inert, such as asbestos; others slightly and only partially modify the composition of the caustics, yolk of egg, flour, &c.; and in all cases, the best are those which best fulfil the conditions of forming:—

- 1st. A sufficiently soft paste to take the form of the parts.
- 2nd. A paste of sufficient tenacity to prevent the caustic from flowing (those which soften at once flow at last.)
- 3rd. A paste sufficiently charged with corrosive matter to cauterise at one application.

For these purposes the following matters have been employed with various success: asbestos, precipitated silica, charcoal, tinder, lint, saffron, gum tragacanth, yolk of egg, and flour.

(To be continued.)

DIABETES MELLITUS. By JAMES GRAY, M.D., Fellow of the Faculty of Physicians and Surgeons, &c., Glasgow.*

It may with truth be said that chemical pathology is but in its infancy. While much has been done within the last few years to elucidate the changes which the primary elements undergo in the living body, much yet remains to be accomplished ere the physician may display anything like mathematical accuracy in the prevention or removal of disease. Still, when we behold the energies of the greatest chemists and pathologists devoted to the subject, we have reason to hope that the day will come when all that is predicted of this important department of science shall be realised.

Of all the proximate principles of the animal economy, none, and deservedly so, has had so much attention directed to it as the saccharine; for, if all the transmutations which sugar undergoes in the system were fully understood, a veil would be removed from the eye of science, and a grand starting-point established for the pursuit of truth.

In the following observations I propose to consider:—

1. The Normal Development of Sugar.
2. Its Abnormal Development as exhibited in Diabetes.
3. Its Treatment, with an especial reference to Rennet.

I. Sugar is a normal principle of the animal economy, and a product of starch, as also of the disintegration of the nitrogenous tissues. It may, moreover, be supplied artificially by a saccharine diet to the system. With the exception of sugar of gelatine, which contains nitrogen, all the other varieties, animal as well as vegetable, differ only in the proportion of water which goes to the composition of each. Thus:—

						C.	H.	O.
Cane sugar .	.	.	.	.	.	12	11	11
Grape sugar .	.	.	.	.	.	12	14	14
Milk sugar .	.	.	.	.	.	24	24	24
Inosite .	.	.	.	.	.	12	16	16

Before entering, however, upon the changes which the amylaceous principle gives birth to, it may not be out of place to premise a few remarks, in order that we may be enabled the more clearly to distinguish the agents at work in the various processes which may come under our view. It is a well-known fact, that in all organised bodies changes are incessantly going on; new matter being deposited, while the effete is expelled. Prior to expulsion, however, it not unfrequently plays an important part in the system, viz., that of fermentation. If we classify the fluids and soft tissues of the body under the two distinct heads of nitrogenous and hydrocarbonaceous compounds, we find that the former, while in a state of transformation by the action of oxygen, are capable of causing, and the latter of undergoing fermentation. We term the one a fermentable body, the other a ferment. As an instance of a ferment, we may take the stomach of the calf. When separated from the body and acted on by oxygen, its

* Read before the Medico-Chirurgical Society.

mucons membrane is capable of converting starch into sugar or lactic acid. Another instance is afforded by the action of cheese in a state of decomposition on sugar-of-milk; as products of which action we have lactic acid, butyric acid, hydrogen, and carbonic acid. It is further established that, in the living body, the epithelium of the mucous membrane of the stomach is constantly being thrown off during digestion, and that in this state coming in contact with the oxygen of the air carried into the stomach by the saliva, it acts as a ferment. According to Liebig, "The decompositions and transformations which occur in the processes of fermentation are effected by matter, the smallest particles or atoms of which are in a state of motion and transposition,—a state susceptible of being communicated to other atoms in contact with the former, so as to cause the atoms and elements of these latter also, in consequence of the resulting disturbance of the equilibrium of their chemical attraction, to change their position, and to arrange themselves into one or more new groups." Finally, it is well attested that matter undergoing putrefaction when introduced into the living body, possesses the property of inducing fermentation.

To the formation of sugar, I shall now briefly advert. Leuchs was the first to affirm that saliva possessed the property of converting starch into sugar, which, by the numerous experiments of Wright, has been fully confirmed. Whilst Magendie next discovered that this function is not due to any one of the salivary glands, but to the aggregate, Lehmann has shown that the saccharifying power of the saliva is the same whether that fluid be acid or alkaline; and Jacobowitch, that its action upon the starch is continued in the stomach. The gastric juice of itself exercises no action upon starch unless mixed with saliva; and that whether the former fluid be acid or alkaline, as the experiments of Lehmann and Jacobowitch testify. The saccharifying power of the pancreatic fluid was first pointed out by Valentine, and has since been verified by Bernard and others. Moreover, such starch as may not have previously been acted upon, is converted into sugar in the small as well as the large intestines. While such is the development of the saccharo-amylaceous principle in the *prima via*, sugar has also been discovered in the serum of the blood by Schmidt and others: Lehmann finding 12 times more in the hepatic than in the portal vein. The glucogenic functions of the liver are clearly demonstrated by the numerous and interesting experiments of Bernard.*

* In a recent publication he states that, instead of searching in the blood for the substance which precedes the sugar, and immediately gives rise to it, we must search for it in the hepatic tissue itself; and made the following experiment, which was repeated by Dr. G. Buchanan and myself:—We took a healthy dog which for several days had been fed exclusively on flesh, and killed it by hydrocyanic acid. The abdomen was immediately opened, and the liver removed without injury to its tissue; and this organ, while still warm, and before the blood had time to coagulate in its vessels, was submitted to a washing with cold water through the *vena porta*, by means of a powerful syringe; the water passed through the liver with great rapidity; it swelled up, grew lighter in color, and the blood was forced out with the water, which escaped in a continuous stream by the hepatic veins; the liver was thoroughly bleached. This washing was continued

Trommer believes he has discovered glucose in the chyle. The muscles contain a peculiar principle termed inosite, which in its action resembles glucose; and in the female, during lactation, sugar exists in increased quantity.

Sugar once obtained, is subject to certain changes. The principal of these is lactic acid, $C^6 H^5 O^5 HO$, having for its composition one atom of sugar split up into two of lactic acid. This substance appears in the juice of flesh as a product of the disintegration of muscular tissue, and is found in the stomach, duodenum, small and large intestines. It also occurs in the chyle in the form of lactate; and although these have never yet been discovered in arterial blood, this arises from the fact that the alkaline lactates are in that fluid rapidly decomposed into the alkaline carbonates. To remove any doubt on this point, Lehmann made the following experiment on himself: he took half an ounce of the lactate of soda, and within 18 minutes his urine had an alkaline reaction.* According to Gregory, the sugar formed in the liver is converted by a ferment in the lungs into lactic acid. This again unites with the soda of the blood, fermentation goes on, the lactate of soda enters the arterial circulation, and is converted into carbonic acid; which, united to its base, returns to the lungs, to be given off as before, and have its place supplied by the lactic acid. Wöhler has proved that the change from lactates to carbonates takes place in the blood.* Lactic acid is found in the saliva, sweat, bile, and urine; its use in the system being to aid the digestion of the nitrogenous compounds, and to serve as food for respiration. In certain diseased states it exists in superabundance, as in diabetes, oxaluria, rachitis, and febrile complaints; while in others, it is deficient, as in scurvy.

II. The mal-assimilation of sugar may originate from various causes. It may depend upon some derangement of the lungs preventing the due oxygenation of the blood, as in pneumonia, bronchitis, &c.; or from a diseased state of the stomach or bowels. It also may result from the introduction into the stomach of anything by the action of which the sugar already formed does not undergo its appropriate change; from medicines of different kinds,—as chloroform, acids, or lastly, from a peculiar condition of the blood, as in rheumatism, oxaluria, and milk-fever.

Diabetes mellitus is a disease characterised by an increased flow of

for three-quarters of an hour; the water was perfectly colorless. The blood from the hepatic vein with the first washing was given to Dr. Penny's assistant, who detected the presence of sugar in it. The liver was also given to him, and submitted to a warm temperature; 24 hours after this, Dr. George Buchanan injected a quantity of cold water through the portal vein, and collected that water as it escaped by the hepatic veins. This fluid, according to Bernard's experiment, contained sugar as shown by the yeast test; but, according to Dr. Penny's assistant, no sugar could be detected in the fluid. Before arriving at any conclusion, however, it will be necessary to repeat this experiment.

* Liebig, moreover, has shown that the alkaline lactates of the blood undergo rapid decomposition into the carbonates, and become one of the best supporters of animal heat.

urine, which contains sugar. I know of no certain sign by which we may detect this disease in its early stage, the symptoms for the most part creeping on slowly and insidiously. Unless, therefore, one is frequently seeing such cases, the diagnosis is difficult, nay, oftentimes altogether erroneous. I could point to many cases that have come under my own care, where the parties have been for months under medical treatment, without the disease having been detected, and that, too, by physicians holding a high position in the profession. At first, dyspeptic symptoms manifest themselves, attended with pain in the region of the stomach, slight increase of thirst, sensation of weakness, loss of flesh, and a dry state of the skin. The bowels are also irregular, with a feeling of uneasiness over the lumbar region. These symptoms are generally attributed to dyspepsia, and treated accordingly. In place, however, of yielding to the adopted treatment, they become too aggravated to be mistaken. The urine passed in 24 hours varies from 7 to 40 pints (the average in the cases I have treated is 15), is usually colorless, although in some cases of a brownish tinge, and is sweetish to the smell as well as taste. Its specific gravity ranges from 1012 to 1059, the average being 1033. In my experiments I have observed an increased deposit by the action of the chloride of barium; thus accounting for the sulphur in the breaking up of the tissues, and for the emaciation, which in many cases is very rapid. I have known a patient in three months lose in weight nearly four stones. In all the cases that have come under my observation, I have always found urea present in the urine, and occasionally the lozenge-shaped crystals of uric acid, which, with Prout, I think a favorable sign, yet not to be depended upon. These may be present in abnormal quantities, along with a large amount of sugar. Dr. M'Gregor has shown that urea is present in increased quantities. In one case he found 1018 grs. passed in 24 hours, in another 945, in a third 810, and in a fourth 512. The quantity excreted in health varies from 362 to 420; nitrogen then is expelled in greater quantities than in health; hippuric acid may also be detected. I have not been able to detect albumen in this fluid in the early stage; and only in two cases, one of which terminated fatally. Thirst is invariably present, in many cases to an excessive degree, and is one of the most distressing symptoms. The mouth is clammy; the tongue, although generally clean, is often red and dry; the gums most usually spongy, and the saliva either sweet or acid to the taste. But on waking from sleep, the taste in the mouth is most disagreeable. From this parched state of the mouth and tongue, the articulation becomes peculiarly indistinct. While in no case have I myself seen what may be termed a voracious appetite, in very many it has been weaker than that of a person in the enjoyment of perfect health. Constipation is a very frequent symptom; the skin is dry and shrivelled, with a disagreeable sensation of heat in the soles of the feet and palms of the hands; and in a few cases the perspiration has been profuse; usually there is pain and uneasiness in the region of the stomach, extending to the liver and kidneys. The debility in many cases is extreme. Many suffer from

severe pains in the calves and thighs. The mental faculties become blunted, the features shrunk and shrivelled, and the whole man wears a woe-begone and hopeless aspect. The sexual appetite is weakened, but the desire and power never entirely absent. It has been remarked, that in many cases of glucosuria the egesta exceed the ingesta, although there may be at the same time no diminution of weight. This by some has been referred to the wasting of the body; while it has been accounted for by others from the absorption of moisture from the atmosphere by the skin and lungs. This phenomenon, however, much depends upon the treatment adopted.

The tests which I employ in the detection of sugar are Trommer's, Moore's, the fermentation test, and that afforded by the growth of the torula. For all ordinary purposes, Moore's answers admirably, the density of the color indicating the amount of sugar; where a doubt exists, each test may be applied in turn. It ought to be borne in mind that ammonia prevents the sugar from being detected in the sulphate of copper test; and that, in the yeast test, carbonic acid, and even sugar may be present in small quantities in the agent itself.

Christison's formula for detecting the solid constituents in diabetic urine is simple. He takes the specific gravity, supposing it to be 10.35; he multiplies the decimal 35 by 2.33, which gives the required amount. In order to ascertain the amount of sugar in any given quantity of diabetic urine, the sure mode is to treat it with yeast, and calculate it by the volume of carbonic acid given off.

In testing for sugar in the blood, a simple process is, to evaporate the serum to dryness, pulverise the residuum, add boiling water, filter, and apply the usual tests to the filtered fluid.

With regard to the causes of this disease, in none of my cases could I trace them to a hereditary disposition. I have experienced the greatest difficulty in getting the patient to account for his complaint. While in general no cause predisposing or exciting could be assigned, I have succeeded in five cases in tracing the first symptoms to an indulgence in cheese in a state of putrefaction, and of skimmed milk. Indeed, it is a singular fact, that more than three-fourths of my cases were, more or less, directly connected with agricultural pursuits.

The proximate cause—I incline to the opinion of Bouchardat—depends upon a peculiar substance, which he has found in the stomach, and has demonstrated to resemble diastase in its action on starch. If I am correct in the view that I have taken in these five cases, that a ferment was introduced into the stomach, then the disease can be easily accounted for. I am not as yet able to prove that this diastase is first introduced into the body, and there increases. The pathological appearance of the different organs of the body is not such as to enable us to locate the disease in any individual one. The brain, lungs, liver, stomach, and kidneys, may severally be found diseased, while, on the other hand, no pathological appearance may be presented in any one. The peculiar appearance and consistence of the blood, however, is more regular and frequent. The blood drawn from a diabetic patient was found by Watt, "of a soft black substance, not unlike pitch in

appearance, but devoid of tenacity;" at a second bleeding "it was extremely dark, and so devoid of tenacity that it could not be elevated with a probe above the surface of the serum." This state of blood has been remarked by Dr. Satterley and others, and I have also found it, in such cases as I have examined that fluid.

In a return given me by the Registrar-General, of the annual number of deaths in England and Wales, from diabetes, 1847 to 1853, I find them as follows:—

1847	.	.	.	.	.	.	.	.	383*
1848	.	.	.	.	.	.	.	.	342
1849	.	.	.	.	.	.	.	.	416
1850	.	.	.	.	.	.	.	.	422
1851	.	.	.	.	.	.	.	.	403
1852	.	.	.	.	.	.	.	.	402
1853	.	.	.	.	.	.	.	.	412

We come now to the treatment of this subtle and formidable malady, which I believe to be curable, unless complicated with organic disease. It is now about four years since I first treated diabetes by rennet, during which period I have had 28 decided cases under my care. I can, therefore, speak with much greater confidence than when I first publicly advocated its curative properties, and in comparing my own with the treatment recommended by others, experience warrants me in pronouncing it superior to any yet adopted. I might not have been prevailed on to bring a paper on this subject before the society, were it not that my opinions and mode of treatment were either mis-stated, overlooked, or unknown. If the treatment be found useful (and it has been successful in other hands besides mine), why not adopt it, and give it a fair and faithful trial? I can, at least, certainly vouch for the safety of the test, and the innocuous nature of the remedy.

The rational basis on which I build my theory is that, if rennet out of the body converts a solution of sugar into lactic acid, it may possess a similar property within the body. Bearing in mind, too, that lactic acid is found in the juice of flesh, and, according to Liebig, is a supporter of the respiratory process, I conceived that if sugar formed in the body of a diabetic patient could be converted by the rennet into lactic acid, it would be burned in the lungs; and that, if a larger quantity were formed than could be consumed in that way, it would be excreted by the kidneys. In this I was not mistaken, for lactic acid was more than once detected in the urine of my first two patients. To say that rennet is an infallible remedy in any or every case of diabetes, would be overrating its virtues. Where this complaint is complicated with organic disease in any organ, the organic disease must be removed before we can look for a cure. It will, at least in such cases, ameliorate the symptoms, mitigate suffering, and tend to lengthen life—the greatest desiderata after an effectual cure.

(To be continued).

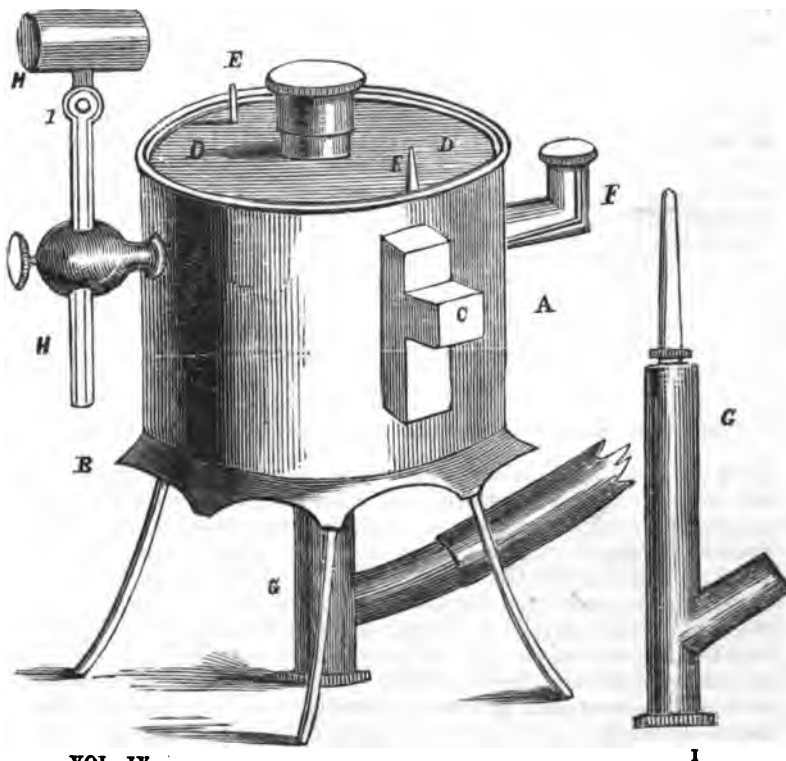
* These statistics show that the importance of this disease is graver than generally supposed.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On a NEW SPIRIT-BLOWPIPE. By THORNTON J. HERAPATH.

IN the *Mechanics' Magazine*, No. 1290, vol. xlviii. p. 416, a description has been given of a peculiar coal gas blowpipe that was invented by my father, Mr. William Herapath, some years ago. This blowpipe has not only proved of great assistance in the arts, in soldering, brazing, glass-blowing, &c., but has also been adopted by many analytical chemists in their laboratories. Having been for several years accustomed to the use of such an instrument, I naturally wished, on proceeding to Chili, to carry out one of the kind with me. On inquiry, however, I found that it would be impossible to make use of it, as coal gas is almost unknown on the coast, it being only manu-



factured at one city in the Republic, viz., Copiapo, a place nearly 800 miles distant from our establishment at Herradura. Finding it to be impossible to avail myself of the coal gas blowpipe, my next endeavor, of course, was to provide a substitute for it, this I at last accomplished by inventing the apparatus I am about to describe, which was manufactured under my directions by Mr. Braham, the well-known optician of Bristol. The apparatus, it will be perceived, consists of a circular wick-lamp, A, similar in construction to those termed Argand, by the trade; this is supported on a tripod stand, B, and furnished with two wooden handles, C. The wick of this lamp is so adjusted, as to be capable of being raised and lowered by a screw and movable cover, D, which may be turned either to the right or left by two small wire handles, E, E, whilst the lamp itself is fed by a small bent tube or feed pipe, F, placed on one side for convenience sake. Into the central draught hole of the lamp, the blowpipe, G, G, is fitted, and by properly adjusting the latter, that is to say, by either thrusting it further up, or by drawing it out as occasion requires, almost any degree of heat may be obtained. When the instrument is required to be used in the examination of mineral substances, &c., where a horizontal or downward blast is necessary, the operator has only to transfer the movable blowpipe to the side support, H, which is fitted with a joint at I, to enable the blowpipe to be inclined at any angle.

This blowpipe has proved so useful to me in this country in glass-blowing, mineral analysis, assaying, &c., and for many other purposes in laboratory practice, that I feel satisfied it will be found a most valuable assistant by every chemist who, like myself, is obliged to employ spirit as a substitute for coal gas. I need, therefore, offer no apology for publishing this brief description of it.

Herradura, Chili, South America,

July 18, 1856.

TRANSLATIONS AND ABSTRACTS.

BIOGRAPHY.

ROBERT BOYLE; a BIOGRAPHICAL STUDY. By M. CAP.

IN the midst of the gallery of scientific men of the sixteenth and seventeenth centuries, to which modern times owe the awakening of positive knowledge, we distinguish a man of uncommon merit and of peculiar physiognomy, now, perhaps, too little remembered, or at least too much neglected by the historians of science. Placed on the boundary which separates the scientific opinions of the middle ages from those which still prevail; successor of the eminent men who led the way to this great reform; of Galileo, whose genius still governs many of them; of Francis Bacon, whose theoretical views had hitherto remained without well pursued applications; of Van Helmont, the last of the idealists, and the first of rational chemists; the contemporary

of Descartes, Pascal, Otto von Guericke, Huyghens and Newton, to whose labors he joined his own in the work of regenerating the physical sciences. ROBERT BOYLE forms the true starting point of the immense and new career which, towards the close of the seventeenth century, began to be opened to human reason and knowledge.

From the end of the foregoing century, the symptoms of this reform had begun to manifest themselves in all directions. Two sciences, which were scarcely separated in the subject of their studies, and united almost everywhere in the definitive results of their researches, physics and chemistry, were forced to collect facts, and to establish principles which should serve as the foundation of their respective doctrines. The physical, natural, and even the medical sciences, so long devoted to the class of doubtful observations and false theories, were impatient to submit phenomena to a new examination, to reject preconceived opinions, to adopt a course which should permit them henceforward to advance on the path of realities. A new philosophy, founded on better processes of investigation and of reasoning, began to do justice to the traditions, to the false systems, to the wild conceptions of the previous age, and of that superannuated school, which since the twelfth century had obstructed all the avenues of what was then called philosophy. Robert Boyle appeared, and he soon became the central point to which all discoveries and rational explanations converged; he was thenceforth the superior guide, on whose track science would ultimately start forth.

His appearance was not surprising or brilliant; he did not attach his name to one of these bold conceptions which suddenly change the face and the bearing of human knowledge. His influence was only slowly founded, by a series of labors successively applied to almost all scientific questions. It was gradually, but continuously developed, thanks to the perseverance with which he accomplished the task which he had set himself. He was thus enabled to lay solidly the foundation of the vast edifice of which Bacon had traced the plan.* Let us hasten to add, that Robert Boyle was one of the first promoters of the foundation of learned societies, an event which forms one of the most important dates of modern history; finally, that his sincere devotion to intellectual progress is made manifest, not only by numerous works, but also by modesty, the most laudable self-abnegation, as well as by his great liberality to science and its votaries.

Robert Boyle is difficult to follow in his extended and varied career, because the whole of his works is wanting in a certain unity. Physicist, chemist, physician, physiologist, and theologian, he eludes, as a savant and as an individual, homogeneous and systematic appreciation. His incessant labors, but not pursued according to a determined line, were applied by turns to very different subjects. This existence of entire labor, application and devotion, presents few points of repose, and scarcely any remarkable event, calculated to vary that long and interesting catalogue of labors. All these reasons have pro-

* *Instauratio facienda ab imis fundamentis.* (Bacon de augment scientiarum).

bably deterred more than one writer from such a task. We do not commence it without some kind of apprehension. May we at least be known to have pleasure in having undertaken it, with no other view than the real usefulness of such a study, with no other object than a kind of seriously accomplished duty towards one of the men to whom the sciences are most indebted for discoveries, improvements and benefits.

Robert Boyle was born at Lismore, in Ireland, on the 25th of January, 1626. It is remarkable, that this date is also that of the death of Francis Bacon, and that Newton was born in the year in which Galileo died; as if Providence had not desired to leave the least gap in this genealogy of genius. Robert Boyle was the seventh and youngest son of Richard, Earl of Cork and Orrery, arch-treasurer and supreme judge of Ireland. His family was very ancient. His father had acquired, by his learning and integrity, considerable wealth and honors. His mother, who was of a very delicate constitution, having been unable to suckle, he was entrusted to a country nurse, whom the Earl of Cork desired to bring him up like her own son, for he disapproved the delicate rearing of cities. "Children are kept from the sun and the rain," said he, "as if they were made of butter and sugar." This mode of reasoning did not prevent Boyle from being throughout his life, of feeble and often bad health. At eight years of age, he was sent to Eton College. He was then intended for the church, but the weakness of his constitution and early infirmities, compelled his parents to give up this idea. At the age of eleven, his father sent him to one of his estates in Dorsetshire, where he was entrusted to the care of Dr. Douch, a friend of the family. The following year, he was sent to travel with one of his brothers and a French tutor, named Marcombes. He came to France, stopped at Geneva, passed through to Switzerland, and stayed some time in Italy. He was at Florence, when Galileo died in a village near that city (1642). He had arranged to return to England, when it was learned that a revolt had broken out in Ireland, which temporarily prevented him from returning to his country. He did not return to London until 1644.

These details respecting his early years were written by Boyle himself, but they were stopped on his return to England. He regarded it as one of his greatest misfortunes, not to have known his mother, who was likewise of high descent. He congratulates himself on having been born of such parents, as also on not having been the eldest of his family. "An inferior birth," he said, "would have deprived me of the advantages of fortune, without which I could not have followed my scientific instincts, and in the condition in which fate placed me, the obligations attached to the position of heir would have forced me to uphold the honor of my house at the sacrifice of my personal tastes."

Boyle had a lively and versatile taste, inclined to fantastic ideas. When only ten years old, at Eton, he had a very serious illness. During his convalescence, he read romances of chivalry, and, among others, *Amadis of Gaul*, which filled him with romantic ideas. To sober his imagination, he applied himself to the study of mathematics, but he remained for the rest of his life under the dominion of his

early impressions. They influenced his character, his turn of mind, and even his style, which, though simple and precise in matters of science, frequently took, in his works on philosophy, mystic, obscure and far-fetched forms.

It was reading Quintus Curtius that gave rise to his taste for sciences. He said that he owed more than Alexander to that historian, and that he had derived from the wars of the Macedonian hero more profit than himself.

Another event of his early youth had contributed to the religious and melancholy turn of his mind. While he was at College the floor of the room in which he slept fell in, and he was buried beneath the rubbish. It is known that Pascal was influenced in the same manner by the accident which befel him near the bridge of Neuilly. This is another example of those fatal shocks, to which the lively and delicate organisations which characterise men of genius are peculiarly sensitive.

When he passed through France on his way to Italy, Robert Boyle visited the Chartreuse of Grenoble. The aspect of these wild places, the austere and silent life of the monks, the ceremonies and the pictures which met his eye, all served to exalt a mind naturally serious and inclined to melancholy. He was agitated by involuntary terrors, and lived for a long time in this anxiety. In order the better to clear up the doubts with which he was assailed, he wished to read in the original text the works of the Fathers of the Church. He applied himself to the study of the Oriental languages, and especially of Hebrew, and put himself in communication with the first theologians of the period. Whence that long series of philosophical works which always proceeded at the same time as his scientific researches, but which we shall avoid here, and we shall consider only the works of the man of science.

On the death of his father, Boyle found himself in possession of a considerable fortune. Since the year 1645, he had collected about him a certain number of enlightened men, with the view of pursuing in common a series of experiments, of which he had settled the plan. It was a kind of Academy, whose meetings were held at Oxford, at the residence of a learned apothecary, named Cross. It was first called the *Society of Invisibles*, and afterwards took the name of *Philosophical College*. These meetings were suspended during the troubles which preceded the catastrophe of Charles I. During the dissensions between the Parliament and Royalty, Boyle, whose family was devoted to the Stuarts, retired to his estate of Stalbridge, in Dorsetshire. There, afar from political agitations, he devoted himself entirely to study, and he quitted this residence only after the death of Cromwell. The Philosophical College, which had adopted the bases of the Academy of the Lincei, of Florence, was then removed to London, where, under the protection of Charles II., it assumed the title of *Royal Society*. Consequently, the foundation of this illustrious Society (1663) is due to Boyle; it preceded by only three years that of our Academy of Sciences.

Boyle retired to Oxford in 1654. In the following year he was admitted Doctor of Medicine in the University of that city. He undertook anatomical researches, formed a friendship with the illustrious Sydenham, who dedicated to him one of his best works.* It was from this time that he began to put in execution the vast system of experiments which he had conceived, and which he regarded as best calculated for the advancement of the physical sciences. This was no other than the plan so broadly traced by Lord Bacon, and which Boyle had proposed at the commencement, to the investigation of the Philosophical College, as the special object of its labors. The fundamental idea of this enterprise was to work in concert at a complete restoration of the sciences, substituting for the scholastic hypotheses and argumentations, observation, experiment and logical induction, based on the greater number of comparisons and exclusions.

Boyle quitted Oxford in 1668, and went to pass the remainder of his life with his sister, Lady Ranelagh. In 1689, he lost several manuscripts, which were stolen from him or burned by an incendiary. This loss afflicted him deeply. In 1691, his sister, with whom he had spent twenty-three years, died, and Boyle survived her only eight days. Being naturally of a delicate constitution and subject to gravel from his infancy, he was sustained for some time only by means of a very strict regimen. He died at the age of sixty-five.

In the history of modern sciences, the advancement of physics precedes, by nearly a century, that of rational chemistry. The sublime impulse which the former science had received from Galileo, Descartes, Torricelli, and Pascal, was sustained by the labors of Dominis, Castelli, Merisienne, Gassendi, Kircher, and Roberval. However, the knowledge relative to atmospheric air did not begin to make any real progress, until it did so in the hands of the illustrious Burgmeister, of Magdeburg, Otto von Guericke, inventor of the pneumatic machine. This machine, even when brought to perfection, was still only composed of a glass balloon, furnished with a tubulure, to which was adjusted a stop-cock, screwed on to the body of a vertical pump, the piston of which was put in motion by a handle, with a horizontal arm; the whole supported by a tripod. Imperfect as was this apparatus, it served this learned person for executing a host of experiments of the highest interest. Otto von Guericke had remarked that, in introducing into his balloon an apparently empty bladder, it swelled up by the dilatation of the small quantity of air which it still contained, in proportion as the piston of the pump was agitated. He had concluded from this that it was the same with the lower portions of atmospheric air, which are compressed by the upper portions; so that then their density is proportional to the height of the atmosphere above them. He had shown that the air is the vehicle of sound, by placing in his apparatus a small carillon, set in motion by watch-work, the noise of which diminished in proportion as the vacuum was made. He had learned, by means of the same mechanism, that air is indis-

* *Methodus curandi febres, propriis observationibus superstructa* (1666.)

pensable to the respiration of animals, as well as to the combustion of bodies; for the former died, and the second was completely extinguished, when submitted to the action of his pneumatic pump.

Almost at the same moment, Torricelli, the pupil and successor of Galileo, resuming the experiments of his master on the gravity of the air, showed that liquids are sustained in the body of a pump only by making an equilibrium to the weight of the atmosphere, and that the height at which they stay is proportionate to their specific gravity. This discovery—which his early death prevented him from following to its consequences—gave rise to the barometer, an instrument of which Pascal availed himself with so much skill to show that, by this means, we may accurately measure atmospheric pressure, and, consequently, heights beyond a given level.

The researches on the weight of the air, on the vacuum, and on atmospheric pressure were at this point, when Boyle took up the subject. This was not only his starting point as a physicist, but it may be said that the study of the air never ceased, during his life, to occupy his mind. Although, perhaps, no man of science has varied more than he did the subjects of his labors, he constantly recurred to this one; and, among his numerous works on physics, the greater part relate to the study of this fluid. *The General History of the Air* (London, 1692, in 4to.) was his last work, and, as if he must even be occupied in the same way beyond the tomb, this history did not appear until after his death. He considered air respectively in physical, chemical, and physiological points of view. No one had made a greater progress than he had in the knowledge of the air, which, in his time, was still ranked in the number of elementary bodies.

It may be conceived that Boyle applied himself above all to the improvement of Otto von Guericke's machine, which was to become the principal instrument of his intended researches. This was, indeed, the prelude of his works, and that by which he introduced himself to the learned world, (1658). This machine had still numerous inconveniences. For the glass balloon adjusted to a vertical pump, he substituted the plate, as well as the pneumatic receiver, and he separated the pump from it, which he made to communicate with the receiver by means of a bent tube. The double pump, whose piston is put in motion by an ingenious system of cog-wheels, appears to have been devised later by Papin. The apparatus thus modified, Boyle resumed, in new conditions, Otto von Guericke's experiments, and rendered them more evident. That of the bladder which swelled up when the vacuum was made, served to demonstrate the elasticity of the air, which he compared to that of wool, which springs up again, as soon as pressure is withdrawn. He proved the influence of the air on the propagation of sound, on the motion of the pendulum, on the life of animals, reptiles, insects, and even of fish; for, in making the vacuum in a receiver which contained water, he extracted from it bubbles of air. Finally, he remarked the pressure which that atmosphere exerts on a bell-glass in which a vacuum has been made, and that it cannot then be raised.

In order better to study this series of phenomena, Boyle, proceeding

by the negative way, applied himself particularly to the study of the vacuum. After having exhausted a receiver of air, he showed not only that combustion could no longer take place in it, but also that the volume of air in which bodies in the state of ignition had been placed also diminished to a determinate point. He first remarked that warm water placed in the receiver boiled at a lower temperature, in proportion as the atmospheric pressure was diminished. He proved that putrefaction and fermentation are not set up in vacuo, and are accelerated in compressed air.

These experiments would lead to the study of the dilatability and compressibility of air. He succeeded, by means of the same apparatus, in compressing this fluid to such an extent as to make it occupy one-twentieth of its volume under the ordinary pressure, and he remarked that it could be dilated to such an extent as to occupy a space 13,769 times as great as that which it fills in ordinary conditions.

Although most of these researches were the consequence of Otto Von Guericke's discovery, they acquired in Boyle's hands a new development, and a greater degree of certainty; thus the studies on the vacuum remained Boyle's own, and the pneumatic machine thus improved was for a long time called by his name (*Vacuum Boyleanum*.)

All this collection of researches and experiments formed the subject of his first work,* which appeared in 1660. Linus and Hobbes raised, it is true, some objections to his experiments and the conclusions which he had drawn from them; but Boyle combated them in a second edition of his work, published two years later.† In this he establishes incontestably that the spring of the air is in the inverse ratio of the pressure, by showing that air condensed into one-fourth of its ordinary bulk, sustains a quadruple weight, and that when it is dilated so as to occupy thirty-two times its ordinary space, it sustains only one-thirty-second of the weight. Afterwards,‡ he established no less evidently, that the spring of the air made an equilibrium with the weight of the atmosphere. He proved the evaporation of liquids in vacuo, thus preparing for Mariotte the facts on which that *savant* established the law of evaporation. Boyle explained the phenomena of suction, of the syphon, of capillarity, and of the propagation of sound; he calculated the probable height of the atmosphere; he compared the density of the air with that of water, and the cypher at which he stopped was very near the truth. He established the boiling-point of various liquids in rarified air, the ordinary density of atmospheric air, and recognised the effects of the intervention of air in several phenomena of coloration.§

It was with relation to this latter subject, and in the same work, that he indicates for the first time the action of acids and alkalis on vegetable blues.

(To be continued.)

* *New Physico-mechanical Experiments touching the Elasticity of the Air, and its Effects*, &c. Oxford, 1660.

† London: 1662, in 4to.

‡ *Continuation of New Physico-mechanical Experiments*, &c. London: 1682.

§ *Experiments and Observations upon Colors*. London: 1663.

METALLURGY.

MEMOIR on the CHEMICAL COMPOSITION of the BRONZE STATUETTES found by M. MARIETTE, ASSISTANT CONSERVATOR of the EGYPTIAN ANTIQUITIES of the LOUVRE. BY M. CHEVREUL.

ARCHÆOLOGISTS now fully appreciate the consequences of the discovery of the Egyptian Serapeum, made by M. Mariette, during a stay of four years in Egypt. They know that to his persevering and enlightened zeal we owe monuments, inscriptions, and objects of all sorts. Among those with which he has enriched the collection of the Louvre, there is a multitude of bronze statuettes, which were found at a depth of a few inches under the pavement, and in the foundations of all parts of the temple.

It would appear that the Ancient Egyptians had buried these statuettes, with the intention of purifying the sand, which, according to their ideas, was impure, and which the feet of the people who repaired to the Serapeum incessantly crowded. M. Mariette brought to France hundreds of statuettes, varying in size from a few centimetres to 80 centimetres. They are covered with a *green layer* of variable thickness, under which there exists a *reddish matter*, of still more variable thickness; when the *green layer* and the *reddish matter* do not constitute the whole statuette, there is found under the latter, a matter capable of presenting the metallic appearance of bronze, when rubbed with a smooth file. The sand which covered these statuettes was identical, M. Mariette informs us, with that of the desert. Grains of sand adhere, in many places, to the green layer. Several statuettes form, with the sand which envelopes them, a kind of marble, because the grains of this sand have been agglutinated by a calcareous cement, as I shall show further on.

During the fifteen years in which I have collected facts concerning the arts of ancient peoples, in order to constitute the basis of my first volume of *Considerations Générales sur l'histoire de la Chimie*, I have analysed a great number of objects connected with them, and particularly colored matters, collected from the monuments of Thebes by the late M. Lefevre, whom Mehemed Ali had instructed to direct the investigations concerning the useful mineral matters of Egypt. Having had the advantage of examining the objects brought to this country by M. Mariette, with M. Mariette himself, and having witnessed the processes practiced at the Louvre for putting the statuettes of the Serapeum into a condition for exhibition to the public in the gallery allotted to them, which cannot be better understood, I felt a desire to examine some of these statuettes chemically; and here I must express my thanks to Viscount de Rougé, honorary conservator of the Egyptian collection of the Louvre, who authorised M. Mariette to furnish me with specimens adapted for chemical examination. It must be borne in mind that these specimens were taken from among remains so dis-

figured and altered as only to be fit to be thrown away. This was the case with every sample of every matter treated of in this memoir.

The ancient Egyptians made two kinds of statuettes; some were hollow, and the others solid.

§ I.—*Hollow Statuettes.*

Those which I examined were filled with a pulverulent matter, of earthy appearance, whitish or colored; I found in them also a matter of a very different appearance.

Matter of Earthy Appearance.—A white matter, of earthy appearance, yielded to water traces of common salt and chloride of calcium. The residue was subcarbonate of lime, mixed with a little clay, which contained a small proportion of organic matter; grains of sand remained after the action of nitric acid.

Colored Matter of Earthy Appearance.—It yielded to water only a trace of matter analogous to the foregoing.

It yielded to nitric acid carbonate of lime mixed with alumina and binoxide of copper, and sesquioxide of iron.

There remained a black matter, consisting of silica, protoxide of iron, and alumina.

Matter which had been Fused.—This matter was formed in great part of litharge, which adhered firmly to the inner surface of the hollow statuettes in which I found it.

A specimen of this matter, which had been fused, presented a compound of litharge and subcarbonate of lead.

Gold Statuettes.—The Egyptians applied gold to hollow statuettes by two processes: 1st, by means of damaskeening: 2ndly, by means of a white matter, serving as a stucco to the bronze, to which gold leaf would adhere.

This stucco was not plaster, as some persons have thought, but a very pure and finely-divided carbonate of lime, to which was probably added an organic matter, of which there still remained a perceptible trace.

Composition of Hollow and Solid Statuettes.—The hollow and solid statuettes which I examined were essentially formed of an excellent bronze. This alloy was presented to my examination in four states:—

1. In the state of a green layer, some portions of which were blue.
2. In the state of a matter of a blood-red color.
3. In the state of a reddish bronze.
4. In the state of ordinary bronze which had not undergone any alteration.

1. Green layer, some parts of which were Blue.—This matter presented the greatest alteration of the bronze; it formed the external layer of the statuettes.

It was essentially formed of a definite compound equivalent to bichloride of copper, binoxide of the same metal, and water; in a word, it was the composition of the oxychloride of copper of Peru.

The blue portions were formed of water, carbonic acid and binoxide of copper; it was the blue hydrated carbonate of copper.

It is unnecessary to state, that binoxide of tin was mixed with the green matter.

2. Blood-red Matter.—This was essentially formed of protoxide of copper, mixed with binoxide of tin.

The solution of this matter in nitric acid contained chlorine.

3. Reddish Bronze.—I thought that I noticed, in comparing different specimens of the remains of statuettes, that parts which were filed and which had retained the metallic state, had not all the golden color of the bronze—that some of them had a reddish color, which seemed to show, that proportionally to the tin, the latter was more altered than the copper.

4. Unaltered Bronze.—This bronze appeared to me remarkable for its good quality.

Revival of an almost completely altered Statuette.—A curious experiment proved to me the possibility of reviving altered bronze.

I took a statuette of Anubis—which might have been about 8 centimetres in length, when it was entire—and introduced it into a porcelain tube. I filled the apparatus, while cold, with hydrogen gas, and then raised the temperature to dull red. I then saw condense in a bell-glass water, colored green, with hydrochlorate of binoxide of copper. I cooled the apparatus, and withdrew the statuette completely revived.

The sand in which M. Mariette found the statuettes which I have examined gave me chloride, soluble in water, and, on the other hand, portions of sand were aggregated by a calcareous cement.

Conclusion.

It appears to me indubitable, that atmospheric air and salt water containing lime and carbonic acid have aided in producing the alteration of the bronze statuettes which I have just examined.

The copper and tin are altered from the exterior to the interior, and the most completely altered layer was that at the surface, which was of a green color.

The tin passed to the state of peroxide; the copper of the superficial layer was converted into bichloride and hydrated binoxide, which are combined together; finally, the bronze immediately in contact with the external layer had passed to the state of protoxide of copper and peroxide of tin.

Some of the statuettes, or parts of statuettes, were completely converted into oxides; in others, the bronze had been altered only superficially; the first layer—always green—concealed a thin layer of protoxide mixed with peroxide of tin, under which the bronze was completely preserved from alteration. This layer of protoxide resembles in its appearance the vessels and ingots of copper from China and Japan, which are covered with a layer of protoxide of copper.

Comptes Rendus, No. 16, Oct 20th, 1856.

On the ALLOYS of ALUMINIUM.

THE alloy of aluminium and sodium easily decomposes water, its production was, at the commencement of the manufacture of aluminium, a considerable source of loss, but this is now prevented by a better combination of processes, and it is, moreover, easy to purify aluminium thus combined, so as to render it perfectly unattackable by water.

For practical purposes, it is not necessary that aluminium should be completely free from iron. I have analysed metal, reduced from impure chlorides, whose malleability and tenacity differed but little from that of pure aluminium, and which, nevertheless, contained 7 or 8 per cent. of iron. The union of the two metals is very easily effected; the iron stirrers used for the liquid baths in the furnaces, in which aluminium is produced, become covered with a brilliant layer of that metal, which produces on their surface a phenomenon similar to that of tinning. The aluminium alloyed with iron, is as easily purified as iodised aluminium, by melting it in nitrate of potassa.

I have alloyed small proportions of aluminium with iron (5 of aluminium to 95 of iron), without communicating to it any properties which differed much from its own. Tempering it, did not modify it. Aluminium readily combines with zinc, giving alloys, the most interesting of which contains 97 of aluminium, and 3 of zinc. This alloy, which is a little harder than the pure metal, is, however, very malleable; it is quite as brilliant as any other alloy of aluminium.

Aluminium may contain as much as 10 per cent. of copper, without completely losing its malleability. The metal prepared in copper boats, contained 5 or 6 per cent., it was easily worked. Beyond 10 per cent. it loses its malleability; 80 per cent. of copper may be added to aluminium without rendering the color yellow; the alloy thus obtained resembles that of the mirrors of telescopes, it is very brittle. The alloy of 15 per cent. of aluminium is, likewise, very brittle, but it has a slightly yellow color, and it is probable that copper loses its color when its proportion is less than 18 per cent., the proportion corresponding to $\text{Cu}^2 \text{Al}$.

The bronze of aluminium, whose resistance is to that of bronze, in the proportion of 51 to 49 contains 10 of aluminium, and 90 of copper; it joins to the property of being malleable, when hot, a great unalterability in the presence of hydrosulphate of ammonia. Its yellow color is very beautiful, but with respect to brilliancy, it is superior to the alloy of 95 of copper and 5 of aluminium.

The alloy composed of three parts of silver, and 97 of aluminium has a beautiful color; it is unattackable by sulphuretted hydrogen. By augmenting the proportion of silver we add to its hardness.

The alloy of 1 per cent. of aluminium, and 99 of gold, has the color of green gold; it is very hard although very malleable. That composed of 10 per cent. of aluminium and 90 of gold, is white and crystallised, and consequently brittle.

Aluminium and Silicium.—As M. Deville truly observes, silicium is far from injuring the malleability of aluminium like iron or copper.

A sample containing 10 per cent. of silicium, although difficult to work, may be used in the manufacture of many articles.

Aluminium and Iron.—Small quantities of iron communicate to aluminium the property of crystallising, and consequently destroy its malleability. Iron raises the fusing point of aluminium. We have fused pure aluminium on aluminium containing 4 or 5 per cent. of iron.

Aluminium and Zinc.—100 grammes of zinc with 10 of aluminium give a brittle alloy, having the appearance of zinc, more fusible than aluminium, but less fusible than zinc.

An alloy composed of 100 grammes of zinc and 25 of aluminium has a firm close grain, brittle, more fusible than aluminium, less fusible than zinc.

The alloy of 100 of aluminium and 50 of zinc does not appear to be homogeneous; when heated on a plate of aluminium, it separates into one portion fusible, and another portion which does not fuse before the aluminium plate itself.

These are the alloys of aluminium and zinc which succeed best as solders, but they do not bear the hammer well.

Aluminium and Nickel.—The action of nickel on aluminium has considerable analogy to that of iron; like this last metal, nickel produces with aluminium crystallised alloys; and when well managed it gives certain qualities to it, such as density, elasticity, &c.

Aluminium and Tin.—100 parts of aluminium and 3 parts of tin give a brittle alloy, a little more fusible than aluminium.

If tin, even in small quantity, injures the qualities of aluminium, this latter metal may on the contrary give to tin the hardness and tenacity in which it is wanting, provided that it is not introduced in too large proportions; 5 parts of aluminium to 100 parts of tin are sufficient.

In the same way as the alloys of zinc, the alloys of tin and aluminium may be employed to solder that metal, because of their fusibility, and the ease with which they adhere to the scoured surface; but they have the same inconvenience as the solder with a zinc base, namely, a fatty consistence, and a fragility which causes them to break under the hammer.

Aluminium and Lead.—Aluminium and lead have so little tendency to combine, that the smallest traces of lead introduced into the aluminium will always be found at the bottom of the button of this latter.

Aluminium and Antimony.—Aluminium has as little affinity with this metal as with lead.

Aluminium and Bismuth.—With bismuth the combination is made with ease, and produces very fusible alloys, but which become very quickly oxidised when fused. They are very alterable in the air at the ordinary temperature, when the proportion of bismuth is considerable. They do not appear to form alloys in all proportions.

The alloy of 100 of aluminium and 10 of bismuth is very hard, very malleable, taking a fine polish, unattackable by nitric acid, and

not blackened by sulphuretted hydrogen ; but it is subject to crack in all directions.

Aluminium and Copper.—The alloys of aluminium and copper are those which are most thought of. From the commencement of his experiments on this metal, M. Deville obtained alloys of aluminium and copper which were light, hard, and white, capable of taking a high polish. Very small proportions of copper are sufficient to destroy a great part of the malleability of the aluminium, giving it a blueish tinge, and rendering it capable of being blackened by the air.

Aluminium in small proportions augments the hardness of copper without producing much effect on its malleability, renders it susceptible of a high polish, and, according to the proportion used, causes the color to vary from that of pale gold to that of red gold.

We have prepared a great many of these alloys, and we must say that they leave nothing to be desired as to brilliancy and color as an imitation of gold. Fusion has much less effect upon them than upon the alloys of copper with zinc or tin, used for the same purpose.

By combining 100 of copper with 10 of aluminium, we obtain a harder alloy than that used in the manufacture of money ; it takes a brilliant polish, and resembles in color the pale gold of the jewellers ; it may be hammered and worked exactly like copper.

The alloy of 100 of copper with 5 of aluminium, is not so hard as the preceding, likewise susceptible of a fine polish, and approaching more nearly to pure gold in its tint.

The resistance, hardness, and elasticity, which can be communicated to the copper, by introducing into it small quantities of aluminium, will certainly bring these alloys into manufacture.

Aluminium and Silver.—Silver is the metal which appears the most suitable for modifying advantageously the properties of aluminium.

Five parts of silver added to 100 of aluminium, give to this latter metal the elasticity in which it is in itself deficient, communicate hardness to it, and consequently the faculty of polishing without injury to its malleability.

All the alloys of silver, up to the proportion of 50 of silver to 100 of aluminium, are more fusible than aluminium ; the fusibility increases with the proportion of silver.

The alloy which contains equal proportions of aluminium and silver is as brittle as that of copper.

By introducing into silver 5 per cent. of aluminium, we communicate to it the hardness of monetary alloy. This alloy is readily distinguishable from that containing copper by a test of aqua-fortis, which turns it white instead of black.

Aluminium and Gold.—Gold is the metal of which aluminium bears the greatest proportion without injury to its ductility.

As to the modifications produced on gold by small proportions of aluminium, we may form an idea of them by saying that 5 parts of the latter added to 100 of the former, render it white, and as brittle as glass.

Aluminium and Platinum.—Aluminium combines very readily

with platinum, and forms with it an alloy whose fusibility depends on the proportion of aluminium.

Five parts of platinum and a hundred of aluminium, produce an alloy which is not sufficiently malleable to be worked; it is probable that by diminishing the proportion of platinum a good alloy might be produced. As for the color, this alloy appears to approach that of the alloy of 5 per cent. of silver.

Cosmos, Nov. 14th, 1856.

INDUSTRIAL MANUFACTURE OF ALUMINIUM.

M. DUMAS presented to the Academy several kilogrammes of metallic aluminium, obtained by MM. H. Sainte-Claire Deville, Rousseau, and Morin, by means of manufacturing processes; he added the following remarks:—

These processes have been for more than a year the object of persevering study. They have received the improvements necessary to remove them from the domain of science into that of manufacture, and to leave them in the hands of common workmen. Now, after several months' experience, as I have myself ascertained, the processes, without any essential modifications, have worked without difficulty, and it appears that science has concluded her part in the preparation of metallic aluminium, and that of industry has commenced.

The means now in use differ but little apparently from those employed when the researches on aluminium were begun; chloride of aluminium has to be prepared, and to be decomposed with sodium so as to leave the aluminium free.

But the methods by which these two matters are obtained, and the apparatus in which they are made to react on each other have received, from necessity, the modifications necessary for passing from the practice of the laboratory to that of the manufactory.

When the alumina is extracted from ammonia alum, it is decomposed in a reverberatory furnace, and this substance remains then in a state perfectly adapted for its conversion into chloride.

It has been ascertained, that this chloride may be produced by the direct employment of kaolin, or even of argil.

This is not all: the chloride of aluminium was difficult to deal with on the large scale, because, after having been formed into vapor, it condenses suddenly into snowy crystals. It was necessary to collect it in chambers and to detach it mechanically from their sides, which involved: 1st, loss of chloride, the condensation being incomplete; 2nd, danger to the workmen exposed to respiring its vapors; 3rd, increase of expense, on account of the disconnectedness of the operations.

By submitting to the action of chlorine, instead of alumina and charcoal, a mixture of alumina, common salt and charcoal, we ob-

tained a volatile and liquifiable double chloride of aluminium and sodium, as liquid as water, and congealing on cooling.

Its preparation is continuous, it proceeds with simplicity and regularity, like a distillation; it requires no other care than that necessary for the production of chlorine, the renewal of the mixture to be decomposed and the replacing, at the extremity of the refrigerator, of the earthen pots in which are formed the cakes of double chloride, which flows into them in a continuous stream.

The operation has therefore a manufacturing character.

It is the same with the extraction of sodium. I have seen this metal, prepared by the process of Gay Lussac and Thénard, at an expense of seven francs the gramme 20 years ago. As it requires at least three kilogrammes to produce one of aluminium, it would then have cost for this item 21,000 francs for its extraction. Now, the expense of extracting sodium, scarcely exceeds seven francs per kilogramme. This extraction, which is more easy than that of phosphorus and which is comparable to that of zinc, is effected with a simplicity which justly astonishes all who witness the operation for the first time, and who remember the difficulties formerly attending it. In acting on a mixture of carbonate of soda, charcoal and chalk, the reaction is so complete, that the actual yield of sodium agrees with that indicated by calculation, and it is so easy that the expensive iron bottles, which were ordinarily employed, may be replaced by luted earthen tubes.

Finally, after many costly and tedious experiments, the employment of the reverberatory furnace was decided on for the action on each other of the sodium and the double chloride. Nothing is more curious certainly, or more worthy of being exhibited to our respected colleague M. Thénard, than to see an incandescent reverberatory furnace charged by means of a shovel, with a mixture of sodium in pieces and double chloride, and to observe that the reaction between these two bodies, which commences only after some time, is so tranquil that it may be effected without danger, on a large scale.

It leaves aluminium in plates, in globules, or in powder. It is separated from the common salt, either mechanically or by means of water.

The cost of aluminium thus produced, if it were not affected by accidental expenses, would be below 100 francs the kilogramme; it is easy to ascertain the expenses of manufacture.

Indeed, when produced from ammonia alum, alumina is too dear; hydrochloric acid costs much more at Paris than it is really worth at the places of its production; carbonate of soda is in the same position.

The duty on common salt weighs heavily on the cost of aluminium in three different ways, for it raises the price of the carbonate of soda necessary for the production of sodium, that of the hydrochloric acid employed for the production of the chlorine, and finally, that of the common salt itself which enters into the double chloride.

Indeed, in working on the large scale, we should find, with the exception of unavoidable losses, in the products extracted from the

reverberatory furnace, a quantity of common salt corresponding to that which entered into the double chloride, and to that from which the sodium itself was produced.

At present, in the experimental manufactory whose operations I have analysed, all those improvements of detail which would reduce cost not being practicable, we must expect to see the price of aluminium remain some time longer higher than it ought to be.

But the production of this metal is now effected by simple and regular processes, which no longer require the supervision of the chemist, and which a workman may conduct; the manufactory produces two kilogrammes of aluminium per day, and this quantity may be increased at will by increasing the number of the apparatus. Under these circumstances, M. H. Sainte-Claire Deville considers his task accomplished, and leaves to the manufacturing world the processes which he has devised.

Comptes Rendus, No 15, October 18th, 1856.

ORGANIC CHEMISTRY.

INVESTIGATIONS on the CHEMICAL NATURE and PROPERTIES of the FATTY MATTERS contained in the BILE.

By M. GOBLEY.

THE numerous researches of which the bile has been the object, have produced many interesting and unexpected results. After having passed through a period when it was stated to be composed of the most various and complex elements, it is now considered as a body which is chemically very simple. May this simplicity not be exaggerated, or, at least, have we not stopped too soon, and have we studied with sufficient care the state in which its constituent principles exist in it?

In a physiological point of view, it would be very interesting to test the experiments which have been already made, to try new ones, and thus clear up the obscurity which still surrounds these investigations. This obscurity is but too evident from the various opinions which men of the highest attainments have formed respecting the bile. Is it a purely excrementitious liquid? Is it of any use during digestion? Such are the questions which still require answering. Chemical analysis must be the basis of any answer, and I therefore now publish some remarks which I have made upon the fatty matters contained by the bile.

I have nothing to do in this case with the general characteristics of this liquid. Its color, consistence, odor and alkalinity, its property of not coagulating with heat, of becoming frothy by agitation, and of changing color under the influence of nitric acid, are all too well known to require any notice. What I wish to ascertain is, whether the state in which we find the fatty matters contained in it, has been

properly appreciated. In the contrary case, I will endeavor to show their nature more distinctly.

The fatty matter of the bile has hitherto been supposed to consist of cholesterine and fatty acids in combination with soda. The experiments which I am about to describe were undertaken for the purpose of confirming this assertion. They were all performed with perfectly fresh bile.

First Experiment.—When ox bile is agitated with sulphuric ether, the liquid which rises to the surface, leaves, by evaporation, a solid residue of a greenish yellow, with a strong odor, formed of cholesterine, a small quantity of oleine, margarine, and coloring matter. However the washing may be prolonged, no other matters are separated.

Second Experiment.—Neutral acetate of lead and chloride of barium cause, in bile, a deposit which contains no trace of fatty matter.

Third Experiment.—When acetic or nitric acid are added to bile, no fatty matter is separated even when the temperature is raised to the boiling point. We obtain only cholesterine and the coloring matter when we agitate the mixture with ether, as if the bile had received no addition of an acid body.

Fourth Experiment.—Bile put in contact with sulphuric acid diluted with water, yields no fatty body at the ordinary temperature, but by subjecting the mixture to heat, two distinct layers are formed; the inferior, liquid, of a yellowish or a brownish color; the upper one, thick and of a soft consistence, which, when put in contact with ether, yields to the latter all the fatty acids which the bile can furnish, and whose proportion is about 0.70 per cent. If, instead of sulphuric acid, hydrochloric acid is employed, these two layers are not formed; the mixture still parts with oleic and margarinic acids when mixed with ether. Both sulphuric and hydrochloric acids separate the fatty acids of the bile; nevertheless, they act in very different manners on that body, as we see.

Fifth Experiment.—Two hundred grammes of bile, with the addition of 10 grammes of potassa, gave, after several hours' contact at the ordinary temperature, the fatty acids when treated with acetic acid and ether.

What conclusion must be drawn from these experiments? If oleic and margarinic acids pre-existed in the bile, they would have been precipitated by neutral acetate of lead and chloride of barium, or separated by acetic and tartaric acids. Now, as they were not, they must be secondary products. The action which the vegetable acids have on the bile, as well as the mineral acids, and alkalies, does not permit me to doubt that these products result from the decomposition of the *lecithine*. This substance, whose characters I have indicated, and whose existence in bile had not been previously mentioned, would, therefore, be one of the constituent principles of this liquid. There only remained, to prove its existence formally, that I should demonstrate the presence of phosphoglyceric or phosphoric acid in the pro-

duct of the evaporation of the bile. For this purpose it was necessary first to eliminate the phosphates which it contains. After having evaporated bile on a sand bath to dryness, I dissolved 170 grammes of dry residue, representing 1880 grammes of liquid bile, in a sufficient quantity of absolute alcohol, and I filtered the liquor. I evaporated this solution again, and again redissolved the residue in absolute alcohol. By filtration and evaporation I obtained the bile free, if not from all, at least from the greater part of its alkaline and earthy phosphates.

The bile, thus prepared, was decomposed by water acidulated by means of sulphuric acid, and the reaction was aided by a slight elevation of temperature. Two layers were formed in the liquid; the upper one, which was thick, was removed; the lower one, which was aqueous, was evaporated to dryness and the residue carbonised; then taken up again by distilled water, it gave an acid liquor, which, after being saturated by carbonate of soda, and acidulated with acetic acid, was mixed with acetate of peroxide of iron. A very abundant precipitate of phosphate of sesquioxide of iron was formed, which, when washed, dried, and weighed, represented a quantity of phosphoric acid evidently corresponding to that of the fatty acids constituting lecithine.

Thence results the fact which I have announced, that the fatty acids removable from the bile, do not originally exist in it, and that they are products of decomposition. The following experiments will be found to support this view.

When bile is left to itself for a few days, it diffuses an unpleasant odor, and shows an acid reaction. When agitated with ether, the latter, when decanted, gives by evaporation, fatty acids and cholesterine, whereas I have proved that, in the same conditions, fresh bile only furnishes fixed fat besides cholesterine and the coloring matter. When instead of leaving the bile to itself as we have said, we add previously a small quantity of sulphuric acid, it gives no odor for eight or ten days, and we see on the surface of the liquid whitish points, which, when separated by means of ether, prove to be nothing but a mixture of the fatty acids.

After having found that among the fatty matters of the bile, were to be reckoned oleine, margarine, cholesterine, and lecithine, I endeavored to ascertain what became of those substances when the bile is diffused through the intestine. Are they absorbed by that organ, or do they remain mixed with the excrementitious matters, and, in this last case, should we find them intact, or are they in any way transformed? To ascertain the phenomena which take place in these circumstances, I separated from ox dung, by means of ether and alcohol, all the grease which it contained. The examination of this latter substance, which I have always found in but small quantity, proved to me that it was neither acid nor alkaline, that it contained neither lecithine nor its products of decomposition, and that there was only a small quantity of fixed fatty matter, and an infinitesimal quantity of cholesterine.

The substances of the bile are, consequently, for the greater part, absorbed by the intestine with the alimentary matter.

On the other hand, when we estimate the amount of fat existing in the blood at its entrance into the liver by the vena porta, and its exit from that organ by the sub-hepatic veins, we find that in the last case it only contains about three-fourths of what was found in it previously. Experiment demonstrates that a portion of this substance may be deposited in this organ. I have now shown that another portion escapes by means of the bile in the intestine.

Now many physiologists have established that neither the digestion nor absorption of the proteic and amylaceous matters are stopped when the bile does not pass into the intestine. But will any one maintain that the fatty matters which it contains, and which do not pass away with the fecal matters, do not aid in nutrition?

When speaking of these various facts, I ask myself if, besides its already mentioned physiological actions, the liver may not still possess another?

The powerful absorption of the venous radicles draws into the blood of the vena porta, considerable quantities of fatty matters, and may not the liver combat the effects of this mistake of locality, and render to the digestion the alimentary substances whose loss would appear, according to MM. Blondlot, Arnold, Nasse, and others, very prejudicial to the nutrition of the animal?

The fatty matters contained in the bile are of the greatest importance to the organism, because they consist almost entirely of cholesterine and lecithine, products which are diffused throughout the system, and there may likewise be cerebrine; and when a fistula takes them away, an animal requires much more food to replace these substances. Whence it soon perishes unless this reparation is aided by the ingestion of abundant quantities of aliments.

Should we not likewise find, in the necessity of a more complete and rapid elaboration of the fatty bodies returned by the liver to the digestion, the explanation of the approximation of the biliary and pancreatic ducts, and their common issue into the intestine.

But I am determined to confine myself to chemistry, and I leave to physiologists to examine into the value of these inductions which have been suggested to me by the comparative study of the chemical qualities of the bile, and the transformations of venous blood.

To sum up, from the facts in this note, it results:—

1. That oleic and margaric acids do not pre-exist in the bile as generally supposed.
2. That they are the products of the decomposition of lecithine under the influence of chemical agents, or of putrefaction.
3. That oleine, margarine, cholesterine, and especially lecithine, are in the number of the fatty bodies of the bile.
4. That the fatty matters of the bile, when diffused with this latter in the intestine, are almost entirely absorbed by that organ, since we find in the excrements only very minute quantities of fixed oil (oleine and margarine) and traces of cholesterine.

5. That it is difficult, consequently, to admit that the bile is useless, because the fatty matters which it contains are almost entirely absorbed by the intestine, and an animal deprived of it, requires a very large proportion of nourishment and more substantial aliments.

Journal de Pharmacie et de Chimie, October, 1856.

On the COMPOSITION of URSONE. By M. HLASIWETZ.

URSONE is a proximate principle, discovered by Trommsdorff, in the ethereal extract of the leaves of *Arbutus uva ursi*. M. Hlasiwetz has submitted this substance to elementary analysis, and has found it to be thus constituted:—

C	.	.	.	.	.	.	78.35	78.45
H	.	.	.	.	.	.	11.18	11.15
O	.	.	.	.	.	.	10.47	10.40
							100.00	100.00

whereas he deduces the formula $C^{20}H^{17}O^2$ corresponding to $C = 78.43$, $H = 11.11$.

From this composition and the properties recognised by Trommsdorff, ursone appears to belong to the category of neutral resins. It fuses at $198-200^\circ C.$ ($388-392^\circ F.$), and, on cooling, it becomes a crystalline mass. Heated to a temperature beyond its melting point, it becomes amorphous and cracks.

Its composition and principal properties agree with those of *Hartine*, $C^{40}H^{24}O^4$, a crystallised resin found by M. Schröller, in the lignite of Hart.

We may remark, moreover, that *arctuvine*, which, as is known, is a derivative of the decomposition of arbutine, is constituted according to the formula $C^{20}H^{10}O^7$. Replacing H by O, we obtain $C^{20}H^{17}$, which differs from ursone only by not containing O^2 .

Neues Repertorium für Pharmacie.

RESULTS of EXPERIMENTS MADE *with the VIEW of DETERMINING*
the ACTION of ORGANIC BODIES on OXYGEN.

By M. L. PHIPSON.

I TAKE the liberty of communicating to the Academy, the results of some experiments mentioned in this note, because I think that they may perhaps throw some new light on the nature of ozone, and elucidate some questions which are at present the subject of much discussion.

Oxygen is converted into ozone under a great number of influences; when this gas abandons any combination whatever, it is in the state of ozone when *nascent*, whether it be disengaged from the combination in question, by means of electricity, or by any other means.

It is in the state of ozone *at the moment at which it combines*, and this is especially remarkable in the case of organic bodies.

I have proved* that not only nitrogenous bodies, but also neutral ternary bodies, and various hydrocarbons, transform oxygen into ozone, when they begin to undergo alteration.

As regards the nitrogenous bodies, my experiments have been made on the nitrogenous juices of certain champignons, and of certain fruits and phanerogamous plants. The test employed was a solution of guaiacum in alcohol. Among binary and ternary bodies, starch, liquid sugar, ether, and alcohol, the essences of bitter almonds, cinnamon, citron, cummin, crude turpentine, balsam of Peru, and the fixed and fatty oils were submitted to experiment. This time the reagent employed was the ozone metrical paper, and I have been forcibly led to the conclusion, that whenever oxygen reacts on an organic body, this gas is in the state of ozone. Light appears to have an influence on the phenomena observed.

Comptes Rendus, No. 18, Nov. 3rd, 1856.

On ALBUMINOÏD SUBSTANCES and their TRANSFORMATION into UREA.

By M. BECHAMP.

1. M. LEHMANN says that respiration consists essentially in an exchange of gaseous elements between the blood and the atmospheric air—an exchange which takes place in the lungs.

But where and how does oxygen acquire the activity which it possesses in the blood?

M. Dumas has, in my opinion, given the solution of this problem. In his work on the blood, in common with M. Prévost, he remarked that there exists a manifest connection between the temperature of an animal and the richness in globules of its blood. The only exception which he has observed is in the tortoise, whose temperature is not in proportion to the considerable amount of globules in its blood.

If the temperature of an animal is higher in proportion as its blood contains a greater number of globules, this is, because, in this animal, the phenomena of oxidation are more powerful. On the other hand, it is the globules which, in the blood, retain the oxygen. Its power of dissolving the serum is hardly superior to that of water, at the ordinary pressure. If, moreover, we admit the theory of M. Küss, that the sanguineous globules are not utricles, but compact organs, solid in all their parts, (the globules only contain 69 per cent. of water,—they are the least aqueous organs), I think that we shall be very near the comprehension of their use, without bringing in their affinity for oxygen, a species of oxygenous combination of globules.

* My observations in this respect have been published at various times in the *Journal de la Société des Sciences Médicales et Naturelles de Bruxelles*; at the close of the year 1855, and in the course of the present year, and that Journal may be consulted for details.

The sanguineous globules condense oxygen in the same manner as porous bodies.

2. It is well known that porous bodies, in condensing oxygen in their pores, singularly aid the phenomena of oxidation. This fact has never been explained. I am now about to try to do so.

Lavoisier* said that all gas is a combination of a simple body and caloric—oxygen gas, such as we know it, a combination of the simple body oxygen with caloric. Now, in the condensation of gases by porous bodies, heat becomes free; the oxygenous gas is decomposed into caloric, which is disengaged, and simple oxygen, which, it will be understood, acts more powerfully than oxygen gas.

It will be remarked also, that the phenomenon is scarcely impeded by the presence of a liquid which is imbibed by the porous body; now, platinum charcoal, impregnated with caustic potassa is a more powerful oxidising body with regard to certain bodies, than the same metal divided and dry.

I admit, then, that the concrete globules of blood in the manner of porous bodies, condense oxygen, and restore it—at least in part—to its simple state—a state in which it is more powerful than oxygen gas, or gas combined with caloric.

This theory of the function of the globules explains these facts—that another gas may drive out the oxygen with which it is united, and that these globules disengage their oxygen into the vacuum.

Porous bodies, in similar circumstances, set free the gas which they have condensed.

3. According to this view, the whole mass of the blood would act as a porous body; the *liquor*, which holds the molecules of fibrine in quasi-solution, would oxidise the easily oxidisable products found in it—the respiratory aliments for example—whilst the globules would be the seat of the oxidation of the albuminoids.

This last opinion is based upon the experimental fact, that the globule is a transitory organ, which appears, lives, and dies.

I therefore consider the sanguineous globule as an organ in which, in the midst of an alkaline liquor, the condensed oxygen—the oxygen which has assumed the properties of nascent oxygen—determines the oxidation, the partial combustion, the combustion with decomposition, of the albuminoid matters.

4. This train of thought has suggested to me the idea of determining the formation of urea by means of the albuminoid matters under the oxidising influence of an alkaline mixture, as nearly as possible in the same conditions as the organic conditions. My first idea was to use oxygenous water and the sanguineous globules themselves. The want of materials made me decide upon replacing oxygenous water by hypermanganate of potassa—a compound which, under the

* In all kinds of gas there should be distinguished caloric, which in some respects plays the part of a solvent, and the substance combined with it—and which forms its base. We call *oxygen gas* the combination of this base (oxygen) with caloric.

influence of reducing agents, is decomposed into peroxide of manganese, oxygen, and potassa.



so that, in the presence of water, it becomes using nascent oxygen in an alkaline liquor. I thus succeeded in decomposing the albumen of the blood and of the serum, fibrine, and gluten. I obtained urea, carbonic acid, ammonia, and many other products, amongst which was a powerful acid.

This experiment is important; for this was the first time that urea has been produced by the oxidation—in some degree direct—of albuminoid matters.

Before transforming albuminoid matters into urea, I made some preliminary experiments; the result of which was, that urea resists the oxidising action of hypermanganate of potassa.

5. Preliminary Experiments.—Action of Hypermanganate of Potassa on Urea.—(a) I put in contact equal equivalents of urea and hypermanganate of potassa, dissolved in about twenty times their weight of water. The whole was abandoned to the ordinary summer temperature, in an apparatus with a disengagement tube. After forty-eight hours no indication of reaction was perceptible—that is to say, no gas was disengaged, and the liquor retained its color and transparency. At the ordinary temperature, therefore, hypermanganate of potassa does not act as an oxidising agent on urea.

(b) It is only by raising the temperature to 80° or 100° C. (176° to 212° F.) that a trifling disengagement of nitrogen is produced; but there is a great deal of ammonia in the liquor; whence we may conclude that, from the first moment of this action, the hypermanganic acid is reduced, carbonic acid is disengaged, and the liquor becomes alkaline, and that the latter, acting on the urea, transforms into carbonate the ammonia which is unattackable by the hypermanganate.

(c) If in experiment (a) we add 2 equivalents of sulphuric acid to 1 equivalent of hypermanganate, we likewise remark immediately a slight disengagement of gas—so slight that, in twenty-four hours, not a fourth part is obtained which should be disengaged. But, by plunging the apparatus into hot water, the reaction becomes evident, but not tumultuous.

In several experiments, I analysed the gas disengaged. There was constantly formed 2 volumes of carbonic acid and 1 volume of nitrogen. The decolorized residue, separated by the filter from the reduced binoxide of manganese, was a mixture of sulphate of potassa and sulphate of ammonia; the result of this is, that the free hypermanganic acid reacts on the urea, and that this reaction takes place as follows:—



These experiments show clearly that, in using hypermanganate of potassa as an oxidiser of albuminoids, there is no reason to fear any alteration in the urea—should it be produced—whether the solution be alkaline or neutral.

6. Action of Hypermanganate of Potassa on the Albumen of Eggs, on the Albumen of the Serum, on the Fibrine of the Blood, and on Gluten.—In the first place, we must pre-suppose that all the nitrogen of the albuminoid matter would be transformed into urea. Calculation indicates that 152 grammes of oxygen would be required, or nearly 1000 grammes of hypermanganate of potassa to 100 grammes of albumen, according to M. Lieberkühn's proportions. These 100 grammes of albuminoid matter would generate 35 grammes of urea.

After several fruitless endeavors, I found the following process :—I dissolved 10 grammes of albumen in thirty times its weight of water, and I added by degrees three-quarters of the hypermanganate required—about 75 grammes. The reduction, at first very rapid, soon stopped. It was then heated to 40° C. (104° F.) in the sand bath, and, from time to time, the liquor was saturated with very dilute sulphuric acid, so as to keep it always a little alkaline. By this means the operation goes on quickly, and there is no fear of destroying the urea as it is formed. When the decoloration was complete the liquor was filtered, and exactly saturated with dilute sulphuric acid. The solution, which was perfectly limpid, was evaporated in the sand bath; when reduced to a small volume, an excess of concentrated alcohol was added; sulphate of potassa and sulphate of ammonia were deposited. The alcoholic solution is evaporated to the consistence of honey, and again dissolved with heat by absolute alcohol. By evaporation, a residue is obtained which has the properties and characteristics of urea.

1st. When treated hot by caustic potassa, it disengages ammonia.

2nd. Treated by nitrous nitrate of protoxide of mercury, it produces a disengagement of nitrogen and carbonic acid, in the proportion of 1 volume of carbonic acid to 2 of nitrogen.

3rd. Nitric acid causes the appearance of the characteristic crystals of nitrate of urea.

4th. Oxalic acid produces a white precipitate, which, when examined in the microscope, shows very characteristic crystals.

5th. Treated with neutral nitrate of binoxide of mercury—with or without the addition of potassa—we obtain the compound $C^2H^4N^2O^34HgO$, studied by M. Liebig.

6th. Finally, urea can be isolated from it. It is sufficient to saturate the precipitate of oxalate of urea with carbonate of baryta, to dry it, and to take it up again with alcohol. It may likewise be obtained crystallised, by M. Marcet's plan; the residue is to be dried *in vacuo*, and exhausted by absolute alcohol; ether is then added without mixing it in; a cloudy precipitate will be perceived between the two liquids, and, on the next day, crystals of urea will be found all round the sides of the vessel.

These characters demonstrate that the product obtained is really urea.

7. Remarks.—Not to introduce confusion in the description of the process which enabled me to show the formation of urea by the oxidation of albuminoid matters, I have said nothing about the intermediate

phases of the reactions ; I think, however, that I ought not to omit these details.

1st. The mixture of albumen and hypermanganate decolors very rapidly at first, and becomes a jelly similar to that produced by adding potassa to albumen. By degrees, as the reaction proceeds, this mixture liquefies.

2nd. If the hypermanganate has been added by degrees, and in insufficient quantity, on filtering the decolorized liquor, and saturating it with dilute sulphuric acid, we find a white flocculent precipitate of apparently albuminous matter. This product is found with the other albuminoid matters.

3rd. If we continue the addition of hypermanganate and the partial saturation with dilute sulphuric acid, we at length find that the filtered liquor saturated with sulphuric acid no longer produces a precipitate. If we then saturate completely, we obtain, on adding alcohol to the concentrated liquor, a precipitate of sulphate of potassa containing very little sulphate of ammonia, and an alcoholic solution. This latter, evaporated to the consistence of an extract, and taken up again by absolute alcohol, yields urea, and there is, as residue, a salt of potassa which becomes perfectly hard by this treatment. This salt softens when heated, and, if the temperature is raised, it burns, diffusing an odor similar to burning horn. It is very nitrogenous. Its solution is precipitated by nitrate of lead, and by nitrate of binocide of mercury. These precipitates, decomposed by sulphuretted hydrogen, give an acid liquid which, when evaporated, leaves a syrupy residue of a fresh, agreeable, acid flavor. This product does not crystallise, but, after a long time, crystalline lamellæ appear in it.

4th. I have not been able to ascertain the formation of any of the odorous principles produced by other processes of oxidation. Neither acetic nor valerianic acid are produced.

5th. No gas is disengaged during the reaction. When we saturate with sulphuric acid, carbonic acid is disengaged, but no trace of nitrogen. This gas is found in the state of ammonia, urea, and the product or products indicated (3rd).

6th. If the oxidation is performed by hypermanganate alone, it will require heating to 80° C. (176° F.); but then we find only traces of urea. If, moreover, we add a slight excess of hypermanganate, we find only ammonia as the residue of the albuminoid matters.

7th. The fibrine of the blood appears to be more easily transformed than albumen and gluten.

8th. Fibrine and gluten dissolve without forming a jelly, when producing the same results as albumen.

From these remarks, it appears clearly that the oxidation of albuminoid matters, under an alkaline influence, gives very different products from those obtained at a higher temperature, by means of oxidising mixtures of peroxide of manganese, or of bichromate of potassa and sulphuric acid.

I intend resuming this investigation to ascertain the products

which would be furnished by gelatine, chondrine, and, generally, the matters which most nearly resemble the albuminoid matters.

Annales de Chimie et de Physique, November, 1856.

MEMOIR on PEPSINE. BY M. BOUDAULT.

IN a former memoir which I presented to the Academy of Medicine, about a year ago, I noticed a process for the preparation of pepsine by means of the stomach of a sheep; and I at the same time made known some of the interesting reactions of this product secreted by the stomach.

Being able now to procure very considerable quantities of pepsine, both in solution and in the pulverulent state, I have endeavored to complete the chemical study of this product, and to give the most suitable means of employing it as a medicament.

I shall now detail some known facts, and give well-ascertained scientific results; but, being desirous of establishing a point of comparison between natural pepsine and pepsine obtained by chemical processes, I think it useful to bring all their reactions together, in order to prove their complete similarity.

What are comparatively the physical, chemical, and physiological properties of natural and artificial pepsine? How is pepsine to be administered as a medicine? These are the questions which I propose to answer in this work.

We used, in our experiments, Dr. Blondlot's remarkable process for obtaining natural gastric juice; it was by means of stomachal fistulas, effected in a great number of dogs, that we were enabled to procure a quantity of natural gastric juice.

In the first place, we may state, that we mean by natural gastric juice the liquid product secreted by the stomach, and by neutral or acid pepsine, the matter obtained by evaporating the gastric juice, or by precipitating it with neutral acetate of lead.

By artificial gastric juice, we shall designate the solution in water of neutral or acidified pepsine, but obtained from the stomach of herbivora.

Natural gastric juice, freed by filtration from the mucus which accompanies it, and from the alimentary matters which line the inside of the stomach, and with which it is contaminated when collected, is liquid and limpid, but is of a light amber color; its density is rather greater than that of water. Its taste is styptic and slightly saline. Its odor is characteristic, especially when heated; it then gives out an odor of broth. After having been exposed for six hours to the temperature of 50° C. (122° F.), it becomes slightly turbid, and loses its digestive properties. When the gastric juice is pure, it may be preserved out of contact with the air, for several years without alteration, and without losing its chemical and physiological properties. Alcohol precipitates pepsine from its solution, and separates it from its acid.

Absolute alcohol dishydrates pepsine and deprives it, if not entirely, at least partially, of the property of digesting fibrine.

Tannin forms a precipitate in the gastric juice; the precipitate possesses none of the physiological properties of pepsine.

The metallic salts form a precipitate in the gastric juice; this precipitate, when decomposed, reconstitutes pepsine with its physiological properties.

The quantity of water which the gastric juice contains is considerable; it is about 0.97 per cent. in the greatest state of purity of the liquid secreted, there is about 1.25 per cent. There remains, then, 1.75 per cent. of salts, which are always nearly the same, but in different proportions. They consist of carbonate and phosphate of lime, chloride of sodium, traces of sulphate, and frequently traces of ammoniacal salts. Finally, the gastric juice also contains a free acid.

The gastric juice should always be acid to be endowed with digestive properties. It is, indeed, demonstrated, that the gastric juice is invariably acid in all animals, of whatever age and species, and even whatever may be their diet. We may remark, that the gastric juice is not always acid in the same degree. It is the more acid as the food contains more amylaceous principles—this explains what acid it is which forms part of the composition of the gastric juice.

We shall not seek to reopen the great discussion as to the presence of other acids—such as the acetic, the butyric, hydrochloric, phosphoric, &c.—either in the free state, or in the state of acid salts. We have certainly proved the presence of a certain number of these acids and salts in the gastric juice, but in very minute proportions, compared with the quantity of lactic acid. It may even be supposed that the acids were set free by the action of the lactic acid on the salts; it was especially after the concentration of the gastric liquid that we were enabled to prove their presence.

The acid of the gastric juice gives to the latter the reactions of acidulated water; it attacks iron filings, and decomposes the alkaline carbonates.

Entering upon this question with the combined aid of chemistry and physiology, we have sought to examine the first of these questions. Is this portion of the gastric juice secreted acid?

Animals were killed during digestion: the mucus membrane was carefully separated; we removed with a stream of distilled water all the soluble matters, until litmus paper was no longer reddened; then the stomach was scraped, the cellules broken, and we collected by washing again with distilled water, a perfectly neutral liquid. This liquid was put in contact with fibrine for several hours, at the temperature of 40° C. (104° F.); no digestion took place. But we added to another portion of this liquid, placed in the same conditions, a small proportion of lactic acid, and at the end of two hours, we obtained a complete digestion. From this experiment, which was repeated a great number of times, on carnivora as well as on herbivora, it is easy to conclude that pepsine is secreted neutral.

Now that it is admitted that lactic acid is always found in the gastric

juice, and that it is one of its principal agents, how is this lactic acid formed? Is it formed by the action of the alimentary matters on the amylaceous matters, or by the contact of pepsine with these same amylaceous matters?

Pepsine has always indeed been regarded as a true ferment. But it certainly does not act like other ferments,—like beer-yeast for example. It acts on alimentary matters by dissociating them, and causing them to undergo an isomeric modification, so to speak, but not a decomposition.

Hitherto it has been admitted that acid pepsine, that is to say, the gastric juice itself, was capable of converting glucose into lactic acid. We have ascertained from numerous experiments, that neutral pepsine may act like a true ferment in presence of glucose; convert it into lactic acid, and reconstitute a gastric juice.

Indeed, I took a certain quantity of pepsine in solution in water, and perfectly neutral, and added glucose; the whole was subjected to a temperature of 40° C. (104° F.), for twelve hours. After some time, there was a formation of lactic acid, and when I added fibrine to this mixture, after a few hours, I obtained a complete digestion. It is, moreover, completely demonstrated that, by means of neutral pepsine, there can be no digestion, and with pepsine and glucose there can be immediately no digestion; it is effected, in fact, only when the glucose is converted into lactic acid. This experiment led us very naturally to explain the presence of a very considerable quantity of lactic acid in the gastric juice. The amylaceous matters are converted by the salivary diastase; considerable quantities of glucose are thus found in the stomach; pepsine completes the conversion into lactic acid.

The other acids may certainly play the same part as acetic acid in the stomach, when this aid is wanting. However, we may state that we have never obtained complete artificial digestions with pepsine, acidulated with hydrochloric or acetic acid, as we have done with pepsine acidulated with lactic acid.

We have given the properties of natural pepsine, and we now come to the examination of artificial pepsine, and to follow them through their reactions.

It very naturally occurred to me to ask, whether natural pepsine precipitated from the gastric juice of the carnivora, and pepsine obtained from the stomach of the herbivora, had the same chemical composition in hundredths.

We endeavored for a long time to procure pepsine chemically pure, either by employing the gastric juice obtained from the stomach of dogs, or from the stomach of sheep. It was difficult for us to obtain twice following a product having in hundredths the same chemical composition. I obtained products, physiologically speaking, perfectly similar, and the chemical reactions of which were exactly the same; but when I subjected them to a complete analysis, I found such great differences, either in the nitrogen, or in the carbon, or in the water, that I could not arrive at a very accurate formula in hundredths. Whenever I obtained a product of great purity, that is to say, after having isolated

it completely from salts and acids, it had lost its physiological properties.

In order to prove the perfect similarity between the two products, I had recourse to their physical, chemical, and above all, to their physiological properties,

I took as a point of comparison, or rather as a normal type, the gastric juice of the dog, obtained in the following conditions, and always from healthy animals.

These animals always received an equal weight of food, always containing the same amount of nitrogenous matter.

Every day at noon, they received this food. On the day on which I wished to collect the gastric juice, instead of giving them this customary food, I gave them tendons of beef, which had been boiled for a very long time in water, and having washed them so that there might be no other matters in them capable of dissolving more rapidly in the animal's stomach.

I dried them in a drying stove, and I cut them in small pieces, and then administered them to my animals.

I attached a caoutchouc bag to the canula, corresponding to the stomach, and I collected in these conditions a gastric juice always nearly identical, that is to say, having the properties which I have indicated above, and especially that of digesting a determinate quantity of fibrine, with a determinate quantity of gastric juice. Thus 100 grammes of gastric juice from the dog should always digest 40 grammes of dried fibrine, by submitting it for four hours to a temperature of 40° C. (104° F.)

It was then easy for me in these conditions to make all my comparative experiments.

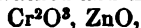
(To be continued.)

ANALYTICAL CHEMISTRY.

On some NEW REACTIONS of OXIDE of CHROMIUM.

By M. G. CHANCEL.

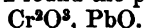
WE are directed, in some chemical treatises, as a means at once of separating chromium and zinc from other metals contained in the same solution, to treat this solution with an excess of caustic potassa, in order to dissolve only the oxides of chromium and zinc (as well as the alumina.) This reaction can never have been adopted practically, for, according to my experiments, chromium and zinc cannot coexist in a solution of potassa, since they are mutually precipitated. Indeed, when we mix together two potassa solutions, one containing oxide of chromium, and the other oxide of zinc, the solution which has been employed in excess precipitates the other completely. We thus obtain a green precipitate, which, washed and dried, contains:



and which presents, consequently, a composition similar to that of

chrome iron ore, magnetic oxide of iron, and the combination of oxide of chromium with various bases recently obtained by M. Pelouze.

The same reaction is observed also between oxide of chromium and oxide of lead, both dissolved in potassa; a green precipitate is likewise obtained, in which I found the proportions:—



I have observed another fact, which may be turned to account in the analyses of bodies containing chromium. Whenever we have oxide of chromium in solution in potassa, or simply mixed with that alkali, it is sufficient to add pure binoxide of lead to the solution or mixture and to heat gently, to dissolve the whole of the chromium in the state of chromate of lead. A yellow liquor is thus obtained, which, freed from the excess of pure binoxide by filtration, precipitates the chromate of lead by supersaturation with acetic acid.

The foregoing reaction furnishes a very simple means of converting oxide of chromium into chromic acid. This transformation by the humid way is much more speedy and convenient of execution than the same transformation by the dry way (by means of nitrate of potassa), which is generally used in laboratories.

Comptes Rendus, No. 19, Nov. 10, 1856.

A METHOD for the DETECTION and QUANTITATIVE ESTIMATION of QUININE and other ALKALOIDS, when COMBINED with FATTY OILS.

By WILLIAM BASTICK.

HAVING been required to examine some specimens of cod-liver oil with quinine, with a view to ascertain the presence of that alkaloid and its quantity in them, and there being no published method for the accomplishment of these objects, I have been compelled to devise one. Believing this method to be as simple as it is effectual, not only for the detection and estimation of quinine when dissolved in cod-liver oil, but also for the discovery and separation of other alkaloids when combined with the fatty oils in general, I venture to lay it before the Members of this Society. To render my method more intelligible, I will briefly describe an example of its actual application. Two ounces of the oil supposed to contain quinine were measured off, introduced into a bottle capable of holding four ounces of liquid, and then one ounce of a solution, consisting of sulphate of soda dissolved in distilled water, and slightly acidulated with sulphuric acid, was added. After closing the bottle, the contents were strongly and repeatedly agitated, so as to thoroughly mix the watery and oleaginous liquids. After these liquids had separated again by rest, half an ounce or rather more of the aqueous solution was removed from the bottle by means of a pipette, and passed through a filter, to separate a few small globules of oil still adhering to it. As half an ounce of this filtrate corresponded to one ounce of the oil under examination, care was taken to measure off this quantity correctly for the final operations, which consisted in precipitating the quinine therefrom by means of a

solution of caustic soda, slightly washing the precipitate with distilled water, redissolving it in alcohol, and after filtering the alcoholic solution, evaporating it to dryness in a water-bath. The residue was then weighed and examined in the usual manner, to ascertain if it were quinine.

I need scarcely explain, that the sulphate of soda in the acid solution is employed to effect the more ready and complete separation of the watery fluid from the oily one, and that the sulphuric acid is added to dissolve out and combine with the quinine, which exists in the oil in (if I may use the term) a caustic condition.

From experiments made with cod-liver oil with quinine, prepared by myself of known strength, I feel justified in observing that the method described gives sufficiently accurate results to be relied on for all practical purposes.

In cases where other alkaloids have to be detected and estimated, the process of separation to be employed would be precisely similar to that described for the separation of quinine. When separated, they will of course be recognised by the methods laid down in the text books on chemical analysis.

In conclusion, I may add that the quantity of sulphuric acid employed in this process should be sufficient to render the aqueous fluid slightly acid, after it has been well agitated with the oil containing the alkaloid, and that the proportion of the extracting fluid may be varied, in accordance with the convenience of the operator, and the nature and quantity of the alkaloid to be extracted.

Pharmaceutical Journal, Nov., 1856.

QUANTITATIVE DETERMINATION of the SULPHUR in MINERAL WATERS.

By J. MAXWELL LYTE.

THE method usually employed for estimating the quantity of sulphur contained in sulphurous waters, (that is to say, the process of M. Dupasquier, using an estimated solution of iodine), appears to me liable to great errors. It has been already demonstrated that the solution of iodine in alcohol decomposes by degrees, forming hydriodic acid. To remedy this inconvenience, it has been proposed to dissolve the iodine in some iodide. This process, it is true, avoids some chances of error; but there still remains a great difficulty to be overcome. In many springs (probably more numerous than we know of), there are hyposulphites, produced either by the oxidation of the sulphurets in contact with oxygen, or by other reactions. This presence of hyposulphites renders the process of M. Dupasquier very inaccurate, because iodine is likewise changed into hydriodic acid by the presence of hyposulphurous acid or of hydrosulphuric acid.

The method which I propose, is to precipitate the sulphur, as sulphuret of silver, by means of a double hyposulphite of silver and soda dissolved in an excess of hyposulphite of soda; this reagent is pre-

pared by dissolving chloride of silver in a solution of hyposulphite of soda, and it may be kept for a very long time, especially with the addition of one or two drops of ammonia. Nitrate of silver, dissolved in a great excess of ammonia, has already been proposed; but in this case, the iodides in it would be precipitated, and if the excess of ammonia is not sufficiently great, or if the water contains carbonic acid, the chlorides and bromides will be precipitated likewise; and should there be, as is almost always the case, some organic matters, they will be precipitated likewise, especially in contact with light, or with heat, should the spring be a hot one.

Comptes Rendus, No. 16, Oct. 27, 1856.

INORGANIC CHEMISTRY.

On LITHIA and its COMPOUNDS. By M. TROOST.

I WAS fortunate enough to obtain, during the Exposition of last year, an abundant supply of the commonest ore of lithia, lepidolite, from which I extracted from 5 to 6 kilos. of carbonate of lithia. These materials enabled me to undertake an investigation of which I now publish those results which appear to me to be the most interesting and new. These researches were made in the laboratory of the Ecole Normale, with the aid of the advice of M. H. Sainte-Claire Deville.

I was at once struck with the necessity of employing the igneous method of extracting lithia on a large scale from its ores.

The process which I have employed is founded on the fact, that, if we heat in a good furnace a mixture of suitable proportions of lepidolite, carbonate and sulphate of baryta, the matter fuses and undergoes a species of liquidation which produces at the lower part of the crucible a perfectly fused, but viscid glass; and above, an extremely fluid liquid, which may be removed while the crucible is still hot, either with an iron spoon, or by decantation.

This liquid, on cooling, gives a crystalline mass, either white or colored pink, by manganese. If, instead of removing this liquid, we allow it to cool in the crucible, we find two solid masses which do not adhere to each other. This crystalline and white mass is a combination of sulphate of baryta, with sulphate of potassa and sulphate of lithia. The large proportion of sulphate of baryta which it contains causes the crystalline form of this last salt to predominate, as far as I can judge by measuring one single angle of the elongated prism which this matter presents. Simply washing with boiling water is sufficient to separate the sulphate of baryta from the alkaline sulphates.

This process, which succeeds perfectly with lepidolite, does not succeed so well with petalite, unless a quantity of sulphate of potassa is added sufficient to raise the total amount of alkali to the same proportion as exists in lepidolite. This influence of the quantity of alkali on the results of the operation, led me to investigate, whether, by increasing still further the proportion existing in lepidolite, I could

not remove a large quantity of lithia; the extreme fusibility of sulphate of lithia, isolated or combined with sulphate of baryta, rendered this hypothesis very probable; an experiment confirmed it. I thus extracted by simple fusion about 3 per cent. of lithia—that is to say, at least as much as I should have obtained by the analytical method, which is both too slow and too expensive for practical purposes.

This process, which I practiced with 100 kilos. of lepidolite and 70 kilos. of petalite, will enable us to obtain easily and cheaply an alkali, the smallness of whose equivalent and whose peculiar properties render it likely to be practically useful.

Lime, and sulphate of lime, may be substituted for the carbonate and sulphate of baryta; lime, and chloride of calcium, may likewise be employed; but the volatility of the chloride of lithia leads me to prefer the sulphates.

The principal properties of lithium are known; this metal is unalterable cold, and even at its fusing point, by dry oxygen; I can melt it in an iron crucible and run it out in the air in a vessel, also of iron, without tarnishing it. The ingot which I present to the Academy was obtained in this manner, and may be preserved without alteration in a flask full of air. Lithium forms with potassium and sodium alloys which I have studied, and some of which are lighter than naphtha.

To prepare the metal I first employed the method of MM. Bunsen and Matthiessen, slightly modifying the arrangement of the apparatus, so as to avoid the loss of chloride projected in small drops, by the bubbles of chlorine which are disengaged at the positive pole of the battery.

I afterwards tried the preparation by chemical reactions; I took a mixture, analogous to that now employed with so much success by M. H. Ste. Claire Deville for the preparation of sodium. The mixture of carbonate of lithia, carbonate of lime and charcoal was heated to a white heat for several hours in an iron bottle, previously luted. The experiment was repeated three times without success. Although so uniformly negative a result left little to be hoped for, I repeated the experiment, by putting the mixture into a bottle of retort charcoal; but the matter, kept for six hours at a dazzling white heat, gave nothing. It is scarcely probable, after this, that lithium should be volatile; and I think I may conclude from these experiments that it cannot be obtained by the process which so readily furnishes potassium and sodium. These failures led me to try the action of sodium on chloride of lithium; it was effected at a gentle heat, and gave an alloy still rich in sodium, and which sunk in naphtha: to increase the proportion of lithium, it is sufficient to immerse it in a glass containing naphtha floating on water; the sodium decomposes the water before the lithium, and we obtain a globule which floats on the surface of the naphtha. This reaction may be turned to account for preparing lithium; it shows moreover that lithium differs from the alkaline metals, and approaches nearer to magnesium, the first metal prepared by this process. On trying the action of oxygen on lithium, neither binoxide nor tritoxide appeared to me to be formed.

This fact also establishes an approximation between lithium and magnesium. The study of the salts of lithium leads to the same conclusion; thus, whilst a current of carbonic acid diminishes the solubility of carbonate of potassa and carbonate of soda, by causing the formation of a bicarbonate, it augments very considerably the solubility of the carbonate of lithia, as it does that of the carbonate of magnesia, which leads me to think that there is no combination. Lithia does not appear to me to form either bisulphate or alum in the circumstances in which the corresponding compounds of potassa and soda are produced. The chloride and nitrate of lithia are more deliquescent than the corresponding salts of magnesia; moreover, in suitable conditions, they may, like the latter, crystallise with water of hydration. The chloride of lithium loses at its fusing point a portion of its chlorine, and becomes alkaline, like chloride of magnesium. The solution of the salts of lithia does not give a precipitate with carbonate of ammonia in presence of ammoniacal salts, and lithia always forms, like magnesia, double salts with the ammoniacal salts. Phosphate of lithia is insoluble, like phosphate of magnesia; finally there exists such an analogy between the reactions of the salts of lithia and of the salts of magnesia, that I know only one means of separating these two bases, namely, by the employment of caustic potassa, which precipitates magnesia without precipitating lithia. In conclusion, lithium appears to me to play, in the series of the alkaline metals, a part analogous to that of magnesium in the series of the earthy metals.

Cosmos, Nov. 14, 1856.

INDUSTRIAL CHEMISTRY.

GENERAL OBSERVATIONS *on the MATERIALS EMPLOYED in CONSTRUCTIONS in the SEA.*

By MM. CHATONEY and RIVOT.

IN their former memorial, MM. Chatoney and Rivot showed the result of the studies which they had made, with the assistance of chemical analysis, to ascertain the nature and the composition of the various hydraulic materials, as well as the reactions which prepare, accompany, and follow the setting of the immersed mortars. This study has enabled the authors to give useful hints to builders as to the choice and proportion of the elements which should, in accordance with circumstances, enter into the composition of mortars. But it is not sufficient, for obtaining durable mortars, that they should have a good chemical composition; the preparation of the materials, the fabrication and method of using the mortars, have a considerable influence on the result, because the same materials may, according to the processes employed, give very good and very bad results. This part of MM. Chatoney and Rivot's work is especially devoted to showing the rules which should be followed in this respect, namely,

in baking and slaking the lime, in the preparation and using the mortars, and in executing the building.

Baking and Slaking the Lime.—Baking limestone is for the purpose of driving off the carbonic acid and combining, by the dry way, the silica, alumina, and lime which they contain. This combination is so much the more perfect as the limestone is more homogeneous and the mixture of primary matters is more intimate.

The importance of this condition of homogeneity has been shown in a striking manner by the following experiment. They have baked the limestones of Fécamp, and as the products could not be extinguished by immersion, they were pulverised and sifted; when soaked in water, these hydrates would not bear M. Vicat's needle until after thirty-nine or forty-three days, and after immersion for thirteen months, ten or thirteen turns were sufficient to drive it in six millimetres. This same limestone, brayed, triturated, and mixed, reduced to paste and cut into cakes, produced a lime which bore Vicat's needle after eighteen hours immersion, and after thirteen months it required forty-three turns to cause the needle to penetrate it at all.

Whence we see that many *thin* limes may contain all the elements of a good hydraulic lime, but that they require braying and mixing before baking.

As the silicates and aluminates of lime combine with less ease with water when they are subjected to a higher temperature, it is important that all parts of the limestone should be equally baked, so that their hydration, as well as their setting, should take place simultaneously, and it is best that the baking should be continued until all the carbonic acid is expelled.

The slaking of hydraulic limes is performed, not in a paste like the fat limes, but by immersion or aspersion. In either case, whether the lime be argillaceous or silicious, all the water of extinction is employed to hydrate the free lime, whilst the compounds of silica, alumina, and lime remain anhydrous. This fact, analytically proved by the authors, has led them to conclude, that it is useful and sometimes indispensable to keep slaked lime a long time before using it. In the first part of this work, they showed that a previous digestion of hydraulic materials under the influence of moisture, prepares them for the chemical actions, and contributes largely to a successful result. They now show that there is no reason to fear that during this digestion, the silicate and aluminate of lime should become hydrated, that is to say, should set. Moreover, previous digestion is not a new fact; it has been neglected because its importance was not understood, but it must be returned to, and our authors give as an example to be followed, that the manufacturers of Portland cement, prepare their products and keep them warehoused for a long time before they sell them.

Preparation and use of Mortars.—Our authors divide mortars into two classes.

1. Mortars of lime and sand, and those of cement with or without sand.

2. Mortars of lime and puzzolane, with or without sand.

The process of manufacture, varying with each kind of material, should be intended to ensure regularity, and consequently stability to the chemical combinations which are made among the elements composing the mortars, and to render the latter compact and impermeable.

Lime Mortars.—The proportion of sand entering into the composition of mortars, has a great influence on their compactness. It will be understood that mortar will be more or less porous according as the volume of the lime paste is less or greater than the interstices of the sand. To avoid porosity, the proportion of lime in powder should not be less than 0.65, the sand and lime being measured without being pressed down.

As to the proportion of water to be employed in the manufacture of mortar, it is especially necessary to avoid excess, because it leads to a separation of the mixture of lime and sand, matters whose densities differ.

In describing the experiments which they have made with various hydraulic limes, the authors point out one general fact in the preparation of mortars; that the hydrates immersed after dessication in the air decompose rapidly, whereas these same hydrates, if immersed as soon as manufactured, or after having set under the influence of moisture, remain unaltered for a long time.

One other fact to which our authors attach great importance, was ascertained by them during the examination which they have made of hydraulic limes placed in the most varying conditions, namely, that everywhere a layer of carbonate of lime has a tendency to form on the surface of the mortars, and that tranquil waters which are powerfully charged with carbonic acid, cause this layer to develop so as to protect even quite soft mortars.

It will be remembered that in their former memoir, the authors showed that an excess of free lime in the composition of mortars is necessary to form, by its combination with the carbonic acid of the water, the protective envelope of carbonate of lime.

The observations which MM. Chatoney and Rivot have made on some old masonry, have proved to them that mortars are always attacked first on their surface, which softens and often becomes mud. This last circumstance, when produced, retards and even sometimes stops the progress of decomposition, by preventing the rubbing of the water against that portion of the mortar which remains hard. This is a preservative action similar to that of slime.

In comparing together the mortars of silicious lime and of argillaceous lime, the authors observe that argillaceous limestone, which is much more commonly met with than siliceous limestone, produces more tenacious lime and fatty mortars; they occasion, besides the setting of the hydrates, more complicated reactions, offer only uncertain chances of stability, and require, when used, peculiar precautions which are unnecessary with silicious limes. MM. Chatoney and Rivot consequently propose to manufacture artificial hydraulic limes by mixing pulverised silex with fat limes. They mention that the

experiments of this nature made at Havre have given excellent results. Hydrates made with proportions varying from 0.50 to 4 of powdered lime, to 1 of siliceous, set in from three to twenty days, and perfectly resisted during twenty months of immersion. They acquired a hardness comparable to Portland cement which has been immersed for two months. The authors therefore think that it would be advisable to establish manufactories of artificial limes for sea ports; especially as, hitherto, the only lime which has succeeded has been a purely silicious one.

Cement Mortars.—Natural or artificial cements are used, not after extinction like lime, but pulverised and wetted, either pure or mixed with sand. The influence of the sand diminishes the shrinking which is apt to take place in cements as they set, but it renders them more porous. The influence of water is very complicated: wetted with sea water, cements take less rapidly than cements wetted with soft water; their resistance, which is at first less, becomes quite equal to the first. The resistance of cements left in the air after their setting, is at first greater than that of immersed cements, but they become less afterwards. If cements which are not immersed are covered with a damp cloth, their resistance is augmented. When cements which have hardened in the air are immersed, their resistance at first diminishes, but afterwards becomes greater. Pure cements should be used with great excess of water, their texture becomes more compact. These various facts have been proved experimentally by our authors.

MM. Chatoney and Rivot describe the processes of manufacture of artificial cements, especially of Portland cements, to which a greater degree of hardness has been given than exists in most natural cements, at the same time that they set less rapidly, which is a great advantage for building in the sea. They recommend, giving most striking reasons, not to employ artificial cements except after long digestion, and with a great excess of water. They consider it advisable to pass it through a fine sieve to remove the larger parts and the vitrified grains contained in it. These residues, when crushed and passed through the sieve, set more slowly, and seem to acquire even greater hardness, than the remainder of the cement.

Mortars of Lime and Puzzolane.—Natural puzzolanes were used by the Romans for constructions in the sea, which are still in a perfect state of preservation. The Dutch have likewise used them with great success in their dams.

But all attempts to use puzzolanes, whether natural or artificial, have failed lately. According to MM. Chatoney and Rivot, these mistakes might be avoided, in the use of puzzolanes, if, according to the custom of the ancients, they were previously subjected to prolonged maceration. These gentlemen have not as yet any experimental proof of this assertion, but it appears very natural. In fact, if previous digestion is useful to mortars, whether of lime or cement, it is indispensable to the success of mortars of puzzolanes which differ from these, inasmuch as the compounds of lime with silica and alumina exist ready

formed by baking in lime and cements, and do not require to be hydrated when used, whereas in the manufacture of mortars of puzzolanes, the silica and alumina have to free themselves from the combinations in which they exist in the puzzolane, and to form with lime, by the humid way, compounds which will become hydrated under water. Whence we see that it is better to mix puzzolane with fat lime in preference to hydraulic lime, because, in the latter case, the hydraulic compounds, formed by the dry way, besides the baking of the lime, would set long before those formed by the humid way can become hydrated, and the setting of those last would run the risk of causing the disaggregation of the mortars.

Artificial puzzolanes are pulverised baked argil. The greater part of them contain lime: the result is, that they contain the same causes of destruction as mortars of natural puzzolane and hydraulic lime; they have not hitherto succeeded in the sea, and will always be difficult to use, chiefly in consequence of the irregularity of the mortars formed of them.

Execution of Masonry.—MM. Chatoney and Rivot recommend the following precautions in building.

To use the materials, not only moistened, but quite wet, so that the setting of the hydrates may take place under the influence of moisture, a condition which is indispensable for their stability: to execute the works rather in the sea than dry; for freestone masonry, to put the stones into a full bath of mortar after steeping in water, and to avoid driving in; finally, to confine the use of impermeable stones to those portions of the work where they are indispensable, and to substitute for them elsewhere, either bricks or chalky stone, which, when thoroughly wetted, would form an intimate union with the mortar.

The authors have, in this memoir, only considered mortars exposed to sea water, but they think that the greater part of their observations are applicable to mortars immersed in fresh water. It is only about two years since, that the injury done by sea water to masonry has been ascertained; it has only been observed since a too great confidence in hydraulic materials has led to the construction of masonry of stone in immediate contact with water, without any of the protections which had formerly been in use. It is most probable that in fresh water the action is similar, although not so powerful as in salt water, and it would most likely vary with the composition of the water, in the same manner that the action of the sea water varies in proportion with the carbonic acid contained in it, the temperature and the motion of the waves.

MM. Chatoney and Rivot are aware that they have not solved all the questions connected with marine constructions, and would observe, that to solve some which they have only mentioned, would require complete experiments of long duration in various localities and under various circumstances.

Comptes Rendus, No. 17, October 27, 1856.

On CASEINE CEMENT. By Professor R. WAGNER.

If caseine, precipitated from milk by means of acetic acid, be dissolved in bicarbonate of potash or soda, a fluid is obtained which possesses adhesive properties in a high degree, and was recommended 20 years ago by Braconnot as a cement. The author, by employing various other alkaline solvents for the solution of the washed and pressed caseine, obtained cements which possessed valuable properties.

By dissolving caseine in a cold saturated solution of borax, a clear fluid of a thickish consistence is obtained, which is remarkable for its high adhesive power, in which it far exceeds gum arabic. This fluid gives a slightly shining, varnish-like coating to paper which is covered with it; and the paper thus coated may be employed with much advantage for tickets, which only need to be moistened to make them adhere firmly. It may also take the place of glue in many cases. Experiments in glueing wood with it, gave the most favorable results. The fluid may also be employed in place of albumen in stuff-printing, in the manufacture of artificial meerschaum, to give lustre and consistence to silk fabrics, in the preparation of English plaster, artificial flowers, &c. Woollen and cotton stuffs soaked with the solution and then dried, may be tanned with tannic acid or acetate of alumina, and thus converted into water-proof stuffs. A solution of caseine in soluble glass is to be recommended as a cement for porcelain and glass. Mixed with calcined magnesia, this fluid might perhaps be suited for the preparation of artificial meerschaum.

Polytechn. Centralbl., 1856, p. 958.—*Chemical Gazette*, Oct. 15, 1856.

On the MANUFACTURE of PHOSPHORUS. By M. FLECK.

WE take from a complete work on the manufacture of phosphorus, some observations which appear interesting in practical chemistry.

Bones freed from fat and broken up are macerated in dilute hydrochloric acid, which gives rise to chloride of calcium and acid phosphate of lime, Ca O , 2 HO , Ph O^5 ; the maceration is continued until all the calcareous substance is extracted, and the gelatinous matter alone remains; the latter is washed and steeped in lime water, and afterwards treated in the ordinary way, in order to extract the gelatine or glue.

The liquid containing the chloride of calcium and biphosphate of lime, is evaporated in glazed earthen boilers, or well baked stoneware boilers, heated by means of the waste heat of the phosphorus furnace. When the solution stands at 38° Beaumè, it is poured out and cooled; biphosphate of lime separates in slender crystals, the proportion is increased by submitting the mother-liquors to further evaporation. Finally, these mother-liquors still retain phosphoric acid, which is separated definitively by means of milk of lime. The product, which

is ordinary phosphate of lime, is then set apart with the residues of the retort.

Owing to its solubility, the acid phosphate of lime cannot be washed; it is purified, either by pressing, or by spreading it on a porous slab, under which the air is rarified in order to force the mother-liquors to pass through the pores of the slab. The biphosphate thus purified is then presented in the form of a pearly mass, which is gritty to the touch; it is mixed with one-fourth of its weight of powdered wood charcoal; the whole is passed through a sieve and introduced into the retorts.

The latter are earthen cylinders, arranged in stacks of five like gas retorts. The disengagement tubes enter a common receiver having the form of a muffle, and which is placed in a trench communicating with a stream of water. This first receiver communicates with a second similar to it.

The fuel may be coke or coal; if the acid phosphate has not been well freed from chloride of calcium, there is produced, during the distillation, a certain quantity of chloride of phosphorus, which diminishes the yield.

The residue of the retorts is composed, as is known, of charcoal and phosphate of lime; this residue is incinerated on iron plates placed on the furnace. The product of the evaporation is treated with hydrochloric acid, conjointly with the phosphate of lime obtained by precipitation. Chloride of calcium and biphosphate of lime again result, and the above treatment is repeated.

Apart from unavoidable loss, the phosphorus thus obtained corresponds, according to the author, to the proportion indicated by theory, that is to say, six or seven kilogrammes per 100 kilogrammes of bones, the old process furnishes at most five per cent.

The difficulty of procuring evaporating vessels of glazed earthenware may, perhaps, throw some obstacle in the way of the employment of this process; perhaps this may be overcome by using boilers similar to those used in the manufacture of alum.

Dingler's *Polytech. Journal*.

AGRICULTURAL CHEMISTRY.

On the Loss of AMMONIA from PERUVIAN GUANO.

By M. A. BOBIERRE.

MANY plans have been proposed to avoid waste from guano; common salt, plaster, and pulverised wood charcoal, have given excellent results. In England, by employing one fifth of pulverised wood charcoal, some experimentalists have said that the harvest on the second year was as good as the first; fine animal charcoal whose absorbant power is so well known and so much used, has been used for the same purpose; finally, it has been observed lately that earth itself, by its contact with guano, diminishes the volatilisation of the ammoniacal principles, which so easily manifests itself in places when this manure is stored.

I wished to ascertain numerically in what conditions animal char-

coal acted on guano. For this purpose I tried the following experiments, taking as the element of appreciation a Peruvian guano containing 0.163 of nitrogen.

I. Thirty litres of air at 50° C. (122° F.), were circulated over 3 grammes of damp guano. The operation last an hour, it was dried before passing into the tube heated in the sand bath, which contained the guano. On passing out of this tube, it passed through a flask containing 10 cubic centimetres of M. Peligots sulphuric test liquor. After the operation I found that the air which had passed over the guano had removed 0.00437 gr. of nitrogen, or 0.00145 per gramme of guano.

II. Three grammes of the same guano were mixed with one gramme of animal charcoal containing 0.095 of nitrogen, and the same quantity of air was passed over the mixture, as in experiment I. The test liquor showed no trace of ammoniacal loss.

III. The same guano was dried in the open air, and in a moderately warm capsule. It was frequently stirred with a glass rod. The guano only contained 0.1503, or a loss of 0.0127 of nitrogen.

IV. The same quantity of guano was dried in a capsule, heated in precisely the same circumstances, but with the addition of $\frac{1}{2}$ per cent. of its weight of animal charcoal. The loss of ammonia amounted to 0.0009, that is to say was almost inappreciable.

V. We left for 10 days in a strong current of air two plates containing, the one untouched guano, and the other a mixture of guano and one third animal charcoal, containing 0.095 of nitrogen; whenever the weather would permit, the plates were exposed to the sun. After this lapse of time, I proceeded to the estimation of the nitrogen. The guano to which the charcoal had been added, had only lost 2 thousandths of nitrogen. The pure guano had fallen from 0.164 to 0.142. It had lost more than 2 per cent.

From these experiments we find; 1st. That Peruvian guano should not be analysed without taking account of the ammonia which has evaporated during its dessication; 2nd. That when accuracy is necessary, and for commercial tests, we may avoid error by drying the Peruvian guano in the presence of fine animal charcoal, whose amount of nitrogen is known; 3rd. That the annexation of an ammoniometrical flask to dessicating apparatus is necessary for giving an exact estimation of the nitrogen in guano; 4th. That the employment in agriculture of Peruvian guano mixed with animal charcoal, would be of great advantage in argilo-silicious soils, because in such cases the soil is improved by ammoniacal substances and phosphates.

Comptes Rendus, No. 16, Oct. 20th, 1856.

PHOTOGRAPHY.

RAPID PROCESS of PHOTOGRAPHY on WAXED PAPER.

By M. DE LA BLANCHERE.

WHEN the paper is well saturated with wax it is steeped in an ioduretted bath, which should fulfil certain conditions; it should contain a

powerful size which fills up the pores of the paper, and assimilate it to a smooth and inert surface. If too thick, this size will produce, when the leaves are suspended, oblong drops or irregular lines, which would form black patches in the gallic acid bath.

There must be but little iodide, so that, after having penetrated the pores of the paper, there should not be too thick a layer of silver, which would produce soft, pasty proofs.

The size should be composed as follows:—

Common water	2 to 3 litres.
White rice	150 grammes.
Linseed	50 „
Fish glue or white gelatine	25 „

Gelatine, and especially fish glue, should be dissolved warm, particularly if the latter is preferred; because it takes longer to melt. The rice and linseed should be allowed to cook in it for some time. The first should be nearly all reduced. The *ioduretted bath* should be composed of:—

The above size, filtered through a cloth while hot	} 1 litre.
Sugar of milk	50 grammes.
Iodide of potassium	35 „
Bromide of potassium	5 „

The pieces of paper are steeped one by one (avoiding bubbles of air) for about ten minutes, they are hung up to dry.

This paper will keep well for two or three months, and after that time its freshness may be restored by steeping again in the bath.

The sensitive bath should be in the usual proportions.

Cosmos, Nov. 14th, 1856.

INVENTION of PHOTOGRAPHY.

In a French work, lately published,* in which a review is given of the results of photography, as shown in the Paris Exhibition of 1855, after awarding great praise to the works of Mons. Bayard there displayed, the author proceeds as follows:—

“Mons. Bayard is one of the inventors of photography on paper. When this discovery existed only in the domain of science, that is, before the publications of Fox Talbot, he, by himself, had already discovered, in his own secluded retreat, the method of fixing on paper the images of the camera obscura. The fact, at the present time, is almost unknown. For this reason, if the reader will permit me to digress, I will relate how Mons. Bayard came to discover the art of photography on paper, and how his discovery has remained a secret from all. The tale, moreover is not long; in fact, as we should say,

* “Les Applications Nouvelles de la Science à l’Industrie et aux Arts en 1855.” Par Louis Figulier, Docteur ès Sciences, Docteur en Médecine, agrégé de Chimie à l’Ecole de Pharmacie de Paris, &c.

it is only the history of a peach. Mons. Bayard is the son of a respectable local judge, who exercised his duties in a small provincial town. He occupied his leisure hours in cultivating his garden. In this garden was an orchard, whose splendid peaches ripened under the autumn sun. Mons. Bayard, the father, was in the habit every year of sending to his friends baskets of this fine fruit, and with the natural pride of a proprietor, he took pains, when he sent them out, to mark by some unmistakeable sign, that the fruits were the produce of his garden. He conceived a singular mode of effecting his object, which unknown to the inventor, was a real photographic process. He picked out a peach on the tree in the process of ripening; it was, as you may imagine, one of the best—one of those "*pêches à trente sous*," which, in after times, thanks to Mons. Alexander Dumas the younger, have played so conspicuous a part in the world—or rather, the dramatic world. In order to preserve it from the action of the sun's rays, our judge took care to cover his peach with leaves. When the peach thus defended from the sun's rays had acquired the desired size, he pulled off the leafy covering, and left the fruit freely exposed to the influence of the light, save that he gummed on the surface of it the two initial letters of his name, neatly cut in paper characters. At the end of a few days, when the protecting papers were removed, the two initials were found distinctly marked in white on the red ground of the fruit, which thus became impressed with an unquestionable stamp at no greater cost than the sun's rays. This occurrence, which young Bayard each year was in the habit of witnessing, naturally made an impression on his mind. As a child, he amused himself by repeating this effect of the sun on pieces of pink paper braided into the form of a cross. The parts of the paper concealed from the light by the overlying bands preserved their color, while the other parts were quickly bleached. Afterwards, having tried, like many others, to fix the images of the camera obscura, Mons. Bayard conceived the idea of using for the purpose the rose-colored paper which had served to divert his infancy. But, placed in the camera, the paper was not sufficiently sensitive to the light. Mons. Bayard then resorted to the use of chloride of silver in lieu of this inactive paper—the very photographic agent in use at the present time. He thus succeeded in obtaining actual photographic impressions on paper, with this remarkable circumstance that the images were direct, that is, without the necessity for the use of a previous negative. In the picture obtained, the lights corresponded to the lights of the object, the blacks to the shadows. His process consisted in exposing paper impregnated with chloride of silver to the action of the light, but only up to a certain point which experience had taught him. When he wished to use it for obtaining a photographic image, he soaked this paper in a solution of iodide of potassium, and exposed it to the action of the light in the camera obscura. The rays of light had the effect of blanching, or rather of giving a very light yellow tint to the silver salt in those parts on which the light shone. There remained only to fix the images by means of hyposulphite of soda. Such is the process of photography on paper which Mons.

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Bayard invented, and which he, for the sake of his reputation, was to blame in desiring to keep secret. It was then that those beautiful impressions were obtained which Mons. Despretz shewed us, fifteen years ago, at his lectures on physics, at the Sorbonne, and which we passed from hand to hand without being able to guess by what magic such marvels had been produced. How could it be guessed that such splendid effects had their origin simply from observing the action of the sun's rays on a peach, and that the delicious gift of the Persian of old had exercised so great an influence on the progress of modern physical science. These very pictures, which so charmed us at the Sorbonne, I think I had the pleasure of recognising in the case sent by Mons. Bayard to the Exhibition. Of course, it is understood that Mons. Bayard no longer obtains his pictures by his old method. He now practises, like almost every body else, photography on glass, but he has carried it to great perfection."

**HELIOGRAPHIC ENGRAVING on MARBLE, on LITHOGRAPHIC STONE,
and on METAL.**

By M. NIEPCE DE SAINT VICTOR.

THIS new memoir on heliographic engraving commences with a fresh study of Judea bitumen and sensitive varnishes.

I. Judea Bitumen.—M. Niepce describes three kinds of Judea bitumen. 1st. A bitumen coming direct from Judea of a dark maroon brown, vitreous and brilliant as jet, without odor when cold, but diffusing a slight odor when heated; this is the most sensitive to light, the best for operations in the camera obscura, provided that it is not too sensitive, in which case it gives cloudy images. 2nd. A very rare bitumen, very dry, of a reddish black in a mass, a red brown when pulverised, with a very bright conchoidal fracture, exhaling an odor of asphalt, specific gravity 1.11, fusible at about 170° or 175°C. (338° to 347° F.), giving no oily matter when distilled, completely soluble in benzine, slowly soluble in essence of turpentine, more sensitive to light than the following. 3rd. A bitumen common in commerce, slightly sticky, of a reddish, yellowish black in the mass, reddish brown when pulverised, with a dull resinous fracture, exhaling a powerful odor of asphalt and resin, density 1.10, fusible at 90° C. (194° F.), giving by distillation more than half its weight of a clear oil which stains paper, completely soluble in benzine, instantaneously soluble in essence of turpentine, which it colors brown; least sensitive to light.

II. Heliographic Varnish for all Operations in Contact.—Take four grammes of bitumen No. 3, the least sensitive; dissolve it in 80 of benzine and in 20 of essence of lemon. This varnish gives all the demi-tints; so that it may resist the action of aqua fortis, it must, before using, be exposed either to the direct light of the sun for about a quarter or half an hour; or to weak diffused light for at least a month. Exposure to light renders it at once more resisting

and more sensitive; practice will teach the amount of manipulation necessary to give it the right proportion of resistance, sensitiveness, and fluidity. When machine engraving is to be used, the most sensitive varnish is the best; for ordinary engraving the least sensitive is preferable.

III. Engraving on Marble and Lithographic Stone.—The marble or stone to be engraved should be very hard, of a very fine grain, uniform in color, without stains, or veins. White Carrara marble has too coarse a grain; still ornaments, with large patterns and flat tints may be engraved upon it. The previously polished stone is cleaned with benzine and alcohol; it is covered with a layer of heliographic varnish, and when this is dry, an ornamental design drawn on thin India paper, or a positive photographic proof on glass or thin paper is put upon it. A longer exposure to light will be necessary than for steel, especially if the stone is black; the action of light is generally slower on stone than on metal. The application of the solvent and washing are the same as for metal, after which, the stone, which has been heated by the rays of the sun, is cooled. The image is engraved by means of water acidulated with very dilute nitric acid; if a photographic proof has been used, it is only very slightly corroded so as to retain all the firmness and demi-tints; a very slight action is sufficient to destroy the polish and give an image by reflection; if an ornamented design has been used, the corrosion must be much deeper, so that the lines may retain the foreign body, mastic or fat ink of different colors to be introduced into them. To go deeply in, it may be varnished a second time with a roller and engraved again; we shall succeed still better by covering the stone with a second layer of heliographic varnish, exposing it to the light for a sufficient time to consolidate the varnish, pouring on the solvent which removes the varnish from the furrows obtained by the first action. The effervescence, or disengagement of bubbles of carbonic acid should take place very slowly, and the bubbles should be very small; the operation must be watched through a lense, and stopped when sufficient. On marble no retouches are possible, it is necessary to be perfect at once.

A design from nature may be taken in the camera obscura on marble or lithographic stone, and engraved as above: if the object is a bas-relief or a medallion, the engraved image will offer considerable relief, especially if the corrosion has been weak. M. Niepce thinks that his process of engraving is susceptible of a great many practical uses. In his opinion, a light engraving on marble, without any colored application should be preferred, for its simplicity and unalterability.

IV. Lithographic Impression on Stone.—Stone to be printed from is operated on in the same manner as marble, with a photographic image; the corrosion is stopped soon for fear of destroying the demi-tints; moreover, very slight furrows are sufficient for printing. After having removed the varnish, the engraving is very carefully cleaned with alcohol and a very soft linen cloth; it is moistened

with ammoniacal water, and the furrows are filled with lithographic ink; the stone is again cleaned and wiped so as leave the ink only in the furrows. Then commences the work of the lithographic printer. He passes over the whole surface of the stone a brush filled with acidulated water, to take off the polish of the free surfaces; he then passes over it a sponge with gum, which adheres to the unpolished surface; the lines of the engraving have been preserved by the fat ink, and when the roller of lithographic ink passes over it, those parts alone take the ink which is then printed with as usual.

If we wish to obtain an engraving in relief, we must operate with a negative and not a positive photographic proof. After having corroded the stone, the varnish is removed, the surface cleansed with alcohol, the gum water is passed over it, the reliefs are cleansed with a cloth steeped in alcohol, and it is inked with a roller.

M. Niepce says that impressions obtained from stones thus engraved, are superior to those obtained from plain lithographic stone.

V. Heliographic Damaskeening.—The first method consists in covering with copper, by means of a battery, a plate of polished steel, on which is spread a layer of heliographic varnish to reproduce an ornamental design, either by contact, or in the camera obscura; when sufficiently exposed, the varnish which has not been modified by light, is removed with a mixture of benzine and naphtha; the surface of copper which has not been exposed, is removed with chromic acid; the copper is gilt by immersion in a suitable bath, and we have as a result, a design of steel on a gold ground.

The second method consists in the application of the sensitive varnish directly on uncoppered polished steel; we then gild by the battery all the parts of the steel covered by that portion of the varnish which has not been modified by the light. The result of these operations is, that we obtain a damaskeening on steel, or even silver, without the aid of an artist.

Cosmos, November 14, 1856.

PUBLIC HEALTH.

On the CHEMICAL COMPOSITION and NUTRITIVE EQUIVALENTS of the FOOD of MAN. By M. POGGIALE.

(Continued from Page 106.)

WHEAT.

MANY chemists have endeavored to determine the constituent elements of wheat; amongst the works published on this important question, those of MM. Rossignon, Boussingault, Dumas, Payen, Peligot, Krockner, Horsford, &c., deserve to be specially mentioned. Having been for some years one of the Committee of military food, I have had to examine a multitude of samples of hard and soft wheat, and I have determined to publish my results. I have already explained the methods which I have employed in the determination of the water, fixed matters, starch, nitrogenous matters and lignine; but the pro-

portion of this last principle being much greater in my experiments, it is necessary for me to press the point.

When wheat is treated successively with dilute acids or alkalis, boiling water, alcohol and ether, the proportion of resisting matter does not exceed 1.5 per cent.; but does this analytical process give exact results? I think not. In fact, slightly aggregated cellulose, such as is found in the interior of the grain, and even a portion of the cellulose forming the first envelope, are dissolved, as I have ascertained, both by acids and alkalis. The lignine of wheat contains, moreover, other substances which are not alimentary—such as coloring, extractive and resinous matters, &c., which likewise dissolve in the separation of the cellulose. I have demonstrated in my work on the chemical composition of bran, that all the nitrogen of wheat is not furnished by the assimilable nitrogenous matters. Thus, a dog was fed for several days on a mixture of broth and bran; the excrements were collected, and consisted almost entirely of bran, which was separated with great facility by washing it on a silken sieve. It was then boiled successively in water, alcohol and ether, until it contained no foreign substance whatever, and finally it was dried at 120° C. (248° F.) The proportion of nitrogenous matter in this residue amounted to 3.516 per cent. In another experiment, bran was analysed which had been given successively to two dogs, and the numbers given were exactly the same. The same bran which had passed through the digestive tube of two dogs was given to a fowl, but the proportion of nitrogen remained unchanged. These results are decisive, and prove that there exists in bran a nitrogenous matter which is not assimilable.

If we separate, by means of diastase, the amylaceous substances from the bran, and if, after washing the residue, we treat it with dilute hydrochloric acid, 100 parts will give by prolonged ebullition 19.563 per cent. of glucose. Now this sugar could only have been produced by the cellulose transformed by the action of hydrochloric acid.

Another experiment which I made recently shows this fact in an evident manner. I separated mechanically the first envelope from the wheat, which I boiled for some minutes in acidulated water, composed of 100 grammes of distilled water and 5 grammes of fuming hydrochloric acid. The residue was then washed, and the glucose contained in the liquid was estimated by the cupreo-potassic tartrate. 100 parts of envelope gave 44.650, and as it did not contain starch it is evident that the sugar proceeded from the cellulose. Wood furnishes analogous results; if we dry 100 grammes of oak wood, containing 18 per cent. of water, and if it is treated successively by dilute hydrochloric acid, an alkaline solution, and distilled water, we find that the weight of the residue does not exceed 46 grammes.

I have found that the method of analysis which depends on the use of acids and alkalis is inaccurate, and that, in the present state of science, the only substance which enables us to isolate the amylaceous matters from the cellulose is diastase. I need not repeat the pre-

cautions necessary. I shall merely mention that I have ascertained the proportion of lignine in wheat* by separating successively with water and ether the substances which are soluble in these two liquids, transforming the starch into glucose by means of diastase, and subtracting from the weight of the residue the quantity of nitrogenous and fixed matters obtained by direct determinations.

The ground wheat, treated by this process, gave a proportion of lignine which was much more considerable than is generally supposed. The following are some of the numbers which I have obtained:—

	Lignine per cent.
White Baltic wheat	4·301
Poulard „	4·525
Hard Spanish „	3·687
Hard African „	3·823
Bordeaux „	4·157
Red American „	4·823
Soft French „	4·629

The dextrine of the starch may be separated without affecting the cellulose, by boiling for a few minutes 25 grammes of ground wheat with a mixture of 300 grammes of distilled water, and 6 grammes of fuming hydrochloric acid. The residue is then washed and dried, the fatty matters are estimated by means of ether, the proportion of fixed and nitrogenous matters are ascertained, and the difference gives the lignine. This process is not quite as exact as the preceding, but it furnishes sufficiently accurate indications. Thus, in one experiment, 100 grammes of soft French wheat gave—

Bran containing neither dextrine nor starch . 8·740

This bran contained—

Nitrogenous matters	2·520
Fatty „	1·798
Fixed „	0·045

4·363

By subtracting this number from 8·740 we have 4·377, which approximately represents the lignine. But we must remark, that boiling acidulated water removes some non-alimentary substances, such as coloring matters.

In several experiments which have required much time and patience, I detached the outer envelope with my hands from some samples of soft wheat which had been previously moistened. They gave about 3·540 parts of dried envelope per cent. of wheat, we could not remove the portion found in the crevice between the two lobes of the grain. This pellicle, when examined through a microscope, represents only cellules, and contains neither gluten nor fatty matter. It contains

* Lignine must not be confounded with cellulose. The latter is a pure proximate principle. Lignine, on the contrary, is formed of cellulose, and of a matter which M. Payen terms *encrusting*, whose composition is complex, but which does not contain alimentary matter.

2.120 of fixed matters and 3.380 per cent. of non-alimentary nitrogenous matter, which may be compared to that contained in the straw of wheat, rye, oats, barley and peas, in wheat husks and in many kinds of leaves, wood, &c. Under this pellicle, which appears to be formed of several ligneous and unassimilable layers, and which, as we have seen, is almost entirely formed of cellulose, are found other integuments united by a gummy matter easily dissolved in water.

It is difficult to separate this second envelope, and I have been unable to determine its proportion. What I did get off, did not contain starch, and only gave 6.370 per cent. of nitrogenous matters.

According to M. Mouries, wheat is composed of three envelopes; 1st. The very light ligneous epicarp, weighing 2 per cent. of the weight of the corn; 2nd. The endocarp covered by debris of the sarcocarp charged with yellow extractive matters and essential oil; this membrane weighs 3.20 per cent.; 3rd. The adherent episperm, very nitrogenous and colorless, weighing 3.3; the embryo and endosperm giving together 91.5 per cent.

All attempts at the decortication of wheat which have been made lately, and especially those of M. Sibille, confirm my experiments. In fact the results of the decortication made at the military establishments, prove that we can remove from soft wheat about 3 per cent. of pellicles. These pellicles do not contain starch. Soft wheat thus decorticated, gave as an average 22 per cent. of bran. Thus we find that the proportion of lignine contained in wheat is much more considerable than we have hitherto supposed. This is a very important fact, to which I desire to call the attention of chemists and hygienists. If, as I have shown, soft wheat contains 4.5 per cent. of lignine, we shall understand the necessity for bolting, and the difference which exists between white bread and ration bread, especially as it used to be prepared. Bolting would be a foolish and expensive operation if it removed only 1 per cent. of cellulose.

This larger proportion of non-assimilable matters contained in bran, therefore justifies its elimination from the flour, and accounts for the loss produced by bolting. The separation of the bran does not lessen its nourishing qualities, and is not, as has been observed, a mere luxury. In fact, every one knows that when flour has been left unsifted, or only to five or ten per cent., the bread made with it is brown, heavy, close, with an untempting appearance, with a sour taste, and difficult of digestion. Bakers have observed that wheat-flour containing the bran, absorbs more water, and produces more bread than white flour. And Parmentier, who has studied so carefully all questions relating to panification, declares in all his works, that bran, however well divided, makes weight, but not bread, and that it is a great obstacle to the manufacture of that aliment. It passes entire, he says, without digesting; likewise, when bread has been prepared with whole flour, or bolted to five or ten per cent., the fecal matters of the soldiers contained considerable quantities of bran.

Daily observation teaches us that the episperm of seeds, and the epicarp of fruits, are very refractory to the digestive action, and

sometimes prevent the assimilation of the nutritive portions. Thus, peas, beans, lentils, cherries, gooseberries, and grapes, will pass unchanged through the digestive tube, when not divided by mastication.

Still, I think that 20 per cent. is sufficient to remove from the flour, the bran then left would probably be useful to retain the assimilable matters longer in the digestive organs. I am disposed to admit that the nutritive principles require to be mixed with more refractory matters. This would be the value of bran if mixed in *proper proportions* with flour in bread.

With too light a white bread, robust young men, exposed to hard labor, like our soldiers, would not be so well nourished, as with a little coarser bread. Bran is likewise useful for the non-assimilable matters which it contains; it augments the volume of the fecal matters, and thus renders the digestive functions more easy and more regular.

According to my experiments, the composition of hard and soft wheat is as follows:—

Starch and dextrine	.	.	.	.	.	63.80
Nitrogenous matters	.	.	.	.	.	14.40
Fatty	"	.	.	.	.	1.90
Fixed	"	.	.	.	.	1.70
Lignine	.	.	.	.	.	4.20
Water	.	.	.	.	.	14.50
						100.00

(To be concluded in our next.)

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS

On CAUSTICS in a CHEMICAL POINT OF VIEW.

By M. E. FERRAND.

(Concluded from page 122.)

The eschars, considered Physically and Chemically, and with respect to their Development.

The eschars are plainly distinguished from each other, especially with respect to the group of caustics which produce them.

1. ESCHARS IN A PHYSICAL POINT OF VIEW.

The eschars produced by *alkaline caustics*, at first very soft, become firmer, translucent, soft, and not greasy to the touch.

Those produced by *acid cauterisation*, at first more or less vapid, solidify a little, remaining soft and greasy; the effect of the caustic is seldom complete, and they are the most voluminous and the heaviest.

Those produced by a metallic salt are opalescent or opaque, dry, and hard; they grease blotting-paper after a long time, but not so much as those produced by acids; their color is various and they are usually smaller both in weight and volume.

The eschars obtained comparatively on clots of blood have given me, when dry, the follow weights :—

With nitric caustic	4.9	With antimonie caustic	1.5
„ sulphuric „	4.0	„ acid mercurial „	1.3
„ potassic „	2.2	„ gold „	1.2
„ calci-potassic caustic	1.8	„ silver „	0.9
„ stannic „	2.1	„ mercurial „	0.5
„ zinc „	1.65	„ copper „	0.4

2. ESCHARS IN A CHEMICAL POINT OF VIEW.

After previous washing, the water of maceration is still alkaline with alkaline caustics, and acid with either acid or metallic caustics.

In the last lixiviations, which are nearly neutral, acid reagents precipitate those of alkaline origin, the alkaline carbonates precipitate those proceeding from acid cauterisation, nitric acid alone excepted. Moreover, analysis shows in the others, the metallic salts employed, even when we take the precaution of destroying the organic matter which accompanies them.

I will here state that I have always found in the washing water, the elements of the salt united to a certain quantity of organic matter, whence we may conclude, without further detail, that a part of the eschar is susceptible of being dissolved in water.

I wished to ascertain whether it were the same with eschars on the living being, and I obtained the same results; I analysed eschars given to me, among which were two by M. Bonnet, a varicocoele and an erectile tumor; from this latter, which was not less than 2 centimetres in thickness, I detached from the lower part, 3 decigrammes of matter which, previously washed, macerated and pressed out several times, gave me the base of zinc caustic used.

This study of the action of water is not a question of pure chemistry proved by laboratory experiments; for it is in the highest degree interesting, because of the surgical questions connected with it; thus, with respect to the intervention of water in the application of caustics, I have ascertained that it is sufficient to change completely the effects of certain metallic salts; indeed, under its influence, chloride of antimony acquires, as well as nitrate of silver, the property of attacking the skin; then, in dressings, I have perceived that its application in the form of moist compresses, cataplasms, &c., considerably weakens the effects of the acids in particular. Its presence, indeed, has always appeared to me to render the action of the caustic more general by aiding the penetration of the agent, but to diminish the intensity of the effect, facilitate the absorption, and afterwards soften the eschars, and to prepare for their separation.

3. ESCHARS WITH RESPECT TO THEIR DEVELOPEMENT.

In studying the developement of the eschar, I have made some experiments, the results of which appear to me interesting and easy to follow.

For this purpose, I deposited on the surface of a thick, transparent albuminous liquid, a species of artificially commenced eschar; but as

the extreme softness of this nucleus, produced by the mixture of blood and certain caustics, causes the experiment to be not always practicable, we may, as I have done, use a mixture of one part caustic, and two parts flour, whether moist or not, so as to obtain a sufficiently soft ball.

These opaque and caustic balls, suspended by means of thread like large pearls in the upper portion of a very transparent albumen, render manifest some obscure conditions of cauterisation.

Some develop rapidly, some remain stationary for some time; the following increase by active and voluminous juxta-position, nitric acid, chloride of zinc; some give a cloudy aureola, containing gaseous bubbles, whose disengagement, although weak, suffices to tear their thin envelope; such are sulphuric acid, gold, and antimoniac caustics, and acid mercurial caustic; some have a comparatively slow development, without much aureola; caustics of silver, chloride of mercury, copper, and tin, especially the last three, dissolve at last and more speedily with agitation, the aureolas remaining for several days, but being destroyed at last. We must observe that the same occurs on the living being whenever caustics with distinct characteristics are used.

In these experiments, the caustic, when placed in a soft, transparent albuminous medium, pursues definitively, regularly, and in an evident manner, its chemical action, until complete saturation, and then, after perhaps two or three days, the increase of the globule stops, and the albuminous liquid in its turn attacks the last layers produced, with a dissolving action, first renders them cloudy, then absorbs them, and at last, in some instances, destroys the nucleus itself. To give an idea of the different development of some, I will give some examples.

Silver Caustic.—After one hour, the volume doubled, circumscribed, and very firm. After six hours, the original volume increased three times. After twenty-four hours, had not further augmented sensibly.

Sulphuric Caustic.—After one hour, volume tripled, effervescent and translucid. After six hours, separated and fused. After twenty hours, had extended, but without consistence or form.

Nitric Caustic.—After one hour, volume quadrupled, effervescent, and cloudy. After six hours, separated and fused. After twenty hours, still more extended, but without energy or form.

Zinc Caustic.—After one hour, volume increased five times, very circumscribed, and firm. After six hours, volume six times as large, like a snowball. After twenty-four hours, still acting, but without much cohesion.

To appreciate the method by which these artificial eschars are developed, I turned to profit the reducing action of light on the silver globule, and I found in the coloration produced by light and the absence of all color in the shade, a means of experimenting, at once curious and certain. After developing for an hour in the shade, the white eschar, exposed to the light, became rapidly colored, sometimes on one side only, according to the direction of the luminous rays.

Again put into the shade, it was increased by fresh white layers thus, by alternating light and shade we can multiply these interesting proofs, and ascertain definitively, that if the original nucleus retain, its volume almost unaltered, that it must be juxtaposition by which its increase is principally effected.

Its progress is, therefore, not due to intussusception, nor in virtue of a catalytic action, but by the direct action of the caustic, until saturated.

All the experiments mentioned in this chapter have a certain value, inasmuch as they confirm the facts and opinions previously advanced as to the homogeneity, consistence, height, circumscription, and development of the eschar, and throw fresh light on its physical and chemical properties, its progress, aureola, its partial solubility, and, finally, on its absorption, which is the next point to consider.

I must here mention one rule, namely, *the depth of the eschar is in inverse ratio to the contracting power of the caustic.*

THE ABSORPTION OF CAUSTICS.

The absorption of caustics applied to the cellular tissues is certain, and the astringency of some caustics is not an obstacle to their absorption, as has been supposed. Silver caustic is the only one which appears to offer any perfect resistance, for I do not think that there is any absorption of this in the living subject.

Concentrated alkalies and acids, whether combined during the cauterisation or not, do not give rise to any dangerous absorption, for they only require to be a little diluted with an aqueous matter secreted or otherwise, to lose their causticity, and they cannot penetrate into the circulation without producing saline combinations different from those already found there. The organic compounds which constitute the eschars, are all more or less soluble, but they are all inert.

The metallic caustics in general form very stable compounds, for I have seen them produced notwithstanding the presence of large proportions of alkalies. The magma is then entirely dissolved without any precipitation of metallic oxide: this is especially the case with antimony, copper, iron, mercury, and zinc, and it is to this circumstance that M. Dumas attributes the absorption of the metallic salts or oxides by the intestinal canal or the skin: they are then, he adds, carried in a state of solution into the serum of the blood to be afterwards eliminated with the excretions.

Even without the alkalinity of the blood and certain secretory liquids, I have observed that the most intimate combinations of the caustics with the tissues, were not absolutely insoluble in distilled water.

With some absorption must be still easier, such as copper, and mercury, or organic compounds, on which an excess of albumen has a solvent power. However definite may be the mode in which absorption takes place, we know that it is confined in small limits, and the organic compounds with which the metallic salts are combined, lessen their poisonous properties; Orfila says, that the phenomena produced

by caustics are precisely similar to those of a burn, and that sometimes we remark symptoms which would appear to belong to an affection of the stomach or intestinal tube. M. Bonnet has ascertained frequently that there is no danger to the patient with chloride of zinc applied extensively and repeatedly.

Caustics are, in reality, eliminated in various ways, and especially by the intestinal canal, as has been observed by Magendie, with antimony; by Orfila, with copper; by Chatin, with arsenic, &c.

This is especially true in cases of poisoning, for then the intestines appear to supply the eliminatory action of the kidneys and skin, when these are insufficient or, perhaps, suspended.

Their elimination, at first evident, soon becomes insensible, for we know exactly, from the experiments of M. Orfila's nephew how well they are tolerated by the economy in their combination with our principal organs, and how long they are retained in the system, ten or fifteen days in some instances, two, six, and eight months in others.

This elimination is of great importance when circumstances require a long continued course of arsenical cauterisation, so as to avoid a poisonous supersaturation. Finally, absorption, by drying the eschars, renders their color brown.

Compte Rendu de la Société d'Emulation des Pharmaciens de l'Est.

DIABETES MELLITUS. By JAMES GRAY, M.D., Fellow of the Faculty of Physicians and Surgeons, &c., Glasgow.

(Concluded from page 128.)

I HAVE been led, from my own personal experience, to classify diabetes under three heads:—

1. Where the stomach is primarily affected, as has been shown by the experiments of M'Gregor (to whom I am indebted for my first clear views on this disease), and since by M. Bouchardat, who, as already stated, discovered a substance similar to diastase, and possessing the power of converting starch into sugar.

2. Where the liver is primarily affected, as has been prominently brought out by the experiments of Bernard. He has shown that, besides its use in the economy as an eliminator of bile, the liver is a sugar-forming agent, and that this power rests with the tissue of the organ, which seems to act the part of a ferment on the nitrogenous fluid circulating in the body, and from which sugar is eliminated; thus one equivalent of protein, fifty-two of oxygen, and four of water, may be represented by two equivalents of grape sugar, three of urea and eighteen of carbonic acid.

In support of such a doctrine, I may state that I have kept patients for weeks on a strictly animal diet, where not a single atom of starch or sugar was taken, and where not a particle of sugar could be detected in the fæces, and yet sugar was excreted with the urine in large quantities.

3. Where both stomach and liver are simultaneously affected.

I am inclined to term the first diabetes gastricus, the second diabetes hepaticus, and the third diabetes gastro-hepaticus; and have adopted such a division the more, that it does not locate the disease in one special organ, believing, as I do, that something of truth may be found on either side.

In diabetes gastricus, where the hydro-carbons come into play, and where uncomplicated, I believe the rennet treatment will be found eminently successful. The mode I adopt and recommend of preparing the rennet is the following:—The stomach of a calf (and the younger it is the better) is gently washed with water, taking care not to injure the mucous membrane; it is then well salted, tied up, and allowed to dry. After this, it is cut up into small pieces, macerated in a pint and a half or two pints of water, according to the size of the stomach, for four days, or longer in the winter, shaking it at intervals; the fluid is then poured off and bottled; and to test its power, a spoonful may be added to a pint of warm milk, which if it curdles, it is now fit for use. Some stomachs are better than others, in which case the preparation is much stronger and more efficacious. This depends materially upon the honesty of the party from whom the bag may be procured; for I have been told by a respectable butcher, that the parties who are in the habit of selling bags for this purpose, not unfrequently take a dozen or so of stomachs, macerate them with salt and water, pour off the fluid, and dispose of it to farmers for dairy purposes; the stomachs are then tied up, and passed off as genuine first-hand articles. I may also call attention to a fact, well known to dairymaids, that after a thunderstorm rennet is useless to curdle milk—thus showing that a certain electric condition of the atmosphere exerts an influence upon the fluid. A little spirits, or decoction of asparagus, may be added to make it keep. The dose of rennet thus prepared is a table-spoonful, three, four, or six times in a day, usually about half an hour after each meal, and during the process of digestion—followed shortly after by an alkali, to neutralise the lactic acid formed. That which I recommend is the alkaline tribasic phosphate of soda; but the carbonate of potash will answer very well, either alone, or combined with the tincture of nux vomica, in five or ten drop doses. The nervous system in these cases seems at fault, and the nux vomica here serves as a stimulant and valuable tonic. In many of the cases treated, I have further recommended the inhalation of chlorine, from the power it possesses of destroying putrefaction, and arresting the fermenting process. In all, I have now treated twenty-eight cases of diabetes by rennet, of which nineteen were males and nine females. Of the males were five gentlemen following no profession, two youths, one chandler, one laborer, one engineer, one wright, one ploughman, one policeman, and six farmers; females, one mill girl, three maid servants, and five domestic married females. Of these, seventeen were more or less connected with agricultural pursuits. The ages varied from eleven to sixty-five years; with three exceptions all were in comfortable circumstances; eleven were in the habit of smoking, or using tobacco in the form of snuff; and although a few occasionally

indulged to excess in alcoholic liquors, this was not by any means a general feature.

The complications of most frequent occurrence I found to be phthisis; next, dysentery, and, lastly, anasarca, depending upon remote disease of the kidneys. The diet which I advise at first is strictly animal; and where the poverty of the patient does not permit of the free and generous use of the better quality of butcher's meat or eggs, I recommend the heart, lungs, liver, and kidneys to be substituted, as many good and savory dishes can be made from these organs; a soup made from the liver answers admirably in the troublesome constipation which accompanies this disease. Bread free from starch may be made as follows, according to the recommendation of Prout and Christison: six eggs beat up, to which are added four ounces of butter, make into cakes with finely-ground bran. An excellent pudding is also made with curds, well washed, dried and powdered—butter and eggs—which is salted and spiced to taste. Salted fish makes a good change, and does not increase the thirst so much as might be supposed. A variety of vegetables may be also given, as greens, kidney and French beans, and nettles even make an excellent dish, lettuce, artichokes, &c. As it is impossible to keep a patient for any length of time on a strictly animal diet without doing injury, I therefore add, after a little while, bran bread as found in the baker's, which I find quite equal to the gluten bread as it is made in this city, and much less expensive.

If we consider the slow and insidious approach of diabetes, and that it generally has a firm footing before the patient is alarmed into taking advice upon his increasing symptoms, and that for weeks or months the true nature of his complaint may remain undetected even by the physician, we will not be surprised that it should take a considerable time to be entirely removed. The shortest period in which I have found sugar to disappear has been two weeks, the average from that to nine months. The importance of air and exercise cannot be over-estimated, as they are indispensably necessary to improve the general health. Hence, I do not consider an infirmary a proper place for treating a diabetic patient. Attention to the functions of the skin is no less requisite, for which purpose the tepid bath, followed by friction should be used, as also anointing the body with oil. The patient being usually melancholy, and a prey to gloomy apprehensions, it is of paramount importance to gain his confidence, and if possible infuse hope into his desponding heart. This the physician should know how and when to do in each respective case. In a word, nothing should be left untried which might tend either to improve or invigorate the body, or restore serenity and hope to the mind.

Where diabetes is complicated with phthisis, I prescribe cod oil and morphine, and although I have never known such a case to recover, I have found such much benefited. I have also seen this disease follow as well as precede consumption. The preparation I have used with most success in such cases was a solution of phosphorus in chloroform, with the tincture of benzoin. Of the twenty-eight persons treated, seven have recovered; that is to say, they have regained the weight

lost, their urine was diminished in quantity to about the usual standard, and contained no sugar, and they expressed themselves as being "fit for their work, and in as good health as they ever were." Three died, two from disease of the chest, and the other from dysentery, complicated with effusion of pus in the left pleura, and seven still remain under treatment. Of the remaining eleven, while I have lost sight of several from the fact that they were at considerable distance from town, I hold letters from others, expressing their thanks for benefit derived from the treatment. In thirteen of those cases, organic disease, and chiefly of the lungs, could be detected. I may finally remark, that only a few of those cases had not been previously under the treatment of other medical men, so that there could be no mistake in the diagnosis.

Case I.—The countenance of John Dryburgh, aged 44, was expressive of great anxiety on the 7th of July. He was much emaciated; the pulse was 66, full and regular; the respiration slow; the mouth dry, in consequence of which he was unable to articulate distinctly; the tongue coated with a thick yellowish fur; the appetite good, and the thirst excessive, so that he drank upwards of three gallons of liquids in the course of twenty-four hours. The urine was very abundant, of a bright straw color, sweet to the taste, loaded with sugar, as indicated both by the sulphate of copper and the caustic potash tests, and in density 1045.

He was ordered a tablespoonful of creasote mixture, to be taken when the thirst was urgent; and ten grains of the alkaline phosphate of soda, three times a-day, in a glass of water. His diet was made to consist of beef, fish, tea without sugar, eggs, bran bread, buttermilk, and water. Under this course of treatment, and attending to the state of the skin and bowels—alternating the creasote with tincture of nux vomica, and tincture of colocynth—he continued for three weeks, with little improvement in the general symptoms, during which period the density of the urine varied from 1045 to 1037.

On the 21st Aug. he felt decidedly better. The thirst had greatly abated, the bowels were regular, the tongue clean, and he slept well. The quantity of food and drink now used in twenty-four hours was 3 lbs. of meat, $\frac{1}{2}$ lb. brown bread, seven cups of tea without sugar, eight ounces of water, and half as much butter-milk. The density of the urine was 1024, and there were only slight traces of sugar. The alkaline phosphate of soda was discontinued, and a few drops of muriatic acid were given along with the rennet.

He had a smart attack of diarrhœa on the 24th. On the 25th he gave up taking the rennet. The tongue was slightly coated with a yellowish fur, and the stools black-colored. He slept well. The quantity of urine passed in twenty-four hours was 64 ounces, and its density 1022.5.

I could not detect the slightest trace of sugar by the caustic potash test. The uric acid was present in lozenge-shaped crystals. Lactic acid was found by Pelouze's test. On boiling the urine with caustic potash, it became turbid; and when allowed to stand, a deposit fell to

the bottom of the test tube, which, when placed on a slip of glass, partially evaporated, and examined under the microscope, presented numerous crystals, some of them much resembling a sheaf of corn, and others arborescent. This deposit is dissolved on the addition of a solution of prussiate of potash.

On November 8th he felt quite well; had gained about two stones in weight since commencing the rennet. The urine had a density of 1018, and no sugar could be discovered in it by any test. He was taking the ordinary mixed diet, and continued the rennet.

Case II.—W. G. On the 11th of October, 1852, I found the urine to contain a large quantity of sugar, and its density to be 1044. He complained much of pain in the legs and fore-arms. The tongue was furred; and his articulation indistinct. He had lost a stone and a half in weight. The pulse was 78. There had been no cough.

He was ordered ten grains of the alkaline phosphate of soda three times a-day in water, and to live as much as possible on an animal diet.

13th.—Density of the urine 1040. A teaspoonful of rennet to be taken after each meal.

December 24th.—I directed him to use chlorine inhalation. I was led to make use of this remedy by the following circumstance. Conversing one day with John Dryburgh, he happened to remark, that, when he had occasion to go in the chlorine-room during his illness when his thirst was urgent, a free flow of saliva took place, which immediately afforded him relief. Under the impression that the chlorine might both act on the sugar circulating in the blood, and also in some way on the nervous system, I was induced to try it in Clark's case.

For ten days or a fortnight before using it, this man passed urine varying in density from 1030 to 1040. In a fortnight after first using it, the density fell to 1020, and every trace of sugar disappeared. With exception of an accident he met with some four months ago, he has enjoyed uninterrupted good health.

Case III.—On the 15th of April, 1854, J. H., aged 45, came under my care. Altogether he had lost three and a half stones in weight, and his countenance was expressive of great anxiety. Average quantity of urine in the twenty-four hours, fifteen English pints (although he has passed nearly double that quantity), is very pale in color, sweet to the taste; and when tested, exhibits sugar in large quantities. Has a urinometer, and frequently himself takes the specific gravity during the twenty-four hours; average of which is 1045, maximum 1057. Skin dry and furfuraceous, burning sensation in the palms of the hands and soles of the feet. Mouth dry and clammy. Unable to articulate distinctly, from the parched condition of the mouth; during the day frequently feels the flow of saliva, and so decidedly acid as to affect the teeth. Complains of pain at the pit of the stomach; pulse 74, full and regular; breathing also regular; and can detect no abnormal sound on either side of the chest; mind anxious, very nervous; and the power as well as desire of sexual intercourse diminished. To

take a tablespoonful of rennet four times daily, followed in half an hour by fifteen grains of the alkaline tribasic phosphate of soda, animal diet, air and exercise, with attention to the skin.

May 23d.—Has gained in weight 7 lbs. Urine, when tested, not so dark in color; specific gravity 1035; and the desire to void it neither so frequent nor urgent. Thirst moderate; complains still of burning sensation in hands and feet; perspires freely; urine slightly saltish, and still containing sugar. To have bran bread added to the diet, and continue the treatment. This gentleman remained under my care for seven months; at the end of which period I was unable to detect sugar in his urine. He had fully regained his lost weight. The quantity of urine passed in twenty-four hours varied from four to six pints. His thirst was easily satisfied, his mind at ease; and he could attend regularly to his business. His diet consisted of the common bran bread, fish, butcher's meat, and green vegetables. Six weeks ago I carefully examined his urine, but failed to detect sugar. Two weeks ago I met him in the street, when he informed me "he was quite well."

Before concluding the present paper, I shall briefly advert to a few of the theories that have been promulgated on this subject.

Bouchardat believes that the amylaceous matters alone are converted into sugar, and that by the agency of a substance similar to diastase; that the quantity of glucose bears a direct proportion to the amount of starch taken, and that the disease exhibits itself by a sudden interruption of the acid secretion of the skin, and that it depends upon an acid state of the blood.

Bernard locates the disease in the liver, and considers it an exaggeration of the physiological function of that organ, which may depend upon one of two causes, viz., first, the lungs may be unable to perform their appropriate function; and, second, the glucogenic power of the liver may be increased, the matter absorbed from the intestines keeping up the excitability.

Mialhe holds that the blood is not sufficiently alkaline.

I had the pleasure lately of perusing an able MS. on this subject by Dr. Brydon, who maintains that it depends upon a free acid in the blood, and instances numerous cases in support of his view. He is of opinion with Prout, that oxalic acid diathesis passes into diabetes, and also that a long continuous and free use of acid drinks leads to this consummation. The experiments of Dr. Pavy go to prove that, when the process of digestion is active, the sugar is supplied by the liver to the lungs in such quantity that the whole is not converted into lactic acid. A portion enters the general circulation, although not to an extent to be recognised in the urine; but when the quantity is increased, and the blood becomes surcharged, that it is then easily detected in the urine. On the other hand, when the process of digestion is not active, he believes that the whole of the liver sugar undergoes the lactic acid catalysis; that this change takes place in the lungs chiefly, although it goes on in the general circulation, and in the vessels which supply the abdominal viscera. His

researches have, therefore, led him to adopt the theory, that the sugar produced in diabetes by the liver has a resemblance to vegetable rather than animal sugar, on which the normal ferment does not produce any catalytic influence.

For my own part, I am convinced that something in the form of a ferment must be *first* introduced into the stomach—that this substance increases, is ultimately absorbed, and enters the circulation—that it there gains ground from the food introduced into the stomach, and by the disintegrations of the tissues—and that, if not arrested, it only ceases when its nature has been overcome. In proof of this, as has been already mentioned, we know of many nitrogenous substances in a state of putrefaction capable of inducing fermentation. It is, therefore, not unreasonable to suppose, that the proximate cause of diabetes is a ferment *introduced* into the body. If such then is my opinion, how comes it, it is asked, that I recommend rennet in this disease? I reply, that I endeavor to imitate the process which goes on in the healthy state of the body, to the end that by time, and attention to all the functions, this ferment may be so neutralised, that a return to health may be looked for. I am confident that to destroy this ferment would be to remove the disease, and in various ways I have tried to do so, but hitherto without success. This, however, should not preclude us from adopting the next alternative of modification, and this I have experienced is best effected by rennet. I have tried the alkaline salts and opium, but with as little success as those who hold them up to admiration.

Glasgow Medical Journal, October, 1856.

On COPPER in the OYSTER. By Signor BIZIO, of Venice.

In the year 1853, I showed by experiment, that oysters contain copper in considerable proportion in the branchia, and in very small quantity in the mantle; and that this did not depend on the locality in which they are found, even when that is a calcareous rock. I have even found copper in the spiria of the murex, and in many other shells.

After having discovered copper in the branchia of the oyster, I was led to think that the green color sometimes seen in these animals was probably produced by the copper which they contained, and that it had its origin in the pathological condition of the animal. I therefore left the branchia to putrefy, and they soon acquired such a green and blue color, by the action of the ammonia which was disengaged, that I felt obliged to believe that in the fact observed by M. Valenciennes, there was a similar phenomenon produced in the living animal, especially as the reaction of acids and ammonia on the green matter of M. Valenciennes, accords well with that of the copper which still forms part of the organic molecule.

Journal de Chimie Médicale, October, 1856.

On the ACTION of URARI and STRYCHNIA on the ANIMAL ECONOMY.

By Professor ALBERT KOLLIKER, of Wurzburg.

THE communication which I now offer to the Royal Society contains a brief statement of the results of a series of experiments which I lately made on the action of the urari poison and of strychnia on the animal economy.

I. URARI.

The urari is the well-known poison from Guiana, also called curare and woorara. That which I employed in my experiments I owe to the liberality of my friend Professor Christison of Edinburgh. The following are the conclusions at which I arrived respecting its operation :—

1. The urari causes death very rapidly when injected into the blood or inserted into a wound; when introduced by way of the mucous membrane of the intestinal canal its effects are slow, and require a large dose for their production, especially in mammalia. When applied to the skin of frogs it is altogether inoperative.

2. Frogs poisoned with very small doses of urari may gradually recover, even after it has produced complete paralysis of the nerves. Mammalia may also be restored, even after large doses, provided respiration is maintained artificially.

3. The urari, *acting through the blood, destroys the excitability of the motor nerves.* In frogs under its operation the terminal branches of these nerves within the muscles lose their excitability in a few minutes, whilst their trunks become affected an hour or two later. If, after the nervous extremities have become paralysed, the heart of the animal be excised so as to prevent the nerves from receiving any further share of the poison, the nervous trunks may retain their excitability for three or four hours.

4. The brain is less affected by the urari than the nerves in the muscles; still when, by the ligature of the two aortic arches, in frogs, the poisoning is confined to the anterior half of the body, the voluntary movements of the limbs speedily cease, whilst automatic movements, of doubtful nature, and probably proceeding from the medulla oblongata, may be still observed for half an hour or an hour after the poison has begun to operate.

5. The spinal cord is considerably less affected than the brain by this poison; and by local limitation of the poisoning (as in No. 4), it is found that the cord retains its *reflex activity* from half an hour to an hour and a half, and the excitability of its white substance or its conducting power from two or three hours after the poison has taken effect. It is worthy of remark that in such cases the impaired reflex activity of the spinal cord may be revived by strychnia directly applied to it.

6. The *sensory nerves*, as shown also by locally limited poisoning retain their functional activity as long at any rate as reflex actions can be excited; and when the depressed reflex activity has been revived

by means of strychnia, these nerves are found not to have been in the slightest degree injured, so that it seems doubtful whether the urari in any way affects them.

7. *The nerves of the involuntary muscles and of the glands* are also paralysed by the action of urari, at least I find this to be true in the following cases, viz.:—

a. *The pneumogastric*, as regards its influence on the heart.

b. *The sympathetic* (its cervical portion), in its relation to the iris.

c. *The nerves of the posterior lymph-hearts* of the frog.

d. *The nerves of the vessels* in the web of the frog's foot.

e. *The planchnic nerves* of the rabbit, as reflecting the peristaltic motions.

f. *The nerves governing the secretion of the submaxillary gland* in dogs.

1. *The voluntary muscles* remain perfectly excitable, but show a greater tendency than usual to merely local contractions. In general the cadaveric rigidity of these muscles appears to set in later than usual.

9. *The plain or non-striated muscles* also remain long irritable after poisoning by urari.

10. The heart, in amphibia, is little affected by urari. Its pulsation as well as the circulation of the blood goes on regularly for many hours after the poisoning is established. The only thing worthy of note is that the beat of the heart appears to be somewhat quickened, probably from paralysis of the pneumogastric nerves. In frogs poisoned with urari, the heart, when cut in two, shows the usual phenomenon, namely, that the half which contains the ganglia continues to pulsate, whilst the other does not; from which it may be inferred that these ganglia are not paralysed. As to the nerves in the substance of the heart, those at least which are derived from the pneumogastric are unquestionably paralysed (*vide* No. 7.)

11. The lymph-hearts of frogs poisoned with urari soon cease to move.

12. The blood of animals poisoned by urari is fluid and dark, but coagulates when drawn from the vessels, and forms a weak clot, which is but little reddened by exposure to air. Directly mixed with blood, urari does not prevent coagulation, but the blood in this case also remains dark, and scarcely reddens on exposure.

13. The blood of animals poisoned by urari has the same poisonous qualities as that substance itself, but not in a degree sufficient to produce the full effect of the poison. Urari when directly mixed with blood loses none of its efficacy.

14. Urari, in concentrated solution, applied locally to nerves, extinguishes their excitability, but only after a considerable time, and it appears to act similarly on the nerves in the substance of the muscles. Dilute solutions have no injurious operation. Applied directly to the brain and spinal chord, urari is altogether harmless, provided its absorption be prevented.

15. When artificial respiration is kept up in quadrupeds poisoned with urari, I find that, as observed by Bernard, many of the secre-

tions become increased—as the tears, saliva, urine, and mucus of the air-passages—which effect appears to be owing to the paralysis of the vascular nerves, and consequent dilatation of the vessels caused by the poison.

16. In mammalia urari causes death by paralysis of the respiratory nerves and suppression of the respiration, which brings on convulsions in these animals as a collateral effect. In frogs the final extinction of the functions may also be ascribed to suppressed action of the lungs and defective oxidation of the blood, which at length renders the heart unfit to perform its office; but it must be observed that in this case the cause of death is not so plain, inasmuch as in these animals the functions are in a great degree independent of the pulmonary respiration.

II. STRYCHNIA.

Some experiments with strychnia (the acetate) gave the following results:—

1. Strychnia has not the least influence on the peripheral nerves through the blood, which is best shown by cutting the nerves before administering the poison.

2. Strychnia paralyzes the motor nerves of the voluntary muscles by exciting them to too energetic action,—a paralysis which may be compared to that caused by powerful electric currents acting upon the nerves. In frogs, when the tetanic spasms are over, the nerves often show no trace of excitability; in mammalia they generally retain it in a slight degree, but never show the same energy of action as when uninjured.

3. Strychnia does not affect the sensory nerves.

4. The heart is not affected by strychnia, not even during the tetanic spasms, with the exception only that its pulsations are sometimes a little slower during the tetanic state. On the contrary, the lymph-hearts of frogs contract themselves as soon as the tetanus begins, and remain in this state as long as the spasms last.

5. The tetanic fits can be brought on in two ways; first, through *the sensory nerves*, which, by irritating the gray substance of the spinal cord, produce the tetanic contractions as reflex movements; and, secondly, *through the brain*, which is not affected at all by strychnia, and preserves its powers of volition and sensation. Accordingly, animals poisoned with strychnia try to move in the ordinary way, but every attempt brings on a tetanic fit, so that it is plain that the spinal cord may also be excited by the brain to its peculiar actions.

6. If the tetanus produced by strychnia has been strong, the *muscles are less irritable*, and pass much sooner into the state of *cadaveric rigidity*, which is very strongly marked, and seems to last longer than it generally does. The same early onset of rigidity may be observed in animals killed by tetanus excited by electricity.

Proceedings of the Royal Society, London.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the PRESENCE of IODINE in ATMOSPHERIC AIR.

By THORNTON J. HERAPATH.

THERE is no subject, I believe, which has created so much discussion of late years, amongst scientific men, as the alleged discovery, by Chatin, of the presence of iodine in the atmosphere. It is now about five years ago, that M. Chatin in a memoir that he read before the French Académie des Sciences "on the presence of iodine in the air, and on the absorption of that element in the act of respiration,"* announced that he had succeeded in detecting iodine in the atmospheric air, by a peculiar process which he described; and in two other papers which he subsequently published "on the absence of iodine in water, and alimentary substances considered as a cause of goitre and cretinism" and "researches upon iodine in the air, water, soils, and alimentary products of the Alps, of France, and of Piedmont," which appeared in the 23rd and 24th vols. of the *Comptes Rendus*—that chemist attempted to prove that the air of Paris contained at least one-five-thousandth of a milligramme of iodine in 4,000 litres, and lost four-fifths of its iodine during respiration; and that it is the absence of that element in the air and water of certain districts that occasions those frightful disorders termed goitre and cretinism. M. Chatin's results were submitted to the consideration of a committee, appointed by the Académie, consisting of several eminent continental savans, who fully corroborated them, but Mr. S. Macadam and many other chemists, who have repeated Chatin's experiments, have failed to detect the least trace of iodine in the air.

Believing that if iodine were present in the media referred to, viz., the atmosphere, rain, water, dew, and snow, its neighbor element, bromine, would be found there too, I was induced to undertake the investigation, partly with the hope of being able to corroborate the results communicated by M. Chatin in reference to iodine, but principally with a view to determine whether an appreciable quantity of bromine was present in the atmosphere. Several minor experiments, in the course of which I subjected to examination quantities of air,

* Proceeding of the Botanical Society of Edinburgh, July 8th, 1852; Quarterly Journal of Chemical Society, VI, 166. See also translation of papers, by Ad. Chatin, "on the iodine in the atmosphere contained in dew," *The Chemist*, vol. 1, p. 76, "on the pretended occurrence of iodine in milk, eggs, and the atmosphere," by D. Lohmeyer, *Phil. Mag.* vi, 287, and an abstract of a paper "on the distribution of iodine in the mineral, animal, and vegetable kingdom," by Dr. S. Macadam, in *The Chemist*, vol. 1, p. 124.

ranging from 150 to 4,000 cubic feet; having given a negative result so far as the detection of iodine and bromine were concerned, I at length undertook an experiment, during which 100,000 cubic feet of air were transmitted through—

1st. A tube containing slips of starched paper; 2nd, a gas bottle, containing iron-filings and water; 3rd, a similar bottle, with a solution of acetate of lead. The starched paper and the liquid with iron-filings were intended to retain any free iodine, and the lead solution any soluble iodide.

At the conclusion of the experiments, and 625,000 gallons of air had been passed through the arrangement, it was found that the papers were not sensibly altered in tint, and that neither the water in which the iron filings were suspended nor the solution of acetate of lead contained the slightest trace of iodine.

For some time before leaving England I devoted considerable attention to the subject, and after trying numerous experiments in various ways, at last succeeded in demonstrating the presence of this element in the atmosphere, in the most conclusive manner. It is unnecessary for me to give any details of my unsuccessful experiments, or to describe the mode in which they were conducted; it will suffice if I explain the method of analysis adopted by me in my latest investigations. The apparatus I employed consisted of a simple aspirator, such as has been described by Mr. W. Johnstone, in the 4th vol. of the Quarterly Journal of the Chemical Society of London, and a bifurcated glass tube, to hold the reagents employed to eliminate the iodine, and to demonstrate its presence. The latter was connected with the aspirator by means of a small vulcanised Indian rubber joint. The aspirator itself consisted of a large glass tube, which I will term A, open at both ends, and provided with a somewhat narrower tube, B, near one extremity, branching off at an acute angle, and connected by a piece of vulcanised tubing, with another glass tube of about an inch in diameter, and nine inches long, C, also fitted with a branch tube, D, of smaller size, projecting from one side. The tube A was connected with the water-tap of a large cistern, elevated about twelve or fourteen feet from the ground, and on turning on the water of course a very uniform and abundant current of air was drawn through the apparatus, i.e., through the tubes C and D, at the rate of about 40 or 50 cubic feet per hour. In the tube D, a small piece of phosphorus was placed, having about a couple of inches of caustic lime placed between it and the outer opening of the tube. In the tube C, at a short distance beyond the insertion point of D, a small piece of microscopic glass, supported in a little card-board frame was inserted, and coated with a thin layer of starch granules, starch paste, and glycerine or chloride of calcium, to act as a test for the liberated iodine. On putting this apparatus in action a current of air was drawn through the tube C, which became mixed with a small quantity of ozonized air from the tube D, and then impinged upon the test-glass coated with the starch mixture, which, from the presence of the small proportion of glycerine or

chloride of calcium, was always preserved in a moist and sensitive condition. The test glass was then removed from time to time in the course of each experiment, and examined by a microscope of moderately high power, to ascertain if any of the starch granules had become tinged of a blue color from the presence of free iodine. It rarely or never happened that a slight but very perceptible blue or slate-grey tinge was not observed after the apparatus had been in work for a few hours. This blue tinge sometimes made its appearance in two or three hours, becoming more or less strong and decided in six or seven hours, but it occasionally was not observed before twelve or sixteen hours, and still more rarely not even after eight-and-forty hours. These experiments were carried on with intermissions, for nearly twelve months, the observations being made at various hours of the day, and during different states of the weather, from January 25th, 1854, to December 14th, 1854.

The following table will give—

- 1st. The days on which the air was examined.
- 2nd. The direction of the wind.
- 3rd. The days on which rain or snow fell.
- 4th. The number of hours that elapsed before the blue tint of the starch test made its appearance, and the effect upon the test-plates; also the time at which the experiment was commenced :—

January 25th S.W.	0-12	10 a.m., 7 hours, many granules, fine blue.
„ 26th S.W. to W.	0-07	10 a.m., same result.
„ 27th S.E.	—	Noon, 9 hours; after 11 hours a few starch granules, fine blue
„ 28th S.S.W.	0-07	Same result.
„ 31st W.N.W.	0-04	Noon 7 hours after 9 hours, blue tinge, well developed.
April 3rd N.	—	5 p.m. 11 hours; after 12 hours, a few grains, light slate gray.
„ 10th N.E.	—	5 p.m. 14 hours; blue tint, very slight.
„ 11th N.E.	—	5 p.m. same result.
„ 18th E. to S.E.	—	6 p.m. 15 hours; tinge slight, but perceptible.
„ 19th N.E. to E.	—	9 a.m. 20 hours; just perceptible.
„ 20th E. to S.E.	—	9 a.m. hardly perceptible.
„ 21st S.E. to E.	—	9 a.m. 24 hours; no coloration.
May 10th N. to W,	0-04	8 a.m. 20 hours; just perceptible.
„ 11th S.	0-02	9 a.m. 18 hours; very perceptible.
„ 13th N.E. to S.W.	—	Noon, 10 hours; 11 hours very evident.
„ 14th N.W. to N.E.	—	Noon, same result.
„ 25th S.	0-06	Noon, 11 hours.
„ 26th S.W.	0-27	Noon, 7 hours; 9 hours very evident.

" 27th S.W.	0.15	1 p.m.	ditto.
June 22nd S.W.	0.02	Noon,	ditto.
" 23rd S.W.	—	6 a.m.	8 hours.
" 24th S.W.	0.04	9 a.m.	6 hours.
August 7th N.W.	—	10 a.m.	12 hours.
" 8th N.W.	—	11 a.m.	9 hours; 10 very evident.
" 9th N.W.	—		ditto.
Sept. 23rd W.	0.19	{ 8 a.m. 6 hours; very decided in 8 hours.	
" 24th S.W.	—	{ 10 a.m. 3 hours; very decided in 5 hours.	
" 25th N.W.	0.08	11 a.m. 7 hours; very decided.	
" 28th S.E. to S.	—	{ 10 a.m. 8 hours; very evident in 10 hours.	
" 29th S.E. to E.	—	ditto.	
" 30th N.E.	—	8 a.m. 11 hours; perceptible.	
Nov. 1st S.W. to S.E.	0.27	9 a.m. 10 hours.	
" 2nd S.E. to S.W.	—	10 a.m. 11 hours.	
" 3rd N.W. to N.	—	9 a.m. 10 hours.	
" 4th N.W. to W.	—	8 a.m. 10 hours.	
" 8th W. to N.W.	—	9 a.m. 14 hours.	
" 9th N.	—	No coloration after 24 hours.	
" 10th W.	—	None even after 17 hours.	
" 17th N.E.	—	8 a.m. 20 hours, slight coloration.	
" 18th N.E.	—	ditto.	
" 19th N.E.	—	ditto.	
Dec. 2nd S.W.	—	10 a.m. 10 hours, slight tinge.	
" 3rd S.W.	—	ditto.	
" 4th W. to S.W.	0.08	Noon 11 hours, slight tinge.	
" 12th W.	—	9 a.m. 10 hours, slight tinge.	
" 13th S.W.	0.10	ditto.	
" 14th	0.14	ditto.	

To be able to judge more readily of the influence of the direction of the wind, and that of the neighborhood of the town on the results of the experiments, I have annexed a bird's eye view of the city of Bristol, showing its position, and the place at which the experiments were conducted (A), which is about four or four and a-half miles distant from the extreme point of Clifton.

NOTE.—At the last moment, we are compelled to go to press without the cut referred to by Mr. Herapath, as the engraver has most unaccountably disappointed us.

On SAPONIFICATION. By Dr. H. C. JENNINGS.

It would seem to be hardly necessary, in a country like Great Britain, in the year 1856—in which period alone 100,000 tons of soap will have been manufactured—to offer any remarks upon a subject so eminently practical, and so immensely important, both as relates to its commercial advantages, and its beneficial effects on the health and

comfort of the whole population of these islands. A little reflection will, however, show that much may be said, and that much ought to be said, upon this interesting subject. Others hold a similar opinion, for Mr. William Hawes favored the public with his views on this head in a paper read to the Society of Arts on the 28th of March, 1856, and printed in their Journal of that date. The paper was by no means favorably received; Mr. Hawes was left, after the discussion, in a painful minority. Dr. Lyon Playfair, the chairman of the meeting, concurred with the majority against Mr. Hawes, who was guilty of advocating M. Chevreul's views upon glycerine, stearine, &c. of which, unfortunately, the whole of Mr. Hawes' adversaries were so utterly ignorant that they one and all denounced Mr. Hawes, as "endeavoring to destroy the beautiful chemical theory and facts, established long ago by the French chemist;" so that each man (some of them soap manufacturers) threw his stone at Mr. Hawes for uttering truth, which their own ignorance perverted into falsehood. Mr. Hawes stated, that 6,000 tons of tallow were converted into glycerine; causing a loss of about £180,000 annually, by this effect alone. The other sources of loss which the trade imposes upon itself, by the destruction and deterioration of raw material, by extra fuel, time, and labor, cannot be estimated at less than double this amount—making the annual loss £360,000. After this recital, it may not be deemed impertinent or presumptuous to offer some reasoning and facts, with a view to mitigate the wholesale destruction of matter, *which is paid for by the consumer*, whilst the destroyer obtains profit—and often a reputation for ability and talent—which he by no means deserves. The painful truth is, that few of the soap makers, or candle makers, desire any improvement. Offers of this kind are rejected with contempt, if not requited with insult. They are often disgracefully ignorant of chemical science—are the miserable slaves of some workman, whose whole talent is expressed by the number of years he has worked at the trade, and the number of good houses he has worked for. Many masters will not admit their total incapacity to produce a good fitting of soap, they are indisposed to learn. They repudiate improvement, in all its features. They, however, hail with deep interest any plan of sophistication or adulteration, by which they may obtain larger profit; this, at least, does not wound their *amour propre* or slight their practical acumen.

Soap is a combination of fatty matter and alkali; the instrument of their union is water with the acid of caloric.

Soap is not, strictly speaking, soluble in water; it is merely miscible with it.

The deterative quality of soap is strictly mechanical. The oil or tallow would grease the object to be cleansed; the alkali would dissolve, shrivel, burn or rot it; the water would be inert. But a properly combined soap—especially those termed yellow, which contain 5 or 6 per cent. of rosin or colophane—is effective. The sticky, glutinous combination it produces with small portions of water combines with the particles of dirt which are then carried out of the tissue together by rinsing in clear water. As rosin is more viscid and sticky than oil or tallow, it acts much better than these; the action is similar

to that of albumen, when used to clarify syrups or jelly; the albumen is coagulated when the temperature reaches 212° F., and entangles the minute solid particles that are insoluble, and forms a coagulum—leaving the liquid bright and transparent.

Soap, in however minute quantity, when added to distilled water, can always be seen by the naked eye. The most minute quantity—such as the thousandth part of a grain—is detected by the microscope; it appears fibrous or flocculent.

Soaps may be divided into the following classes:—

TRANSPARENT SOAP

WHITE CURD

MOTTLED

YELLOW

SOFT SOAP

METALLIC SOAPS

Transparent soap is produced by dissolving soap in alcohol, more or less diluted with water. This soap is remarkably hard, being absolutely anhydrous; and may be generally regarded as the purest form of this substance; its specific gravity is greatest of all, from the absence of water.

White curd soap is a combination of caustic alkali, tallow, and water. Its hardness and color greatly depend upon the purity of the goods employed, and the degree of heat used in the boiling. 1,600lbs. of material generally yield one ton or 2,240lbs. of soap.

Mottled soap is nothing more than white curd soap less strongly boiled; and the residual lees permitted to remain between the interstices of the partially curdled soap; this gives it the marbled or mottled appearance from which it is named. The color is principally due to iron.

Yellow soap is a more delicate compound than any above mentioned; and perhaps no soap has so durable and firm a hold on the people of this country as this article. It lathers or froths with great facility; it cleanses perfectly, and leaves a fresh and agreeable smell upon the objects washed.

This soap is composed of tallow, rosin, and alkali; in proportions varying according to the mode of manipulating, the quality and relative quantities of the materials operated on, the degree of heat, and the period during which that heat is employed. This soap is, probably, never perfectly made, unless by accident; the details of some of the phenomena are instructive and interesting; to comprehend them, we must beg a few minutes' attention to the chemical elements of the substances that are introduced and combined.

1st. Tallow, a compound of stearine and oleine; the relative proportions of these (one is solid, the other liquid) varies in every animal, and in every part of every animal; we may average the oleine to be about one-third: the stearine two-thirds.

2nd. Rosin—this substance is composed of three acids, viz.—sylic, pinic, and pimanic. Their relative proportions vary with the climate which produces the trees from which it is obtained, the kind of tree, its elevation above the sea, its age, &c., &c.

One of these acids dissolves in alcohol; the other two do so imperfectly; two of the acids combine with equal facility with oleine and elaine; one of them slightly with stearic acid, forming a compound in every property resembling wax, with which it is isomeric. Heat enables these acids to combine with caustic alkali, and also with neutral alkali, whether soda or potassa; the one giving a hard substance, the other a soft one; the soap maker may put what quantity he pleases into his pans, but only a small part (one of the three acids) will incorporate and combine with the stearine and alkali, about five or six per cent.; the rest will combine with the alkali and part of the oleine, and form what is called by the trade, "niger;" (a dark liver-colored substance) the constant contact with the lowest and hottest part of the pan, causing a gradual carbonisation, and a partial transformation into rosin oil. This is caused by the constant increase of specific gravity which the evaporation of the aqueous parts of the niger undergo during the boiling; and in proportion to the density of the niger will it eventually become softer and blacker, necessitating a future boiling with fresh tallow and alkali, and after all, yielding only a soft and inferior quality of soap. The fob, or frothy saponaceous matter that rises to the surface during the boiling is merely the hardest and lightest particles of the soap, rendered of less specific gravity by combination with steam and atmospheric air. The hardness and brightness of the best pale primrose yellow soap is caused by the deposit of the greater part of the rosin; and is, in fact, the finest and hardest of the three above named acids, combined with stearine, alkali, and water; the best proportions obtained by the existing processes are—

Stearine	.	.	.	.	.	.	.	.	65
Oleine	.	.	.	.	.	.	.	.	11
Alkali	.	.	.	.	.	.	.	.	6
Rosin	.	.	.	.	.	.	.	.	6
Water	.	.	.	.	.	.	.	.	12
									100

Saponification should be begun with a weak alkali; if the solution is too strong no saponification is possible; for the caloric combines with the grease and renders it lighter and less capable of contact with the solution of alkali beneath it. During the boiling about 10 per cent. of glycerine is produced; it is impossible to expose fats or oils to the action of steam or aqueous vapor, without producing some glycerine, and this increases in proportion to the heat, and the time that it is acted upon; the *formation of glycerine* is, however, *greatest* when chemical agency or double decomposition is also going on, as must be the case when caustic alkalis are employed with long continued heat; great density of the goods acted on is also effected. The formation of glycerine is due to the fixation of water; it is composed of hydrogen, carbon, and oxygen, infinitely soluble in water, and is obtained at the expense of the tallow or oil employed.

Stearic acid requires 2 per cent. of caustic alkali to perfectly neutralise it.

Rosin requires $6\frac{1}{2}$ per cent.

Oleïne requires 9 per cent.

Tallow requires 7 per cent.

The whole of the free water in soap evaporates in time.

The chemical equivalent of water in soap depends upon its hardness, and dose of combined alkali—it varies with every kind of soap; but on an average a sound and pure soap should not contain more than 12 per cent. of water.

Every moderate family could make soap enough for the consumption of house and servants from the kitchen stuff or dripping produced; the whole process requires but two hours, and no utensils are needed but such as are to be found in every domestic establishment.

The soap maker often has what is called a miss-fit, for which he is unable to account; this soap has to be re-boiled, or must be sold at a lower price. I presume, then, that no precise chemical laws of saponification are known to the trade; it is a rule of thumb or practical matter alone, which cannot be the most satisfactory condition required.

The numerous practical devices, such as crutching in more water, adding sulphate of soda, common salt, and other substances of cheap and inferior quality to give weight, only render the soap less detersive, less hard and economical, and are frauds upon the public. Under the old excise laws, the object of the unprincipled manufacturer was to “run” as much goods as he could without detection, and so cheat the revenue; now, the same end is attained by adulteration and other tricks well known to the unscrupulous part of the trade.

Soft soap is made with the vegetable alkali or caustic potassa, the combination of horse oil, fish oils, and every common and offensive fatty or oily matter of low price; it is sur-charged with alkali, and therefore is only used for the commonest purposes; this soap never hardens or dries, because potassa has a great affinity for water, of which this soap contains a considerable portion.

Metallic soaps are insoluble, and are only used to form plasters, varnishes, &c.; during the formation of these soaps a much larger quantity of glycerine is formed than in any of the above recited, because a greater fixation of water is effected, in consequence of the insolubility of the metallic soap, and also of the more decided molecular decomposition of the matter under treatment.

Acid saponification is not yet strictly out of the domain of theory, but it must soon become a vast and powerful practical lever, which commerce will wield with singular energy and advantage; it will eventually revolutionise the existing saponaceous system, and also the stearine processes now so fertile in waste of matter, time, and labor.

I will now observe, that in all the above processes, two radical vices exist; both formidable, and both unnecessary:—

1st. The employment of caustic alkali, which is produced from the common soda ashes, carbonate of soda, &c.; with the aid of lime

in its most anhydrous state (quick or burned lime); this combines with the water, and produces much latent and sensible caloric, and in that state combines with the carbonic acid: by this means the alkali is rendered so extremely caustic, that it will dissolve wool and silk, and acts powerfully upon vegetable fibre of every kind. The lime combines with some of the alkali, it is an expense, and soon accumulates in a large factory, so as to cause much inconvenience and occupy much space; it occasions lumber and filth, and must be speedily removed.

2nd. The formation of niger in the manufacture of yellow soaps; this article is formed of the alkali and the oleine or elaine, which cost originally the price of the tallow; it has again to be treated with fresh goods; fire, labor, and time are consumed, and it can never produce anything but an inferior soap.

These two evils can be corrected; the trade holds it as a maxim, that the "grease cannot be *killed*" but by a caustic alkali: this is an error, capable of the most demonstrative practical proof, and I assert, and am ready to prove, that a better soap, cheaper, harder, can be produced without lime or caustic alkali, and in less time and with much economy. That the soap produced by a neutral alkali is less, (even not at all,) destructive of animal tissue than the soaps now prepared, and the absence of all glycerine will cause a greater yield of soap than when glycerine is formed; but such is the hopeless apathy, inertness, prejudice, and ignorance of the trade generally, that I have found hardly any one who would fully go into the matter, or give me a fair trial; I have found some who would partially and defectively try what could be done; but not one who would thoroughly and liberally carry out all that was required: therefore success or conviction became impossible.

I must here make one honorable exception. Mr. Lilly (Harris and Lilly), Curtain-road, afforded me the most unbounded practical information and access to his works and operations; but as he is candle-maker, soap-boiler, and tallow-melter, and does not use steam, and as his locale is too much confined for space, he could not adopt my views and process.

I engage—

1st. To remove and separate the hard from the soft parts of tallow, viz., the stearine from the oleine, the palmitine from the elaine, and that without distillation, hot or cold pressing, or saponification; the stearine shall be pure, dry, and hard, and burn bright and white, without smoke or smell; the elaine or oleine shall be white, clear and free from acids; shall be fit for burning in lamps, for making soap or for anti-attribution purposes, and no glycerine shall be produced.

2nd. I will engage to unite and chemically combine 50 to 60 per cent. of rosin with tallow or oil, and make a soap of good color and quality, hard, economical, and dry, deterative and sound, without using lime or caustic alkali, and with a saving of goods, time, and labor.

If I realise these pledges, surely the astounding advantages to com-

merce, and to society at large justify a fair trial, and a liberal treatment; but those engaged in the trade are making money, they are in the trammels of centuries of old habits and prejudices; many of them are led by foremen or old hands, who know the old beaten practice, and who are unable to conceive the possibility of any fact that contradicts and destroys their experience; all invention is fatal to them; selfishness, prejudice, and ignorance quicken them into malignity and hostility; they would designedly spoil a ten ton pan of soap, to sustain their position and defeat the introduction of any amelioration or improvement; the master is too often the slave of his workmen; he cannot afford to risk his goods, his money, and his reputation, and perhaps become the jest of his men and of the whole trade; thus it is, that the most tangible and valid discoveries, and the most important improvements linger and eventually lapse, and society is held back and damaged by the tyranny of £. s. d., and the most revolting ignorance and selfishness.

8, Great Tower-street, Dec. 22, 1856.

TRANSLATIONS AND ABSTRACTS.

BIOGRAPHY.

ROBERT BOYLE: *a* BIOGRAPHICAL STUDY. By M. CAP.

(Continued from page 186.)

In the first half of the seventeenth century, communications took place between scientific men only by correspondence and journeys. Academies were not in existence; no periodical communication kept men of science informed of foreign researches; so that dates were not sufficient for establishing priority of discovery. Thus, although Torricelli's experiments date from 1643, they were not known in France until the following year, by the communications of P. Mer-senni. Those of Pascal, were made in 1645; but his work on the gravity of the air appeared only two years afterwards. The first researches of Otto von Guericke, which date from 1654, were published by Father Schute, only in 1657, and Otto himself did not print his work until 1670. Such are the causes which throw so much uncertainty on the precise date of these discoveries, and on the part of each physicist, both in the initiation of certain researches, and in their publication.

Boyle had repeated the experiments of the physicist of Magdeburg, from the year 1658, and he gave a summary of them, in his first work, in 1660. His researches on the barometer were made in the preceding year. It was he who caused to be constructed, according to the principle that the spring of the air makes an equilibrium to the weight of the atmosphere, the first instrument which bore that name. He gave, in his *Physico-Mechanical Experiments*, a description of it which was afterwards reproduced in the *Philosophical Transactions* (1666, No. 11). He had observed, he said, that the column of mercury rose when the air was more pure, and the higher it was raised,

the less was its height, because the air became lighter. "I own," he adds, "that I suspected that the phenomena which the barometer presents, and which hitherto had rather embarrassed than instructed us, might lead to some great discovery, of which we could form no conception, if a certain number of zealous correspondents would continue this kind of research." In the same work, he confirmed the theory put forward by Toricelli, and—which would lead us to suppose that Pascal's researches were then scarcely known in England—he induced the Royal Society to send two of its members to repeat the experiments of the Florentine physicist, on the peak of Teneriffe. A very amusing anecdote is related with regard to this substance. As the Canary Islands belong to Spain, the Royal Society asked of the Spanish Ambassador recommendations to the authorities of those islands. The Ambassador received the deputies with kindness; but, taking them for merchants dealing in Canary wine, he asked them what quantity they proposed to take. They replied, that it was not with a mercantile view that they undertook this voyage, but to make experiments on the weight of the air. "What!" said the Ambassador, "you wish to weigh the air!" On their replying in the affirmative, he took them for madmen, sent them away abruptly, and hastened to tell the adventure to his friends. Unfortunately, he learned that the King and the Duke of York were at the head of those whom he had treated as mad, which scarcely made him reverse the idea he had formed of this singular expedition.

Boyle's researches on the air were the prelude to the discoveries which took place in the course of the following autumn, and which are due to Black, Priestly, and Cavendish. In his last work, he defined air as a thin, transparent, compressible, and dilatable fluid, which envelopes the earth to a very great height, and which refracts the rays of the sun. With regard to its composition, he believed it to be formed of three kinds of molecules, the first arising from the exhalations of water and animals; the second from magnetic effluvia; and the third of the elastic portion of the atmosphere. We shall see further on what were his ideas as to the chemical action which the air exerts on other bodies.

We find in the same work a description of the thermometer, reproduced in 1665, in his *Treatise on Cold*. Van Helmont had already remarked that the water contained in a hollow stem of glass rose and fell according to the temperature of the surrounding medium. Boyle substituted spirit of wine for water, and, in order to render the instrument more sensible, he colored the alcohol with cochineal.

Finally, to him is attributed the first idea of the *sympiesometer*. This is an apparatus composed of a flask, the bottom of which is covered with a layer of water about a centimetre in depth. Into this water dips the extremity of a very narrow tube, which is made to pass through the cork of the vessel. The mass of air contained in the latter is thus isolated, and, by means of a gentle insufflation, an excess of air is forced in which increases its density, and, consequently, causes a small column of water to rise in the tube. Things being thus

arranged, the least want of equilibrium between the external and internal pressure causes an elevation or depression of the small column of water in the tube, which renders this apparatus still more sensible than the barometer to the variations in atmospheric pressure.

Boyle's researches relative to the air, considered solely in a physical point of view, were followed by studies concerning cold and heat;* antagonistic phenomena which depend, according to him, on the mechanical and physical properties of the molecules of bodies. He produced cold by means of frigorific mixtures, for which he gave several formulæ; he established thus the term of the congelation of urine, beer, wine, oil, &c. He produced artificial heat not only with quick lime and water, but by means of various chemical combinations, by mixing iron filings, water, and sulphur, by the amalgam of gold and mercury. He accounted for the heat produced in these combinations by the motion which took place between the molecules of bodies, having ascertained that all heat was inseparable from motion: this was a very advanced theory, which the most modern observations tend daily to confirm. He explained cold by saying that it resulted from privation of heat—a theory likewise since confirmed by Rumford's researches. He remarked that salt retarded the congelation of water, as well as its boiling point. He ascertained that water is dilated, and increases in volume, by congealing. He proposed to take as his thermometrical starting point the term of the congelation of water,—that is to say, that of nascent ice. He studied the sphere of activity of cold and the direction of its motion. He gave a table of the melting points of ice in different liquids; and finally, his was the idea that the density of bodies is especially due to the degree of heat by which they are penetrated.

At about the same time, he published his *Experiments and Observations upon Colors*, (London: 1663). It is here that we find for the first time the assertion, that colors consist in the modification of light by the surfaces which reflect it; that corpuscles absorb and annihilate light, whilst the surface of white bodies present facettes, which, like mirrors, reflect light without modifying it. The third part of this work contains experiments on the diamond.

Two years after the publication of these beautiful researches, Robert Boyle published a new work, entitled *Hydrostatical Paradoxes made out by new Experiments, for the most part Physical and Easy*, (London: 1665, in 8vo.). This part of physics owed much to the labors of the preceding century; to Leonardo da Vinci, who had published a Treatise on the motion and measuring of water; to Castelli, the pupil of Galileo and master of Torricelli, to whom we owe a treatise on the measuring of running water; and, finally, to Pascal, who, more recently, had published his treatise on the Equilibrium of Fluids. Boyle resumed and continues in his work this long series of labors. After having proved that it is the weight of the air which sustains liquids in the body of the pump and in syphons, he shows

* *New Experiments and Observations touching Cold, &c.* London: 1663.

that the direct pressure which a body steeped in water supports is equal to the weight of a column of water whose base is the upper surface of this body, and the height that of the water placed above that surface. He showed that the actual pressure increases with the depth of the body immersed; that the excess or deficiency of its weight, with relation to that of water, makes it sink or float; that, in all fluids, the upper layers exert a pressure on the lower layers; that a slight pressure of a fluid is sufficient to make the water rise in the body of the pump; that a fluid presses equally in all directions; and a host of other principles, which completed the knowledge of the period on this subject.

Boyle had remarked, that when we immerse in cold water the aperture of a eolepyle, or of a tube filled with vapor, the water is immediately raised into it as by suction. He did not account very well for this phenomenon; but this experiment gave him, doubtless, the first idea of forming a vacuum by the condensation of steam—an idea which Papin turned to account long afterwards, in his discovery of the force of steam, and of the means of forming the vacuum in a capacity closed in every part.

Papin, indeed, worked for several years with Robert Boyle, and the latter does him the justice to say that he aided him in his researches with rare talent. Born at Blois, in 1647, Papin practised medicine at Paris towards 1671. He lived at the Bibliothèque Royale, and worked in the laboratory of the celebrated Dutch Physicist Huggens. He published, in 1674, a work entitled—*New Experiments on the Vacuum*; in which he described some improvements of his invention in the pneumatic machine. At the close of the year 1675, Papin went to England, and introduced himself to Boyle, who had so happily modified Otto Von Guericke's apparatus. The two *savants* joined in their labors. Boyle, who was older than Papin, and at that time troubled with gravel, directed the French Physicist in some experiments which he wished to show to the English. Their united efforts were continued for three or four years. Boyle found, moreover, that Papin was very skilful in the construction and management of physical apparatus. "Many of the instruments," says he, "which we used—and, among others, the pneumatic machine and the air-gun—were of his invention, and, in part, the work of his hands." It was Boyle who, in 1680, caused him to be admitted to the Royal Society. The following year, Papin published his *New Digester*. It was on this occasion that he invented his safety valve, the great importance of which he did not at first appreciate; but which afterwards became one of the principal parts of his steam engine.

These different points of physic are not the only ones which occupied Boyle's attention. He studied also the porosity of bodies, the properties of the magnet, the origin and form of the primitive corpuscles, the spontaneous evaporation of liquids and even of solids. Cuvier says, that he made experiments on the large scale on the electrical properties, and that he knew their faculty of producing sparks. He gave a table of the specific density of a great number of bodies, the

first which had existed in science, and his results differed very little from those which we now possess. It was on this occasion that he invented the areometer, for determining the density of liquids, already invented and described, in the fourth century, by Synesias, under the name of *hydroscopeum*.

Most of the principles which Boyle established in physics have become familiar and common, but these are the first bases on which our present knowledge is founded. The ideas which he conceived relative to the nature and properties of the air, on the measurement of heat, and on several other points of general physics, have remained acquired to science. As regards certain details, and certain theories always presented with reserve, they must not be judged strictly and in an absolute manner. Looking back to the date of their enunciation, we should consider them with that feeling of justice and respect, to which the first monuments of our knowledge, the works conceived and directed in a new way, and especially the men who prepared the brilliant future of sciences, of which our age is beginning to reap the fruits, are entitled.

(To be continued.)

ORGANIC CHEMISTRY.

MEMOIR on PEPSINE. By M. BOUDAULT,

(Concluded from page 158.)

HAVING a certain quantity of artificial pepsine in solution in water, I could bring it to the normal type,—that is to say, estimate its digestive power—by adding water to it when it was obtained too concentrated, and evaporating it at a gentle heat, if it did not digest the same quantity of fibrine as the gastric juice of the dog.

It was, therefore, by making numerous artificial digestions, and all in the same conditions, and in presence of digestions made comparatively with the gastric juice of the dog, that I obtained pepsine similar to the normal type.

In the preparation of pepsine by chemical processes, the greater part of the lactic acid is eliminated. This acid must be added to it; the gastric juice of the dog serves us also as a point of comparison. By means of a metrical solution of tincture of litmus, we estimate the acid pretty accurately, and ascertain the quantity which must be added to artificial pepsine, in order to give it all the properties of the gastric juice of the dog.

Indeed, we find its color to be amber, its density being the same, and resembling in odor that of the dog. But one of the principal chemical and physiological properties which they alike possess is, that when maintained at 50° C. (122° F.), for six hours, both chemical any natural pepsine are destroyed. Chemical pepsine becomes slightly turbid, and loses its digestive properties. It may be preserved for any length of time, if protected from the air. But, when the bottle is

opened, it putrefies sooner than that of the dog, unless the proportion of acid be augmented.

Alcohol precipitates it from its solution; tannin does the same; the salts of lead and mercury produce in it abundant precipitates which are capable also of regenerating pepsine by their decomposition.

We have so far arrived at demonstrating a perfect similarity between the properties of the two pepsines. We shall now compare their products of digestion; in order to leave no doubt on this subject. But, before doing so, we will state how we effected our artificial digestions, and, consequently, in what conditions we have tried comparatively our two liquid pepsines.

We followed, in the first place, the processes directed. We put in contact determinate proportions of fibrine and gastric juice in small phials, whose necks were surmounted with a bent tube dipping into lime water. These phials were submitted to a temperature of 40° C. (140° F.), for four hours, in a water bath. The phials were agitated as often as possible. After the time mentioned, the fibrine was entirely digested, and the resemblance between the two products could be perceived at first sight.

We sought to approximate, as closely as possible, to natural conditions. The following was the means employed. We used caoutchouc bags as thin as possible, of a pear shape, and very long. We introduced these bags into the stomach of our dogs by the stomachal fistula; into these bags we had put the matters to be digested, and we operated comparatively with natural pepsine and with chemical pepsine, and in the same proportions as in the first experiments with the phials. Our artificial digestions were effected in the same manner, only more rapidly, by reason of the peristaltic motion.

Not satisfied yet with this almost natural means of obtaining a product of digestion, we had recourse to the natural functions. We deprived a dog of nitrogenous food for several days; we proved that the stomach was quite free from alimentary matters, and that the gastric juice which flowed out through the stomachal fistula, was perfectly normal. We then gave to this animal a very large quantity of fibrine; the digestion took place rapidly and we were enabled to collect, by means of the canula, a viscid liquid, which we could compare with the first liquids obtained.

Let us examine these various products of digestion.

When a digestion is complete, it should not precipitate by boiling. The assimilable product is then soluble in water, which is one of the most certain characters for distinguishing a product of digestion from a simple solution of fibrine in the dilute acids.

All the various products of digestion obtained in the different conditions above indicated had this property—when boiled, they did not precipitate; they were therefore perfectly similar in this respect.

When submitted to all the chemical tests, they all furnished similar results.

Precipitation by the salts of lead and mercury, tannin, alcohol and solution colored with nitric acid—in a word, the pepsine of the gastric

juice of the dog—chemical pepsine gives perfectly similar products of digestion.

If we now compare the solution of fibrine in the acids with these same digestions, we no longer obtain the same reactions.

To effect a digestion we must therefore have the combined action of the acid and pepsine. The acid acts first. It disengages the fibrine, and the pepsine completes the digestion. I used the process of M. Longet for comparing the products of digestion with each other. This very ingenious process consists in putting in contact a mixture of glucose and bitartrate of copper and potassa (Barreswil's liquor), and a nitrogenous product of digestion; when the digestion is complete, there is no reduction of the salt of copper by the glucose—in a word, the nitrogenous nutriment masks the glucose.

Indeed, when we put in contact a determined proportion of Barreswil's liquor, and a determined proportion of glucose, with a solution of a product of digestion, the liquor becomes violet, and it is not changed by boiling. If we greatly increase the quantity of glucose, a reduction is thus effected; but still not a precipitation, as occurs when glucose alone is mixed with the bitartrate of copper and potassa.

This experiment was repeated with the products of digestion obtained with both chemical pepsine and the pepsine of the dog: the similarity was perfect, even in the violet color. Hence, as always, we may conclude on the perfect identity existing between the products.

We have intended to show the perfect resemblance, not only between the chemical and physical properties of the two pepsines, but also between their physiological properties. We may, therefore conclude on the substitution of one for the other.

Dr. Corvisart, in his memoir on nutriments and aliments, first pointed out that pepsine might be employed in dyspepsia. It would be necessary to find a great quantity of pepsine, in order to use it as a medicament, and especially to give it always identical; being able to replace the pepsine of the carnivora by that of the herbivora, the difficulty is overcome.

It remained only to examine under what form this new medicament might be administered.

We have said above, that acidulated pepsine is soluble in all proportions in water, and also that these solutions are altered very easily by exposure to the air; it was difficult to use them thus. As its taste is not very agreeable, there would be great repugnance on the part of the patients to take liquid pepsine, even associated with sugar or substances capable of concealing its taste.

We were under the necessity of finding a means of presenting this medicament, without the inconveniences which we have just noticed. After much research, we were enabled to concentrate pepsine to a syrupy consistence; but it would attract moisture, and would not keep better than when more dilute. We desired to find a substance which might be incorporated with it, so as to be able to reduce it to powder.

It was especially necessary that this body should be inert, that it should not alter pepsine, and at the same time, it was necessary that the pepsine should not be transformed into a nutriment, or indeed, not act on it as a true ferment; it was likewise necessary that this body should absorb the moisture from the pepsine without, however, being sufficiently hygrometric afterwards to attract moisture from the air.

We had recourse, after numerous trials, to starch, powdered and dried at 100° C. (212° F.)

Starch does not undergo, so to speak, any transformation by means of pepsine, and it perfectly fulfils all the conditions which we desired; moreover, it in no way impedes digestion.

Intimately mixed with starch thus dried, pepsine may be reduced to powder, and, when preserved in well-corked bottles, it undergoes no alteration, and may be preserved indefinitely without losing its physiological properties.

Its disagreeable taste is singularly modified; the odor alone remains, so to speak, the same; but diminished owing to the dryness of the product.

The quantity of starch added must always be determined; otherwise, we should have a nutritious powder not always possessing an uniform degree of digestive property. Now, it is of the greatest importance, that a medicine should always possess the same properties. We also had recourse to artificial digestions, in order to estimate the digestive power. We gradually added starch in such a manner, that one gramme of pepsine to which starch had been added, had always the power of digesting four grammes of dried fibrine; which operates in the stomach the digestion of the lean of a chop.

Thus prepared, pepsine constitutes a true medicament; it is capable of effecting digestion in the place of the powerless stomach, and to comfort it, at the same time economising its powers.

It is in cases of want of appetite, of slow and painful digestion, diarrhoea, vomiting and the weak digestion, which still exists at the commencement of convalescence from serious fevers, and in the course of most chronic diseases, and in consumption from insufficiency of nourishment, that pepsine manifests itself a powerful digestive agent.

It may be administered either acid or neutral.

When acid, it possesses all the properties of the gastric juice, and may be administered whenever there is not too great a quantity of acid in the stomach. On the contrary, it is given neutral (we do not mean chemically neutral, but neutral as distinguished from that which has been acidulated) whenever the diseased stomach contains too much acid.

Pepsine may be taken easily before the meal enveloped in unleavened bread; if it be taken in the first spoonful of soup, care must be taken that the latter does not exceed the temperature of 45° C. (113° F.), for the digestive property would evidently be lost.

After the meal, it may be administered in cherry syrup, which completely disguises its taste.

We have endeavored to give pepsine under the form of syrup; it

would have been as easy to estimate it in solution in cherry syrup, as mixed with starch; but we remarked, after a certain time, a very distinct action of the pepsine on the cane sugar.

Pepsine put for several days, ten or twelve, for example, with cane sugar, converts it into glucose, and afterwards into lactic acid; it acts as a ferment. And certainly there can be no salivary diastase in the pepsine which we prepare, since this latter results, not from the complex liquid found in the stomach, but from the gastric mucous membrane previously washed.

It was, therefore, impossible to keep this mixture of pepsine and cherry syrup.

The pepsine may be mixed with a certain number of medicaments, which do not modify its digestive properties.

Hydrochlorate of morphia, in the dose of one centigramme at each meal may be added to pepsine, in cases of virulent pain in the stomach.

Strychnine, in the dose of three milligrammes per day may be given, in cases in which the peristaltic motion is deficient.

Other medicaments, such as the trisnitrate of bismuth, lactate of iron, carbonate of iron, iodide of iron, and pulvis ferri, may be mixed with pepsine, and in no way impair its properties.

It has been my object to show, that chemical pepsine might, in every respect, be substituted for the pepsine of the gastric juice of animals, and that it may be employed as a medicament.

Chemical experiment has absolutely confirmed this similarity, and chemical pepsine may now be employed in cases in which there is a deficiency of gastric juice, for it is as beneficial to the system, as if the stomach had secreted its natural juice.

Journal de Pharmacie.

On SOME COMBINATIONS of BENZOYLE. By M. C. VOIT.

THIS work comprises the study of some combinations of benzammic, or amidobenzoic acid, and the description of a new amidised acid, diamidobenzoic acid.

Benzammic or amidobenzoic acid is formed, as is known, by the reduction of nitrobenzoic acid by means of sulphuretted hydrogen. To prepare nitrobenzoic acid, fused benzoic acid introduced by small portions at a time, into a mixture of two parts of concentrated sulphuric acid, and one part of nitric acid, of the density of 1.5, and the whole is heated for half an hour. Cold water precipitates from this mixture white flocks of nitrobenzoic acid, without any mixture of dinitrobenzoic acid.

Nitrobenzoic acid is formed also, when chloride of benzoyle is allowed to fall drop by drop into a mixture of sulphuric and nitric acids.

It was converted into amidobenzoic acid by boiling with hydrosulphate of ammonia. The liquor having been filtered and evaporated,

hydrochloric acid was added to it. Mammilated crystals formed by the combination of the hydrochloric acid with the amidobenzoic acid, were deposited.

The following are the formulæ of some of the amidobenzoates:—

Amidobenzoate of Soda $C^{14}H^4(NH^2)NaO^4$ (at 100°). Delicate needles obtained by decomposing the baryta salt with sulphate of soda and adding absolute alcohol to the concentrated aqueous solution.

Amidobenzoate of Baryta $C^{14}H^4(NH^2)BaO^4$ (at 100°). Voluminous prisms, rather colored, prepared by saturating the amidobenzoic hydrochlorate with carbonate of baryta, and evaporating the filtered liquor; the amidobenzoate crystallises at once.

Amidobenzoate of Strontia $C^{14}H^4(NH^2)SrO^4 + 1\text{ Aq.}$ Colorless prisms, very soluble in water, sparingly so in alcohol, which lose their water of crystallisation at $100^\circ C.$ ($212^\circ F.$)

Amidobenzoate of Lime $C^{14}H^4(NH^2)CaO^4$. Delicate needles, which turn red in the air.

Amidobenzoate of Magnesia $C^{14}H^4(NH^2)MgO^4 + 7\text{ Aq.}$ Large transparent six-sided prisms, with an oblique terminal face, obtained by double decomposition with the baryta salt and sulphate of magnesia.

The dinitrobenzoic acid of M. Cahours, was obtained by boiling benzoic acid for several hours, with a mixture of sulphuric and nitric acid. It may easily be etherified by treating it with alcohol and sulphuric acid; water precipitates from the acid liquor oily drops, which solidify by cooling, this is dinitrobenzoic ether. It is deposited from boiling alcohol in long silky needles. Ammonia converts it into dinitrobenzamide $C^{14}H^5(NO^4)_2O^2$. This substance, which is fusible at $183^\circ C.$ ($361^\circ F.$) is sparingly soluble in cold water, and rather more soluble in boiling water. The solution is neutral, and has a bitter taste. The ammoniacal solution does not precipitate nitrate of silver.

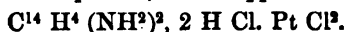
To convert dinitrobenzoic acid into diamidobenzoic acid, a current of sulphuretted hydrogen is passed for a long time through the warm ammoniacal solution of the nitrogenated acid. The liquor, separated from the sulphur, is evaporated, supersaturated with hydrodinitric acid, and filtered while hot. After some time, it deposits yellow crystals of diamidobenzoic hydrochlorate.

Diamidobenzoic acid itself may be obtained by digesting the diamidobenzoic sulphate with carbonate of baryta, and evaporating the filtered solution, first in a water bath and then in vacuo. It consists of small greenish prismatic crystals, which dissolve easily in water, alcohol and ether. They fuse and turn black towards $195^\circ C.$ ($383^\circ F.$) Their solutions are neutral and have no taste.

Diamidobenzoic acid is not an acid, properly speaking. Far from combining with bases, it forms with the acid very definite and crystallisable compounds. We here meet with phenomena analogous to those which have been noticed with aniline, and which relate to the modifications impressed on a given substance by the molecules, which enter into it in substitution for hydrogen. Aniline is a base, but its basic

power decreases and finally disappears, in proportion as the molecules of hydrogen are replaced in it by chlorine. So also, benzoic acid is a powerful acid; but its acid properties are much diminished when it is converted into amidobenzoic acid by the substitution of a molecule of amidogen for a molecule of hydrogen. They disappear altogether in diamidobenzoic acid, with the introduction of a second molecule of amidogen into the combination. Diamidobenzoic acid is capable of forming salts only with the acids.

Diamidobenzoic Hydrochlorate ($C^{14} H^4 (NH^2)^2 O^4, 2 H Cl$). This combination is formed in the preparation of amidobenzoic acid. It is purified by dissolving it in a small quantity of cold water, and adding to the solution concentrated hydrochloric acid. White needles separate immediately, or else a flocculent precipitate, which is transformed into needles. This operation can be repeated at will. The diamidobenzoic hydrochlorate dissolves easily in water, alcohol and ether. Exposed to contact with the air, the aqueous solution is decomposed, depositing black flakes. When it is mixed with chloride of platinum, and the solution is put over a vessel containing sulphuric acid, brown crusts are deposited, which appear to contain



Diamidobenzoic Sulphate $C^{14} H^4 (NH^2)^2, S^2 H^2 O^8$. The hydrochloric combination having been dissolved in dilute hydrochloric acid, there were deposited by concentration, flocks tables and plates, which were obtained almost colorless by recrystallising them in alcohol. These crystals dissolve easily in water, and rather more difficultly in alcohol. These solutions, which are of a brown yellow by transformation and green by a reflected light, are easily decomposed.

Independently of the foregoing salts, the diamidobenzoic nitrate, acetate and oxalate were obtained.*

Annales de Chimie et de Physique, November, 1856.

ANALYTICAL CHEMISTRY.

ESTIMATION of CHLORINE. By M. MOHL.

WHEN we wish to determine the quantity of chlorine in hydrochloric acid, or a soluble chloride by filtered solutions of nitrate of silver, it is difficult to attain the exact point when all the chlorine is precipitated. An excess of nitrate of silver is generally used. To avoid this cause of error, M. Levot has advised the addition to the chloruretted solution of a certain quantity of phosphate of soda. In this case the chloride

* M. Boulet, professor at the Lyceum of Strasburg, was occupied at the same time as M. Voit, and without any knowledge of his results, with the reduction of the nitrogenous combination of benzoic acid. The method which he employed was that of the earthy salts, first pointed out by M. Béchamp. M. Boulet informs me, that not only has he obtained by means of this method amido and diamidobenzoic acids, but also amido and diamido cuminic acids. M. Béchamp made allusion to these results, in a work published in the March number of the *Annales de Chimie*, 1856.—A. Wurtz.

is precipitated first, and as soon as this precipitation is complete, the excess of nitrate of silver causes the precipitation of phosphate. The phosphate of silver being yellow, the conclusion of the operation is marked by a change in the color of the precipitate.

But phosphate of silver is a very pale yellow; for the difference of tint to appear with certainty, some little quantity is obliged to be precipitated. This is a cause of error, which M. Mohl proposes to avoid by replacing phosphate of soda by chromate of soda, or still better, by chromate of potassa. The second precipitate showing when to stop, is then of a dark red, and thus all uncertainty is prevented. It is necessary to avoid operating on acid liquors; they prevent the formation of the chromate of silver.

This process is applicable to the estimation of the chlorine in urine, in spring water, in mineral waters, in nitre, in commercial potassa and soda, and in chlorate of potassa.

To estimate the chlorine in the chlorides of barium, calcium, mercury, and all the metals whose chlorides are insoluble, it is necessary to decompose them previously, by means of carbonate of soda; then the operation can be performed on the chloride of sodium thus formed, after adding a small quantity of chromate of potassa. A slight excess of carbonate of soda will not interfere with the accuracy of the process.

Chimie et Pharm. Journal.

INORGANIC CHEMISTRY.

On the NEUTRALITY of SALTS.

By M. MARGUERITE.

Of all the distinctive characters of a saline compound, the most difficult to ascertain and define in a precise manner, is without contradiction, the degree of saturation of its elements which constitutes neutrality. To show how vague and uncertain this definition is, it will be sufficient to recall the considerations by which it has been established.

The vegetable colors which have always served to distinguish acids from alkalis, and which still continue to be the best means possessed by chemistry to prove the presence of either of them, have been found insufficient to determine the complete neutrality of a salt.

It is not that colored reagents were deficient in sensibility with regard to saline solutions, to show the more or less complete state of saturation of the acid and base, but that the indications which they gave did not accord with the preconceived opinion and the principle of classification which it was desired to establish.

Indeed, in the reaction of the salts in solution on the blue or reddened color of litmus, we find that many of them behave like acids or alkalis, and that it is only in a very few that the acid and the base

are so exactly saturated by each other, as not to produce any evident modification in the vegetable colors, and thus present, at least in appearance, the character of neutrality properly so called.

The example of a really neutral salt being such a very rare case, it was impossible to generalise the exception. Notwithstanding this difficulty, classes of neutral salts were established, in which were included salts which were acid and alkaline to colored reagents. Salts without action on vegetable colors, as well as those which are acid and alkaline to these reagents, were considered as neutral, and the following plan was thought of for doing away with the evident contradictoriness of such a designation; two sorts of neutrality were admitted, one real and evident by colored reagents, the other hypothetical and being merely an induction from the first.

Choosing as type in each species of salts that which is neutral or the most neutral to reagents, and taking as term of comparison the relation which exists between the oxygen of its base and the oxygen of its acid, it was determined that all salts which presented this same relation, should be called neutral salts.

For example, in sulphate of potassa (KO, SO^2), which exerts (or is believed to do so) no action on vegetable colors, and which is consequently supposed to be quite neutral, we find the proportion of 1.3 between the oxygen of the base and the oxygen of the acid; this proportion, therefore, became the generic character of the neutral sulphates, whatever may be the reaction of the salt, the degree of oxidation and the number of equivalents of the base and that of the acid. Thus the sulphate of peroxide of iron $\text{Fe}^2 \text{O}^3 (\text{SO}^2)^2$, which has a decidedly acid reaction, is considered as completely a neutral salt as sulphate of potassa (KO SO^2). The peroxide of iron, $\text{Fe}^2 \text{O}^3$, is assimilated to the potassa, KO, and a salt which contains 3 equivalents of acid to a salt which contains but one.

The generic proportion of the other neutral salts has been determined in a similar manner.

If all the acids, without exception, formed with the powerful bases at least one salt neutral to colored reagents which could be taken as a type, this system, notwithstanding its defects, would be capable of a general application.

But this is not the case, and when the formula of neutral carbonates or sulphates has to be decided, we are left in the most perfect uncertainty, because as the salts which carbonic and sulphurous acids form with the powerful bases are all alkaline to reagents, and not one of them can be taken as a neutral type, there is no means of choosing the proportion which would be the expression of the neutrality of these classes of salts.

We see, therefore, that the indication of colored reagents is the important point, and when this is wanting or doubtful, this system of classification is inapplicable; it follows, moreover, that it has no foundation if we find that the salts which served to determine the neutrality of each class of salts are not themselves neutral to colored reagents.

Now, it is easy to demonstrate that the salts which have hitherto been considered neutral are not so, and that there exist only alkaline and acid salts.

Thus, we can easily discover the alkalinity of the following salts:—

Sulphate of potassa, which has been crystallised six times in succession, after having been calcined with sulphuric acid;

Sulphate of soda after four crystallisations;

Sulphate of magnesia after four crystallisations;

The chlorides, iodides, and bromides of potassium, sodium, strontium, calcium, and magnesium, after numerous crystallisations;

The nitrates of potassa, soda, baryta, and lime;

The chlorates and perchlorates of potassa, soda, and baryta, after several crystallisations.

I do not speak of the oxalates, acetates, and other salts with powerful bases whose alkalinity is not doubted.

These results, which differ from those hitherto obtained, depend entirely on the degree of sensitiveness of the colored reagent used, and also on the time during which it is left in contact with the saline solution.

Indeed, if we steep reddened litmus paper in a perfectly pure solution of sulphate of potassa, we do not at first observe that the blue is restored; in other words, that sulphate of potassa has an alkaline reaction.

But if, instead of litmus, dahlia paper is used, its violet color takes, after a short immersion, a very distinct greenish tinge, demonstrating that sulphate of potassa is manifestly alkaline.

I have found that reddened litmus paper, prepared by the means which are recommended as giving it the maximum of sensitiveness, is likewise turned blue by a solution of sulphate of potassa, when it is left in it for some hours.

In the experiments made on saline solutions, dahlia paper has been employed concurrently with that of litmus, and the indications of the latter, although not so prompt, have always confirmed those of the dahlia paper.

These results, and the still more striking example of the sulphites and carbonates, prove that there is no such thing as complete neutrality. Now, if neutrality of reaction does not exist, how can we deduce neutrality of composition? Is it by the proportion of oxygen of the base to that of the acid existing in the most neutral salt? But this same proportion is found in the most acid salt. It would, consequently, be a matter of indifference whether we select as type the most or least perfectly saturated salt.

The application of this system, which rests upon the principle of neutrality, leads to consequences which are essentially opposed to this principle and contradictory to each other; that is to say, to the most distinct alkalinity and acidity.

It is, indeed, a strange result that starting from sulphate of potassa, we should arrive at sulphate of peroxide of iron, which differs from the former, as much as one salt can differ from another, in reaction, in

the degree of oxidation of its base, in its composition, and in its chemical behavior.

We must, therefore, acknowledge, that no relation exists between the reaction of a salt and the proportion of oxygen of its base to that of its acid; that this proportion being neither the consequence nor the principle of neutrality, cannot be its expression.

We have seen that the type salts which have served for the formation of the neutral classes are alkaline in their reaction. We shall now proceed to show that they are not neutral with respect to their composition; for this purpose it is only necessary to recal^l past experiments, and to give them their obvious sense.

It will be better to explain as precisely as possible what we understand by the neutrality of a salt.

M. Chevreul, treating of the action of saline solutions on colored reagents, expresses himself thus:—

“An absolutely neutral salt would be one whose principles could no longer unite with bodies with which they combined before their neutral combination.”

This definition, which applies so well to neutrality of reaction, is also the most exact that can be given of neutrality of composition.

A salt neutral in composition would be one which, with respect to its acid and its base, manifests no affinity for more considerable quantities than already constitute it; in other terms, which cannot be rendered more acid nor more basic, because its acid and its base are supposed to be in a reciprocal state of complete saturation.

If we now examine the different salts classed as neutral, we remark that most of them will combine with further quantities of base or of acid, that consequently they are not really saturated—they are not veritable neutral salts.

Among alkaline salts, both earthy and metallic, we find that:—

Carbonic acid unites with bases in the proportions of 1 : 1, : 2, : 3, : 4; 2 : 3, : 4, : 5.

Sulphuric acid in the proportions of 1 : 1, : 2, : 3, : 4, : 5, : 6, : 7; 3 : 2, : 3.

Phosphoric acid in the proportions of 1 : 1, : 2, : 3, : 4, : 5; 2 : 3, : 4, : 5, : 6, : 7, : 8.

Arsenic acid of 1 : 1, : 2, : 3, : 4, : 5; 3 : 2.

Chromic acid 1 : 1, : 2, : 3, : 4, : 5; 4 : 1.

And finally silicic acid of 1 : 1, : 2, : 3, : 4, : 6, : 12; 2 : 3; 4 : 3, : 6, : 9; 2 : 1; 3 : 1; 4 : 1; 6 : 1; 8 : 1; 10 : 1.

Amongst all these combinations containing such various proportions of base and acid, which is the one which is to be selected as the best saturated, and serving as a neutral type of each description of salts?

Is it the carbonate with 1, 2, 3 or 4 equivalents of base?

Is it the sulphate with 1, 2, 3, 4, 6 or 7 equivalents of base?

Is it the silicate with 1, 2, 3, 4, 5, 6 or 12 equivalents of base? Or the silicate with 1, 2, 3, 4, 6, 8 or 10 equivalents of acid?

Can we, for example, consider as neutral the sulphates of potassa or soda? Evidently not, for if the alkali were completely

neutralised by a single equivalent of acid, it would not unite with a second equivalent of sulphuric acid, and the bisulphates of potassa and soda would not exist. The alkaline carbonates, sulphites and borates are in the same category; they are not neutral, because they produce the bicarbonates, the bisulphites and various other borates which contains as much as 6 equivalents of boracic acid.

Neither can we admit that the potassa in the chromate (KO Cr O_3) is saturated when it can take 2 and 3 equivalents of chromic acid ($\text{KO}, 2 \text{Cr O}_3$) ($\text{KO}, 3 \text{Cr O}_3$).

It is unnecessary to press this point further, for the basic or acid salts are an evident negation of the *neutrality* of the corresponding *neutral* salts.

However, there do exist some combinations which present the characters of neutrality of composition, that is to say, which are in such a state of saturation that they cannot become either more basic or more acid.

It is thus with most of the salts formed with very oxygenous acids, whose action is, moreover, very weak on colored reagents.

Thus we do not find binacid or bibasic salts among the nitrates, chlorates, perchlorates, or bromates of potassa, soda, baryta, strontium, lime, or magnesia; but that is essentially in consequence of the nature of the base and of the acid: for if no binirate of potassa exists, there are the biniodate, the trisiodate, the biborate, and sexborate of potassa. If basic nitrate of potassa does not exist, there are bibasic nitrates of antimony, bibasic and tribasic nitrates of silver, and quadribasic nitrate of copper. There are also the chlorate, perchlorate, and bibasic bromates of mercury, and the sexbasic bromate of copper.

These irregular combinations, formed by the weak bases, would not touch the neutrality of combinations with powerful bases, if they existed; but it must be acknowledged that the nitrate of potassa, because it contains but one equivalent of base, in no way establishes the neutrality of nitrate of copper which can take 4 equivalents of oxide.

Consequently, to conclude that nitrate of copper (Cu O NO_3) is neutral, because the proportion of the oxygen of its base to that of its acid is the same as that observed in nitrate of potassa, is to admit that they possess an equal power of saturation with regard to an acid.

Although experiment has proved that the same acid is completely and very equally saturated by different bases, still salts have been indistinctly united in one class, which are incompatible by their reaction and state of saturation.

A neutral salt may, therefore, be considered as the external result of two affinities reciprocally and rigorously satisfied, as an isolated fact which can only be generalised by creating erroneous analyses.

In this respect it appears that nature has not desired to render the elements immovable by causing them to form combinations whose neutrality is perfect, and that the essential condition of their move-

ment, which is almost infinite, should be this incomplete solution, which leaves the circle of their reactions open.

However this may be, we find no absolutely neutral salts.

As regards certain acid and basic salts, we may, it is true, consider them as double salts, in which the second equivalent of acid or of base would not be attracted or retained by the incompletely satisfied affinity of the base or acid, but by the tendency which unites two salts to form a double salt.

This interpretation rests on the double function of water, which is a base to acids, and an acid to bases.

The remarkable work of Mr. Graham on phosphates, and the impossibility of isolating in an anhydrous state, the greater part of the acids, have given great likelihood to the basineess of water, and we now readily admit that the hydrated acids, such as sulphuric, tartaric, oxalic acids, &c., are salts of oxide of hydrogen, so that the bisulphates, bitartrates, binoxalates, &c., may be considered as double salts.

We cannot, however, always imagine such a mode of combination; bisulphate of potassa can be obtained acid by calcination, which renders it anhydrous, and leaves the salt thus constituted $\text{KO } 2\text{SO}^3$. There is here no double salt, and, moreover, it is remarkable that the second equivalent of sulphuric acid is not driven off by a temperature, (nascent redness) which is much higher than that at which anhydrous sulphuric acid SO^3 is distilled, 30°C (86°F).

By what force is the sulphuric acid so powerfully retained, if not by an affinity which subsists in the sulphate of potassa, an affinity which, by being exerted with regard to an acid, can only proceed from the potassa? Consequently, sulphate of potassa is not neutral.

Bichromate of potassa, which is anhydrous, is likewise an example of an acid salt, whose existence cannot be attributed to the formation of a double salt. Chromate of potassa is alkaline.

On another side the hypothesis started that the hydrates are to be considered as salts in which water is the acid, in the same way that the hydrated acids were salts of oxide of hydrogen; and in this we cannot consider as strange the double function attributed to water (H O), for this is not the only compound which has the property of being a base to acids, and an acid with regard to bases (silica, alumina).

Now, if the hydrates and hydrated acids are salts, the hydrated basic or acid combinations are double salts; we shall thus understand the juxtaposition, to a salt already alkaline or acid, of an excess of base or an excess of acid which would cause no reaction.

The formation of acid or basic salts is, therefore, explained, either by their reaction, which indicates an incomplete saturation of their elements, in consequence of an active affinity which requires to be satisfied, or by their association with hydrates or hydrated acids.

After examining the hypotheses by which the formation of acid and basic salts have been explained, we are led to acknowledge that it

is still in the natural play of ordinary affinities that we find the most simple and rational interpretation, which consists in admitting that the elements of a salt are not neutralised as long as they manifest any affinity for any quantity whatever of the acid of the base contained in them.

It is, therefore, useful to examine whether neutrality of reaction has not a direct relation with neutrality of composition, and whether we have not deserted the very clear indication of colored reagents, because we have expected from them that which they were not calculated to give; in other words, because we have wished to find more neutral salts than existed.

When we are guided by colored reagents, we find that combinations are divided, first, into neutral salts, or at least those which appear to be such; secondly, into alkaline salts; thirdly, into acid salts.

In this classification we remark—

1st. That the salts with a neutral reaction, or nearly so, do not produce basic or acid salts; example—the nitrates, chlorates, perchlorates, of powerful bases.

2nd. That the salts with an alkaline reaction form acid salts; example—the carbonates, borates, sulphates, phosphates, arsenates, and chromates of potassa and soda.

3rd. That the salts with an acid reaction give rise to basic salts; such are the greater part of the metallic sub-salts.

From this simple statement does it not appear evident that chemical tendencies of a salt are connected with its reaction? Does not acidity cause us to anticipate the formation of a basic salt, and alkalinity that of an acid salt, and neutrality an absolute indifference?

Whence, it follows, that the reaction of a salt with respect to colored reagents is, without doubt, the most important of its characters, because it indicates at once the relative saturation of its elements, the nature of the affinity which continues active and by that means the nature of its ulterior modifications.

Without any hypothesis this character may serve as the basis of a natural and logical classification, but very different from that which it has been pretended to establish in accordance with the same principle.

Only reckoning the results of experiment, saline combinations may be divided:

1st. Into neutral salts, which remain indifferent.

2nd. Into acid salts, which may become basic.

3rd. Into alkaline salts which may become acid, that is to say, into three perfectly distinct classes, of which each contains salts possessing common characteristics, the same reaction, and consequently the same chemical tendency.

If we consider, on the contrary, the reaction of the salt, and the relation existing between the oxygen of its base and that of its acid, we unite into the same class the neutral, acid, and alkaline salts, salts with every reaction.

Thus, on the one hand, we have a rational classification, and on the other, the greatest confusion, because in the one case the character of the reaction is fixed and applied as the one principle, and in the other, this same character, at first invoked, is abandoned in application.

The division of saline compounds into three classes according to the indication of colored reagents, appears to me free from all objections, for it is the result of experiment; in fact, a salt must be always neutral, acid, or alkaline.

We have now seen by what considerations the real sense of the word *neutrality* has been twisted and extended, and how it is that we no longer understand clearly either words or things, and have formed a classification which confounds what it was intended to distinguish, by uniting combinations with opposite characteristics.

I have endeavored to show that if neutrality of reaction cannot serve as the basis of a classification, because this character does not exist in an absolute manner, or is in all cases very exceptional, the very nature of the reaction of each salt with litmus paper has a distinctive character, and thus becomes the basis of a natural division of combinations into three groups.

The neutral salts (more or less).

The acid salts.

The alkaline salts.

Admitting as an immediate consequence, that the reaction of a salt indicates the degree of saturation of its elements, we can ascertain that its chemical tendencies accord with this character, and we can anticipate from the neutrality of a combination, its chemical indifference, from its acidity, its tendency to become basic, and from its alkalinity, its tendency to become acid.

Although this is the case in most instances, we should, however, consider certain exceptions, and endeavor to explain them satisfactorily.

Among these exceptions are found salts which, already alkaline, or already acid, will become still more acid or alkaline, this cannot be explained by the action of an incompletely satisfied affinity, because the reaction of the salt indicates, on the contrary, supersaturation.

We are, then, obliged to have recourse to the hypothesis which makes water play the double part of base and of acid, and by which we have considered the acids and hydrated bases as salts, and their combinations with acid or alkaline compounds, as double salts.

But whatever be the value of this hypothesis, which is here but of secondary importance, we must not lose sight of the principal fact, namely, the subdivision of salts into three classes, according to their respective reaction with litmus paper.

There are, therefore, two principles which cannot be considered simultaneously; the reaction of the salt with litmus and the relation of the oxygen of its base to that of its acid. The first rests on a chemical property which is characteristic for each salt, the second on a purely numerical relation. There is no tie between them; and moreover,

if we observe that instead of the relation of the oxygen of the base to that of the acid, we might take the sum of the metals to that of the oxygen, we should obtain quite as simple relations, we shall remain convinced of how little importance should be attached to these classifications.

From the preceding it results that the designation of neutral salts, taken in the present sense of the word, is erroneous, and that by reason of the influence exercised by the nature of the bases and acids on the basicness or acidity of combination, it is preferable to represent the pure and simple composition of a salt without endeavoring to indicate a state of neutrality, or of saturation which does not exist.

It is, moreover, unnecessary for this purpose to change ordinary language, for with salts with 1 equivalent of base and 1 equivalent of acid, the actual designation expresses their composition (carbonate of potassa, sulphate of soda, and nitrate of baryta).

For salts with 2, 3, 4, 5, 6 and 7 equivalents of base, to 1 of acid, it is the same (bibasic, tribasic, quadribasic, &c., sulphate of zinc).

We designate in an analogous manner the acid salts of various degrees (bisulphate, quadroxalate, sexborate of potassa).

As to combinations formed of irregular proportions of base and of acid, the expression of their formula is scarcely more difficult.

Undoubtedly language becomes less simple and less brief as the composition of the salt becomes more complicated; but this is an inconvenience which can only be escaped by leaving the sense of the formula vague and incomplete.

Our nomenclature enables us to tell the whole in terms which do not become thus embarrassed until it becomes an impossibility to use them.

"The French nomenclature," says M. Dumas, "is a monument which must not be touched rashly."

If we have to guard against modifying this nomenclature in any of its details without great reason, we must likewise avoid straining its application; we must leave its clearness and logicity untouched, and bear the slight inconveniences which it presents for the sake of its advantages.

It is clear and logical when in perfect harmony with the formula, it ceases to be so when it does not reproduce it entirely and as it is.

Now, when we say sulphate of peroxide of iron, and we write $\text{Fe}^2 \text{O}^3 3 (\text{SO}^3)$, the nomenclature does not indicate that we allude to 1 equivalent of peroxide of iron and 3 equivalents of sulphuric acid.

This discord results from the wish that an entirely acid compound should be considered and designated as neutral. It may, perhaps, be said that this is an anomaly which all understand, a general convention; this is true, and if this anomaly were a simplification, it should be retained; but if, instead of an advantage, it complicates and falsifies the nomenclature, it should be renounced.

Would it not be preferable always to mention the formula as written? Ordinary language would thus gain in truth and precision,

and we should soon find that the denomination of *neutral salts* would disappear, having no real character, and not possessing even the theoretical signification and importance which has been mistakenly attached to it.

Annales de Chimie et de Physique, November, 1856.

On a CASE of SPONTANEOUS DECOMPOSITION of CHLORIDE of LIME.

By M. LANDERER.

WHILE the cholera was at Athens, I sent to Trieste for a great quantity of chloride of lime for fumigation: it was of a pasty consistence; but answered perfectly, for the purpose for which I required it.

This chloride was deposited in a cellar, in an earthen vessel covered with a lid. From time to time I looked at it and found it in good condition, only that it became more liquid from the humidity of the cellar; I determined to prevent this by closing the vessel hermetically. One day there was a sudden odor of chlorine diffused through the house; I went into the cellar and I found the chloride of lime frothing over the top of the vessel and running down the sides. There is no doubt that the humidity of the cellar had produced this decomposition; the water had decomposed, hydrochloric acid had formed, which had decomposed the hypochlorite of lime, and occasioned the disengagement of the chlorine.

The phenomenon thus described, was produced twice more with the same chloride. In the interval a fresh quantity of hydrochloric acid was formed, which acted on the chloride of lime; this continued until all the chloride of lime had been converted into chloride of calcium.

Archiv. der Pharmacie.

INDUSTRIAL CHEMISTRY.

OIL of COTTON SEED. By Mr. WAYNE.

THE manufacture of oil from cotton seed is not a novelty; a small quantity has been made at the South for some years past; lately, however, the demand for lubricators has turned the attention of many to the cotton seed (immense quantities of which are allowed to rot, or used only as manure upon the cotton fields of the South) as a material from which large quantities of oil might be profitably obtained.

At New Orleans I have been informed that a quantity of this oil has been of late produced, a sample of which I have seen. It was a very bland, light colored oil, said to be made by steaming the seeds and collecting the oil by skimming it from the surface of the water. I cannot vouch for the correctness of the above process.

In Cincinnati some of the oil millers have made the attempt of pressing cotton seed for the oil, but the experiments so far, I believe,

have been unsatisfactory, both in regard to the quality of the oil obtained, and in the cost of it.

The oil, to be made profitably, should either be manufactured in the vicinity of the cotton plantation,—as the seeds from the attached fibre are bulky, and the cost of transportation an item,—or the seed should be hulled at the spot, and shipped to the place where it is to be pressed in that condition, as it requires three or four bushels of seed in the wool to produce one bushel of hulled seed ready for the mill.

The oil as it runs from the press is of a very dark red color. After standing some time it deposits a portion of the coloring matter, also a portion of a semi-solid fat; and in cold weather this is deposited to a large extent; and is only partially taken up upon increase of temperature. The color of the oil obtained by pressure is one great objection to its general use, and is very difficult to remove; in bleaching it, at least ten or fifteen per cent. of the oil is lost, a portion of which may be recovered, and used in the manufacture of soap, for which purpose cotton seed oil is better adapted than for any other purpose. It is a drying oil, consequently not fit for lubricating, and when burnt gives a smoky flame, and forms rapidly a crust upon the wick. Others say that it answers for both purposes; but all I have met with, upon being used practically for lubricating or burning, gave unsatisfactory results.

A very good soap is made from it in New Orleans for common purposes; but I think that a soap in every respect equal to the imported Castile could be cheaply made with it.

The dark color of the oil obtained by pressure is due to the presence of a dark resinous substance, presenting itself in small dots throughout the seed. These may readily be seen by examining a section of the seed with a lens, or even the naked eye.

The hull and attached fibre are useful for paper stock; and the cake left after the extraction of the oil is nearly as valuable a food for cattle as that of linseed.

Proceedings of the American Pharmaceutical Association.

On OIL of PIGNUT HICKORY. By MR. WAYNE.

THE *Caraya porcina*, commonly known as the pignut hickory, is a forest tree of wide range in North America. This tree produces a nut which contains a very oleaginous kernel, between which and the shell, and the lobes of the kernel, a corky matter of a very astringent taste is deposited. The nut in consequence is not edible, but yields by crushing and pressure a light yellow colored oil, possessing a bland, slightly astringent taste; the astringent taste is due to the accidental presence of a trace of the astringent matter mentioned, which might easily be removed, and then it would be equal for many purposes of the pharmacist to the best olive oil.

This oil has been for several years past an article of commerce in

Ohio, the market value of which ranges from 1 dollar to 1 dollar 25 cents per gallon, and the supply of late has not been equal to the demand.

It is very rich in olein, and remains consequently fluid at a much lower temperature than most fixed animal and vegetable oils. During the extreme cold of last winter in Cincinnati, where sperm and other oils were in a solid condition, this oil thickened, but could be readily drawn from the vessel containing it.

As a lubricator it is said by those who have used it to be equal to sperm oil, and for the purposes of illuminating not inferior.

The value and extensive demand at present for oils for lubricating and other purposes, should attract attention to the production of oil from this nut, thousands of bushels of which are allowed yearly to fall to the ground and rot, which if collected and the oil extracted, would cut off the necessary importation of a large amount of oils, such as the rape, olive, &c., and pay a handsome remunerating profit to the maker. I have been informed by those who make it in Ohio, that the yield per bushel is one and a half gallons, and that they purchase the nuts, through the country, of the store-keepers for 50 cents per bushel.

Proceedings of the American Pharmaceutical Association.

On FLORIDA INDIGO.

INDIGO was formerly cultivated in Florida, for which the climate and soil is well adapted. It grows wild upon the barrens in almost every portion of the Peninsula. When cultivated by the English in this country, the indigo of Florida was considered in the London market superior to all others, except that of Caraccas. The manner of cultivating and manufacturing advantageously is as follows:—

The seed, which is very small, is soaked for some twelve hours, then mixed with ashes or sand, and sown in drill rows, about eighteen inches apart. The time for sowing in Florida is from the middle of March to the first of April. When the young plant makes its appearance, it resembles white clover, and must be carefully weeded, and the earth kept loosed about its roots. Three weedings are sufficient to carry it up to the first cutting, which commences when the plant begins to bloom, say about the first of July. The plant is so easily injured by the sun after it is plucked, that the cutting should be in the afternoon. As fast as it is cut, which is done by a sickle, it is carried to a vat called the steeper. This vat is made of plank, is water-tight, and varies in size according to the extent of the operations of the planter. The steeper is filled with cuttings immersed in water. Planks with weights upon them are then placed on top to keep the cuttings beneath the water. In this state steeping is continued for about ten hours or less, according to the temperature of the water. When the water assumes an olive color, it is drawn into the "beater," another vat placed alongside and beneath the steeper, and connected by a tube, and fastened with a valve or spigot. The liquid is now churned by hand or with machinery, until it becomes lighter

in color, and a blue fecula begins to make its appearance. From time to time lime-water is thrown into the beater during the "churning." After the fecula spoken of distinctly appears, the water is suffered to remain about four hours for the indigo to settle. It is then drawn off, the sediment placed in bags, and hung up to drain. When drained sufficiently, it is placed in boxes to dry, under gentle pressure; and when dried firm, it is cut up into square cakes and placed in the shade to become completely dried by evaporation. The shorter the steeping and the less the beating, the lighter will be the color of the indigo. The indigo plant will yield two or three cuttings a season, and one hand will cultivate about three acres, the result being from 175 to 200 lbs. of the article. Unlike sugar-cane or corn, the indigo requires no expensive machinery. Where it is made only for domestic use, barrels are used for steeping and beating.

Florida News and American Journal of Pharmacy.

AGRICULTURAL CHEMISTRY.

EXPERIMENTS on the FERTILISING POWER of VARIOUS KINDS of OIL CAKE. By M. CORENWINDER.

FOR the purpose of ascertaining the fertilising properties of some foreign oil cakes offered in the market at Lille, as being preferable to the indigenous cakes, I made some experiments, with the assistance of M. Jules Lepacq, a skilful farmer in that neighborhood, on the cultivation of beetroot, manured with various kinds of foreign oil cake.

In the month of April, 1855, after having prepared by ploughing and harrowing a field on which oats had grown last, and which was consequently much impoverished, we divided it into parcels of 2 aires, 21 centaires each, (25 yards), and we manured them separately.

- 1st, with 100 kils. of crude arachide cake
- 2nd, " 100 " of sesamum "
- 3rd, " 100 " of touloucanna * "
- 4th, " 100 " of poppy "
- 5th, " 100 " of camelina "
- 6th, " 100 " of rape "
- 7th, " 100 " of linseed "

These parcels were sown with the same quantity of seed, (250 grammes for each parcel) of the same species. The weeding was performed at the same time, and by the same laborer; the plants thinned as nearly as possible to the same distances; in one word, every possible precaution was taken to avoid error.

The prices of the cakes were as follows:—

	100 kilos. francs.
Crude arachide	12.00
Sesamum	15.50
Touloucanna	18.50

* Touloucanna is the fruit of the *Carapa Touloumouna* of Senegambia.

	100 kilos. francs.
Poppy	19-00
Camelina	18-50
Rape	18-00
Linseed	18-00

In the month of October, 1855, the beetroot was gathered, and the yield of each parcel ascertained separately.

The yields were as follows:—

	kilos.
1st, with crude arachide cake	1,452
2nd, „ sesamum „	1,511
3rd, „ touloucanna „	1,320
4th, „ poppy „	1,585
5th, „ camelina „	1,326
6th, „ rape „	1,278
7th, „ linseed „	1,200

These beetroots we sold at 20 fr. the 1,000 kilos. If we deduct from this the cost of 100 kilos. of each cake, we find that there remains, to pay labor, rent, &c. :—

1. Crude arachide, 1,452 kilos. at 20 frs. =	29-04
100 kilos. of cake	12-00
	14-04
2. Sesamum, 1,511 kils. at 20 frs. = . .	30-22
100 kilos. of cake	15-50
	14-72
3. Touloucanna, 1,320 kils. at 20 frs. = .	26-40
100 kilos. of cake	13-50
	12-90
4. Poppy, 1,585 kilos. at 20 frs. = . .	31-20
100 kilos. of cake	18-50
	12-70
5. Camelina 1,326 kils. at 20 frs. = . .	26-52
100 kilos. of cake	18-50
	8-02
6. Rape, 1,278 kils. at 20 frs. = . .	25-56
100 kilos. of cake	18-50
	7-06
7. Linseed, 1,200 kil. at 20 frs. = . .	24-00
100 kilos. of cake	18-00
	6-00

We see, therefore, that the foreign oil cakes give greater profit than those which are indigenous; it is therefore much to be regretted that the former should be—as they are—entirely neglected, even in the arrondissement of Lille, so justly remarkable for its good farming.

Before publishing these experiments, we determined to await the crop of wheat which followed the beetroot this year, and which received no manure. It might be supposed that the greater effects produced by some cakes were in consequence of their being more active (hotter) than the others. After this last crop we examined the wheat with great attention, and we found no appreciable difference.

The fertilising power of the cake of arachide and touloucanna is confirmed by chemical analysis. The following is the composition given by my analyses:—

Crude Arachide Cakes.

Water	12.50
Oil	3.47
Vegetable albumen (nitrogenous matters)	32.26
Non-nitrogenous organic matters	45.17
Ash	6.60
	100.00

Touloucanna.

Water	12.50
Oil	4.46
Albumen (nitrogenous matters)	27.31
Non-nitrogenous organic matter	50.91
Ash	4.82
	100.00

Their amount of nitrogen is as follows:—

	Per cent.
Crude arachide cake	5.163
Touloucanna	4.370
According to M. Boussingault sesamum contains	6.800

On comparing these numbers with 4.92, which M. Boussingault gives as the amount of nitrogen in rape cake (I have found 5.285 in rape cake, in the neighborhood of Lille) we can estimate the fertilising power of these foreign cakes, and I do not hesitate to advise farmers to use them, as my practical experiments leave no doubt of their efficacy.

All experienced farmers know that there is great difficulty and some uncertainty in experiments on beetroot, proceeding especially from the fact that the growth from the seed is not always equal; thus fresh young plants are obliged to be planted where the seed has failed, and these transplanted plants never attain as good a development as the seed which has germinated well on the spot.

The above numbers must consequently be considered as only ap-

proximations, and not as absolutely representing the proportions of the fertilising power of the cakes employed.

These numbers only prove that foreign cakes are quite as good manure as the indigenous cakes, and that farmers are mistaken in not using them.

Journal d'Agriculture pratique, December 5th, 1856.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

EXPERIMENTS upon DIGESTION.

By FRANCIS G. SMITH, M.D., Professor of the Institutes of Medicine, in the Medical Department of Pennsylvania College.

AN opportunity was lately afforded to the writer of examining and experimenting upon Alexis St. Martin, the Canadian, with a fistulous orifice in his stomach; a man whose name is familiar to almost every medical student as the subject of the experiments upon digestion performed by the late Dr. Wm. Beaumont, of the U. S. Army.

It will be remembered that, when quite a lad, St. Martin received the contents of a loaded gun, accidentally discharged. The charge, consisting of powder and duck shot, took effect in his left side, and after tearing away the integuments, muscles, portions of the fifth and sixth ribs and of the left lung and diaphragm, perforated the stomach, creating a wound through which the contents of the latter organ escaped by an orifice large enough to admit the forefinger. This orifice has never closed, although the surrounding wound cicatrised readily.

The man has enjoyed general good health, and has been the father of a large family, whom he has supported, up to the present time, by hard labor.

With one single exception, that of the Esthonian peasant reported by Grunewald and Schroeder, St. Martin is the only instance in which the opportunity of watching the process of digestion, in an otherwise healthy person, has ever been afforded; the case reported by Circaud in the *Jour. de Physiologie*, so far as we can learn, never having been experimented upon.*

The experiments performed by Dr. Beaumont are so familiar to every student of physiology, that any analysis of them is uncalled for, even if the limits of this journal permitted it. The object of the present paper is merely to detail the results of recent observations, and compare them with those obtained from other sources.

Several questions relating to the physiological action of the stomach may be regarded as still unsettled; among these is that relating to the nature of the acid contained in the gastric juice, and the influence of this secretion upon the various alimentary principles as classified by Prout, to wit, saccharine, oleaginous and albuminous food. The present paper will be devoted to the consideration of these points.

* *Cyc. of Anat. and Phys.*, Art. Digestion.

It must be premised that the analyses were made upon the fluids obtained from the stomach while digestion was in progress, for that which was withdrawn from the organ while the man was fasting, (that is in the morning, before rising from bed,) was found to be putrescent, although only twenty hours had elapsed after its reception by the writer. The analyses were conducted by the careful hands of Professor R. E. Rogers, of the University of Pennsylvania, in the presence of the writer, who thus acknowledges the valuable aid received by him.

In every instance, and with all the kinds of food employed, the reaction of the fluid of digestion was distinctly *acid* to litmus paper, while that of the *empty* stomach, as shown by the introduction of test papers through the fistulous orifice, and of the fluid obtained by mechanical irritation, was as distinctly *neutral*. The *temperature* of the stomach, while digestion was in progress, was about 100° to 101° Fahr. When empty, about 98° to 99° Fahr.

The fluid for examination was obtained by placing the man upon his right side, and gently introducing a large sized gum elastic catheter, or a small glass speculum. He was then allowed to turn himself towards the opposite side, when the contents of the stomach would readily flow out. In no instance was food allowed to remain in the stomach longer than two hours. The mucous membrane of the empty stomach presented a pale pink color, as described by Beaumont, with the surface lubricated by mucus; when digesting, its color was deepened, and the peristaltic motion could be distinctly seen. During all the experiments St. Martin maintained his usual good health, was in excellent spirits, and took his food with appetite.

Previously to the opportunity afforded to Dr. Beaumont by St. Martin's accident, specimens of gastric fluid were obtained by means of sponges attached to strings, as was done by Reaumur and Spallanzani; by exciting vomiting after eating, as recorded by Leipzig; by killing animals, while digestion was going on, as was done by Prout and others; or by establishing fistulous orifices in the stomachs of lower animals, as performed by Blondlot, Lassaigne, Bernard and Barreswill, and others since them.

All these observers, from the earliest to the latest, agree on one point, to wit, the existence of an acid reaction in the fluid of digestion; but as to the *nature* of that acid, they differ widely, some contending that it is *organic*, others that it is *mineral*; some that it is acetic, lactic, or butyric acid, others that the acidity depends upon the presence of hydrochloric acid, or upon the acid biphosphate of lime. The latter theory, advanced by Blondlot, has been disproved by Dumas, Bernard and Melsens, who have shown, that not only the carbonate, but the basic phosphate of lime, are soluble in gastric juice, as are also zinc and iron, with the evolution of hydrogen gas,—properties which a solution of acid phosphate of lime does not possess.*

The analysis of the fluid obtained from St. Martin by Dr. Beaumont

* Phys. Chem., by C. G. Lehmann.

in 1833-4, and submitted to Prof. Dunglison, then of the University of Virginia, showed, as the latter states, the presence of "free muriatic and acetic acids, phosphates and muriates, with bases of potassa, soda, magnesia and lime, and an animal matter, soluble in cold water but insoluble in hot." Dr. Dunglison further states, "we distilled the gastric fluid, when the free acid passed over, the salts and animal matter remaining in the retort. The quantity of chloride of silver thrown down was astonishing."*

Previously to this analysis, in 1824, Dr. Prout had made the same assertion as to the presence of hydrochloric acid, based upon the examination of the contents of the stomachs of rabbits killed while digesting; and Braconnot, in 1835, subsequently to Dunglison, states that he obtained evidences of free hydrochloric acid in gastric juice obtained by sponging the stomachs of animals.†

More recently, Bernard and Barreswill, Pelouse and Thompson, have been led to believe, from their own experimental researches, that *lactic* acid is the agent upon which the characteristic reaction of the gastric juice depends, and attribute the presence of hydrochloric acid in the free state to the decomposition of the alkaline chlorides by the lactic acid at a high heat. Hence, supposing *lactic* acid to be present in the fluid of digestion with the chloride of sodium, the fluid which passes over by distillation will, *at first*, be destitute of hydrochloric acid; but as the liquid becomes more concentrated and the temperature rises, hydrochloric acid will pass over.‡ Lehmann denies the power of hydrochloric acid to decompose the chloride of sodium, but asserts that chloride of calcium is decomposed by lactic acid, even *in vacuo*; and that hence it is not surprising that pure gastric juice should develop vapors *in vacuo*, which, when passed into a solution of nitrate of silver, should form chloride of silver.§

Still more recently, Messrs. Bidder and Schmidt declare, as the result of eighteen corresponding analyses, "that pure gastric juice of carnivora, after eighteen to twenty hours fasting, contained *free hydrochloric acid only*, without a trace of lactic or any other organic acid; while the gastric juice of herbivora contains, with free hydrochloric acid, small quantities of lactic acid, which may, however, be referred to their more amylaceous food."¶ Grunnewald's experiments led him to the conclusion that the acid was an organic one; while Schroeder maintains that the fluid obtained by irritating the stomach by peas, owed its reaction to hydrochloric acid.¶

Amidst all this conflict of opinion a reconciliation is scarcely to be hoped for; it is suggested, however, that a portion of it, at least, may be owing to the variety of animals experimented upon, and the ques-

* Experiments and Observations on the Gastric Juice, by Wm. Beaumont, M. D., Surgeon U. S. Army.

† Annales de Chimie, T. 59, p. 348.

‡ Carpenter's Human Physiology, Amer. Edit. p. 109.

§ Phys. Chem. vol. i., p. 93.

¶ Cyclopædia of Anat. and Phys., part xlvii., Art. Stomach and Intestines.

¶ Dissert. Inaug.

tion may be asked, whether observations made upon the human subject in the healthy condition, should not be relied upon, rather than those derived from experiments performed upon lower animals, in whom the severity of the operation and the emotions necessarily excited thereby, must unavoidably vitiate the results. The difficulty is somewhat relieved by the fact that only *two* acids are involved in the question, and it narrows itself to the decision as to whether they are both present together, or whether one substitutes the other. The following experiments may serve to decide this question.

May 6th, 1856, at 10, A. M.—Two ounces of dry wheat bread were given to St. Martin, which he masticated deliberately and swallowed. At 12½, P. M., the contents of the stomach were removed by Dr. Bunting, in the presence of a number of medical gentlemen and students, and carefully preserved for immediate analysis. The reaction was *decidedly* acid, s. g. 1009. Microscopic examination showed large epithelial cells, mucous corpuscles, amorphous granular matter and starch granules, some broken down, others perfect, together with a few cells of cylinder epithelium.

Experiment 1.—A portion of the fluid thus obtained, was subjected to distillation. In the early vapor that came over, no trace of acidity could be detected by litmus paper; the distillate was neutral, neither acid nor alkaline, and did not precipitate with nitrate of silver. The distillation being carried further, so as to concentrate the material in the retort and increase its temperature, the distillate was found to become acid, and a portion being added to a solution of nitrate of silver, a faint precipitate, which was soluble in ammonia, took place. (This experiment has been repeated since, with the material discharged from the stomach, *at will*, after a meal of bread. The distillate became *distinctly* acid, but threw down the faintest precipitate, a mere opalescence, with nitrate of silver. The acid of the distillate gave all the evidence of lactic acid.)

Ex. 2.—A portion of the material from the retort being tested with chloride of barium, gave no visible indication of sulphuric acid.

Ex. 3.—Another portion of the gastric fluid was heated in a porcelain capsule for the purpose of incineration. The vapor that escaped gave no evidence of acidity, but the residue became increasingly acid in proportion as it became more concentrated.

May 9th. Two ounces of bread moistened with water, were introduced into the stomach, through the fistulous orifice. In an hour and a half the contents were withdrawn. The fluid was very viscid, and presented, as before, a decidedly acid reaction. Some portions of the bread were undissolved, although the greater part had disappeared; sp. gr. not noted. The microscope revealed fewer epithelial cells than in the examination of the previous fluid, some mucous corpuscles and abundance of starch granules, some of which were broken down.

Ex. 4.—A portion of the fluid just obtained was decanted from the bread particles and carefully distilled, without presenting any evidence of acidity to litmus in the fumes. The distillate was acid to

litmus, and when tested with nitrate of silver, presented a *very faint* indication of the presence of hydrochloric acid. The residue in the retort, when somewhat concentrated, gave a deep acid reaction.

Ex. 5.—A portion of the same fluid, as in *Ex. 4*, was gently boiled in a retort; the distillate was acid, and when tested as before, gave the same faint evidence of the presence of hydrochloric acid. The residue, when taken from the retort and examined with litmus, was found more acid than before the distillation. It was then carefully evaporated and examined from time to time, with the effect of manifesting a constantly increasing acidity so long as it remained liquid. The heat was then carried still further, so as to dry but not char the material; on moistening it with water it was found even more intensely acid. Heat was again applied and carried to *incipient* charring, and then the material was moistened and tested again, exhibiting a *diminished* acidity. The same experiment repeated and carried to increased charring, showed, on moistening the residue, a still diminishing acidity, and on heating the residue to thorough charring and until all empyreumatic odor ceased to be given off, it was found that *all* acidity had disappeared.

Ex. 6.—It was suggested that the acid reaction of the residuum might be due to phosphoric acid, and that it might have been decomposed by the heat employed and the carbon which was present. To determine this, another portion of the same fluid was mixed with three drops of a solution of phosphoric acid, and the mixture was carried through the same series of experiments, giving a successively increasing acidity, with this peculiar result, however—that even after the whole material had been thoroughly charred, as before, and still more highly heated, the acid reaction still remained, thus demonstrating that the acid detected in the product of digestion in the first experiments was *not* the phosphoric.

Ex. 7.—To ascertain whether hydrochloric acid, if present in the free state, could resist the distilling heat and remain in the residuum when concentrated, a minute drop was added to a quantity of water so large as to render its reaction undetectable by litmus; and a like quantity to the fluid of digestion, and both were distilled. In both cases, a *very* distinct evidence of the presence of the acid was obtained in the distillate by a decided precipitate with nitrate of silver.

Experiments 6 and 7 go to show that the acid of the gastric juice, that at least upon which its most decided action depends, is not phosphoric acid, for it does not resist high heat, as that acid is known to do. It is probably not hydrochloric, nor acetic, for these are both highly volatile and are detected readily in the distillate by nitrate of silver.

Ex. 8.—This experiment was performed in verification of the doubt just stated, that hydrochloric acid is not present in the free state in the fluid of digestion. A portion of all the digestive fluids obtained from St. Martin, and a quantity vomited at will by another individual, were tested with pure deutoxide of manganese, without giving the slightest trace of the presence of chlorine.

Another portion of the digestive fluids was carefully filtered, and a minute trace of chloride of calcium added to it; the material was then tested with oxalic acid, when immediately the white precipitate of oxalate of lime took place. Had any free hydrochloric acid been present, it would have prevented the appearance of the precipitate by dissolving it. To prove this, another portion of the same gastric fluid was filtered, and a minute quantity of hydrochloric acid and chloride of calcium were added to it. The addition of oxalic acid now produced no precipitate. (See Lehmann's Phys. Chem. p. 93, vol. i.)

It thus became a demonstration that the strong acid reaction of these gastric fluids was not due to the presence of free hydrochloric acid. It seems equally clear that it was an *organic* acid, from the fact that it was destroyed by heat, as in Experiment 5; and almost certain that it was lactic. To decide this doubt, a portion of the distillate and another of the residue in the retort were tested with zinc, as recommended by Lehmann, (Phys. Chem. p. 92, vol. i.,) with the effect of producing the characteristic crystals of *lactate of zinc*.

It will be remembered that in experiments 4 and 8 a faint evidence of the presence of hydrochloric in the distillate was manifested by the reaction with nitrate of silver. It will also be remembered, that Bernard and Barreswill assert that this hydrochloric acid is due to the decomposition of the alkaline chlorides at a high heat. To ascertain this the following experiment was performed:—

Ex. 9.—Lactic acid was mixed with chloride of sodium, and the two were heated in a retort. The distillate gave the *faintest* possible trace of opalescence when treated with nitrate of silver. This evidence can scarcely be relied upon, for the solution above described required so high a temperature to produce ebullition, that it was difficult to prevent a spurious distillation of the chloride of sodium along with the vapor, and from this, it is believed, arose the opalescence in the reaction between the distillate and nitrate of silver. If lactic acid can decompose the chloride of sodium, it can only be in very small amount; *chloride of calcium*, as Lehmann has shown, can be decomposed by lactic acid, and if this be present in gastric juice with lactic acid, we may have hydrochloric acid developed by distillation.

May 8th.—A meal of roast beef, with a small portion of 'salt as a condiment, was given to St. Martin at 2 P.M. At 3½ o'clock of the same afternoon the contents of the stomach were removed. The fluid was viscid, inodorous, presented a flocculent deposit and a marked acid reaction; s. g. 1008. The microscope revealed numerous epithelial cells from the mucous membrane of the mouth downwards as far as the stomach, mucous corpuscles, amorphous granular matter, oil globules in great abundance, and transversely striated muscular fibres, in some of which the sarcolemma was softened and ruptured, and the sarcolemmas just liberated.

The gastric fluid was carried through the same series of experiments as those to which the product of bread digestion was subjected

to, and with a like result. The distillate was distinctly acid, but gave very faint traces of hydrochloric acid. The residuum became most intensely acid as it was concentrated, and the presence of lactic acid was manifested, both in the distillate and the residuum, by the test of the characteristic crystals of lactate of zinc.

From the preceding experiments the following conclusions are fairly deducible:—

1st. That the secretions of the stomach when digesting are invariably acid.

2d. That the acid reaction was not due to the presence of phosphoric acid.

3d. That if hydrochloric acid was present, it was in very small quantities.

4th. That the main agent in producing the characteristic reaction was *lactic acid*.

It is but just to say that the experiments were conducted with the utmost care and precision, with a single eye to *truth*, and not with a view to support any favorite theory of digestion. So far from this it may be stated, that the results arrived at are at variance with the doctrines maintained by the writer for many years. Each experiment was repeated several times, so as to leave no room for doubt, and was carefully compared with the results obtained by examination of the fluids discharged at will by another individual.

It is true that this is only one series of observations, and the first that has been published, so far as St. Martin is concerned, since the analyses of Dunglison and Emmett; and now that he has again become a subject for experimentation, perhaps other and different results may be obtained. There may be sources of fallacy, unknown to the writer at the time, which may vitiate the conclusions arrived at; if so, they will be abandoned as readily as those formerly maintained.

*Si quid novisti rectius istis, candidus imperti;
Si non, his utere mecum!*

(*To be continued.*)

PHOTOGRAPHY.

DRY COLLODION. By M. DUPUIS.

M. DUPUIS states that his process, which is nearly similar to that of M. Taupenot, is free from the little inconveniences found in the latter.

M. Dupuis' process consists in rendering the collodionised plates sensitive in aceto-nitrate of silver, washing them well and covering them with a layer of dextrine. Since the first publication of his process in November, 1855, M. Dupuis has regulated, if not modified his process, and he asserts, that he can now give certain directions to those amateurs who may wish to use it. It may be used for obtaining stereoscopic positives, and transferring them on to cloth or paper.

M. Dupuis gives the preference to collodion with iodide of zinc, which he makes in the following manner (temperature 20° C. 68° F.):—

Ether at 60°	180 cubic centimetres.
Alcohol, at 36°	60 " "
Gun cotton	2 grammes.

The solution is agitated and left to repose. When it is desired to use it a certain portion should be decanted, say 50 c.c., and to it, is to be added—

Iodide of zinc	0.5
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As soon as the solution is complete it may be used, filtering it through cotton. M. Dupuis prepares his iodide of zinc, which is very white and completely soluble, by precipitating old silver solutions with iodide of potassium, and reducing them with a plate of zinc, taking care to add some drops of tincture of iodine to begin the reaction. The metallic silver is precipitated as a black powder. The liquor retains iodide of zinc in solution. It is then only necessary to filter and evaporate to dryness with proper care. We thus obtain a very white and very pure product, which must be kept in a carefully closed bottle.

The plate, after receiving the layer of collodion, is plunged in a bath composed of:—

Nitrate of silver	20 grammes.
Distilled water	200 "
Crystallisable acetic acid	25 "
Washed and humid iodide of silver to saturation.	

The 25 grammes of crystallisable may be replaced by 40 grammes of common acetic acid of ten degrees.

The plate, after having remained in this for at least half a minute, is then put into a bath of distilled or rain water. It is left in this for several minutes: it is then removed and a fine stream of pure water run over it, until all greasiness disappears; it is then ready for varnishing.

The aqueous varnish of dextrine has always given M. Dupuis excellent results. It is composed by saturating 200 grammes of water with commercial dextrine, and adding two grammes of camphor to prevent it from becoming mouldy. This thick solution is then diluted with water until it only marks three or five degrees of the saccharometer. It is filtered every time that it is used.

When the sensitive plate has been well washed, a little of the dextrine solution is poured on and immediately poured off, so as to remove the excess of water; a sufficient quantity is then poured and kept for some moments on the whole surface.

It is poured off by one angle and it is left to drain in a vertical position in a case on a fold of blotting paper; the plate is very soon dry. If it is wanted for immediate use it may be dried at the fire or over a lamp, after being allowed to drain for about five minutes. A

heat of 50° or 60° C. (122° to 140°F.) does not injure the sensitive layer; still it is preferable to allow it to dry spontaneously.

Collodion with iodide of zinc, is rather less rapid than with iodide of ammonium; but for the dry process, it gives superior results. For the stereoscope with a simple sixth objective M. Dupuis exposes it for two minutes; with the simple normal objective from four to five minutes, according to the object to be reproduced.

Gallic acid, employed as if for albumen, perfectly develops dry collodion; but pyrogallie acid is preferable. M. Dupuis uses the following formula:—

Pyrogallie acid	.	.	.	.	.	1 gramme.
Distilled water	.	.	.	.	.	300 "
Citric acid	.	.	.	.	.	1 "

He has long substituted citric acid for acetic acid, as well for moist as for dry collodion. Citric acid costs much less, is found everywhere, and magnificent tones and splendid skies have been obtained with it.

When removed from the frame the plate is covered with a layer of water to moisten the whole surface. This water is run off and replaced by a solution of pyrogallie acid, which is allowed to remain for a few moments; this solution is then received in a small capsule, in which eight or ten drops of a solution of nitrate of silver of 5 per cent. have been put, and is then poured again upon the plate, which is moved about so that the whole is covered. After a few moments the picture begins to develope, and soon acquires great depth of tone. The action is stopped when wished; the plate is washed, concentrated hyposulphate of soda is used to fix the picture; it is again washed and left to soak for a quarter of an hour in plenty of water; it is then put up to dry and then varnished, unless we wish to transfer the image, which must be done as soon as the washing is complete.

Cosmos, November 28th, 1856.

DRY COLLODION *without* DEXTRINE or SYRUP.

By M. MONTREUIL of Tonnerre.

M. MONTREUIL asserts, that the preservation of the sensitiveness of dry collodion is not due in any way to the solution of dextrine, but *merely* to the washing which precedes its application.

He has made many experiments with dextrine and has found, that if the plates are not well washed before the application of the dextrine, they will only remain sensitive as long as this substance continues fresh and humid, but when carefully washed, they may be preserved indefinitely; the layer of syrup is, in his opinion, a useless and troublesome addition.

I affirm, he says, *that all collodion, which would answer well when moist, might, if carefully washed, be preserved dry and sensitive, I believe, for an indefinite period.* In a former letter, he spoke of a sensitive acid bath, and of the development of the image as being possible only

with the aid of gallic acid. He now goes further, the silver bath may be acid or neutral at pleasure; the development may be performed with gallic or pyrogallic acid; the fixing with hyposulphite, or with cyanide of potassium.

He has just arrived from Troyes, bringing six views taken on glass plates rendered sensitive long before, three weeks or a month, some with a neutral bath of moist collodion, some with an old acid bath, which he had used the year before for paper negatives. The result has been perfectly satisfactory, not a patch or a difference of strength between the acid plates and the neutral plates. He prefers developing with gallic acid, as giving deeper shadows.

It has been supposed that dry collodion always gave transparent skies. M. Montreuil has only found this the case when he wished it; and he is sure that this occurs with moist collodion as well as with dry, whether covered with syrup or not.

He cannot imagine that any one will persist in using collodion with syrup, when once the method which he proposes has been tried. It is as follows:—

ANY COLLODION WHICH ANSWERS WELL WHEN MOIST.

Silver bath, composed of—

Distilled water	.	.	.	.	100 grammes.
Nitrate	.	.	.	.	8 to 15 "
Acetic acid	.	.	.	.	8 to 15 "

to this may be added, if wished, a few drops of nitric acid.

Should a neutral bath be preferred, use the same bath as for moist collodion.

The development may be performed, either with gallic or pyrogallic acid. M. Montreuil prefers gallic acid. He operates in the following manner:—He takes a wide-mouthed bottle and he dissolves in it, in alkali and to saturation, but without heat, as much gallic acid as will dissolve, and this solution may be kept indefinitely. When he has a proof to develop, he takes a flat bath and pours into it about half a centimetre of filtered water; he puts in two or three tea-spoonfuls of the solution of gallic acid, a few drops of acetic acid, and a few drops of nitrate of silver. He puts his glass into this, the collodion upwards, and leaves the operation to continue. If the exposure has been sufficient and the bath very strong, at the end of a quarter of an hour, or even less, the proof will be evident in all its details, and should the tones be deficient in strength, the addition of a little nitrate of silver will rectify that; in one word, the process is precisely similar to that for paper negatives. Should pyrogallic acid be preferred the proof will develop more rapidly, but with less certainty. Take 300 grammes of water, 1 gramme of pyrogallic acid, 15 or 20 grammes of acetic acid, and only add a very small quantity of nitrate of silver, and that only when all the details of the proof are perceptible.

As for the exposure, M. Montreuil varies from three to seven minutes, according to the strength of the light, and the focus of the

objective; he uses a simple objective of 45 centimetres focus, and another of 70 centimetres.

He only washes his plates for about a minute, that is until the water, in running off, leaves no appearance of greasiness. He washes under a stream and not in a bath; this last method is insufficient and frequently delusive, so that the collodion which has scarcely adhered to the glass, sometimes is detached from it.

M. Montreuil is quite certain that those who follow his directions with ordinary care, are sure to succeed always.

Cosmos, November 28th, 1856.

PHYSICS.

On the MAGNETISM and ELECTRIC CONDUCTIBILITY of POTASSIUM and SODIUM. By M. LAMY.

MANY physicists have endeavored to discover some connection between the weight or atomic volume of bodies, and some of their physical properties—such as density, crystalline form, boiling point, specific magnetism, &c. With relation to this last property, it is generally admitted, that the most magnetic metals are those which have the smallest atomic volume—the least magnetic metals, those whose atomic volume is the greatest.

With the desire of ascertaining what degree of confidence should be placed in this hypothesis, I have arranged a table containing the names of the principal metals, their density, their exact chemical equivalents, according to the present opinion; and the quotient of these last by the second—that is to say, the numbers which represent the atomic volumes. Now, it results from an examination of this table—first, that the exceptions to this supposed law are very numerous, then, that sodium, and particularly potassium, should be much more diamagnetic than bismuth itself. This last consequence was too important to be passed over, without making some confirmatory experiments.

According to Faraday, sodium and potassium should be diamagnetic. But, notwithstanding the great authority of this illustrious man, I will venture to say that my experiments tend to prove, on the contrary, that these metals are very slightly magnetic. I have taken every possible precaution to avoid the presence of iron in the masses examined; and I have always found that, at the moment of the closing of the circuit of Ruhmkorff's electro-magnet, there was a powerful repulsion, due to the production of induced currents in the influenced mass, followed by a manifest and permanent attraction, when the electro-magnet attained its state of magnetic equilibrium. This attraction was exerted on a globule of potassium extracted with the battery, directly and without the intervention of mercury, from some powerfully diamagnetic potassa. In the same circumstances, globules of copper, and of pure silver, were likewise repelled by the poles of the magnet.

My experiments, therefore, appear to establish that potassium and sodium are really magnetic; feebly, it is true, but in a manner that is rendered indubitable by all the samples of these metals which I have been able to procure. Moreover, were they feebly diamagnetic, as asserted by Faraday, the relation between the atomic volumes and specific magnetism would not be confirmed by their example.

Rotary movement due to Magnetic Induction in the Metals Potassium, Sodium, Copper, Silver, Gold, &c.—The action of the electro-magnet on potassium and sodium determines, at the moment when the magnetism commences or ceases, powerful movements of repulsion and attraction, due to the production of induced currents in the mass. But a more curious phenomenon appears, which must also be referred to magnetic induction, and which I do not remember to have seen mentioned,—namely, the rotation which can be given to a mass of these metals of any form, by breaking and closing successively, the circuit of the electro-magnet. The following is a summary of the facts:—

If we place a globule, or a cube of sodium, copper, silver, &c., in any part of the magnetic field, with the exception of the vertical plane which divides the polar surfaces into two equal parts, there is always rotation, when we work the commutator from right to left and *vice versa*, so as to close and break the circuit, or change its direction. The rotation is not continuous; it is intermittent, like the ruptures which produce it. It always takes place from left to right, when the mass is by the operator whose right hand moves the commutator, and from right to left, when the mass is behind the medial vertical plane of the magnet. A succession of ruptures and closings of the circuit, without reversing the current, likewise produces rotation; but less powerfully than when the circuit is at once broken and reversed.

Under the influence of these successive actions, we can turn the large copper tube of Ruhmkorff's apparatus for several minutes, so as to twist the suspension wire sufficiently to cause the whole mass to turn rapidly the other way, when the electrical motion is stopped. This phenomenon of rotation is very complex, and it appears to me difficult, in the present state of science, to give a satisfactory geometrical explanation.

Electrical Conductibility of Sodium and Potassium.—Induction in metals appearing to be closely united to their electrical conductibility, we should be led to suppose that sodium and potassium, in which the phenomena of induction had manifested themselves in so evident a manner, would be good conductors of electricity. I endeavored to measure this conductivity.

Potassium and sodium were either run into thin rods in glass tubes of 1 or 2 millimetres in diameter, or drawn into wire. I have followed, in my observations, as nearly as possible, the mode described by MM. Pouillet and Ed. Becquerel, in their investigations as to the conductivity of metals. The sensitiveness of my apparatus enabled me to appreciate a resistance of 1 millimetre of a silver wire of 0.287 diameter. According to the numbers which I have obtained, sodium

would be placed, with respect to electrical conductivity, after the best conductors, silver, copper and gold, and before tin, zinc and iron. Potassium, which I have always found to be a worse conductor than sodium, should still come before iron. Various phenomena have proved that these metals are likewise very good conductors of heat.

Comptes Rendus, No. 14, October 6th, 1856.

PUBLIC HEALTH.

On the CHEMICAL COMPOSITION and NUTRITIVE EQUIVALENTS of the FOOD of MAN.—By M. POGGIALE.

(Concluded from Page 179.)

RICE.

RICE forming a part of siege provisions, and being introduced into military rations, I determined to estimate its value very carefully. Some authorities say that it is of small nutritive value; others consider it as very substantial nourishment, because in some countries, such as China and India, rice forms the principal food of man. But M. Boussingault has always found that it ordinarily replaces bread, is associated with meat, or boiled with milk.

It has been pretended that rice is the only aliment of the natives of the East Indies; but Doctor Lequerri, who has examined the alimentary regimen of the natives of Pondicherry, states that all the Indians eat *kari*, which is composed of meat, fish, or vegetables, mixed with rice. The chemical composition of this seed and my physiological experiments confirm the observations of M. Lequerri.

Piedmont rice contains—

Starch, dextrine and sugar	74.470
Nitrogenous matter	78.00
Fatty matters	0.235
Fixed matters	0.320
Lignine	3.445
Water	13.730

100.000

We thus find that rice is very rich in respiratory aliments, and that it contains but little nitrogenous, fatty, and saline matters. The carbonaceous principle far exceeding the fatty and albuminoid matters, animal alimentation cannot be complete with rice, except it is associated with aliments which are rich in plastic substances, such as meat, fish, milk, &c.

I made some practical comparative experiments on some powerful cocks in strong health, and as nearly as possible equal in every respect; some were fed exclusively on rice, the others on wheat, obtained the following results :—

1st. Experiment.—One cock, weighing 2 kil. 335 grammes, was fed for six days with 530 grammes of rice; it was weighed and found to have lost 163 grammes. While this bird was fed with rice, another received 530 grammes of hard wheat, containing the same quantity of water as the rice. After six days it had increased 7 grammes in weight, although it had fasted several times, the other bird eating his ration of rice slowly.

2nd Experiment.—The same cock which had been fed for six days on wheat, was now fed exclusively on rice, and in twelve days it lost 182 grammes. It had eaten 493 grammes of rice. The other cock which had been fed on rice, received in the same time 493 grammes of hard wheat; the diminution of weight was only 40 grammes, although it had received an insufficient quantity.

3rd Experiment.—Two cocks of the same size received as much as they liked for twelve days, one of rice, the other of hard wheat. Each day when they were fed we weighed what was left, so as to ascertain the real consumption. The first had lost 258 grammes; the weight of the second had augmented 358.

In other experiments, birds were fed comparatively with rice, barley, and soft wheat, and the results were nearly identical.

The result, therefore, of chemical analysis, physiological experiments, and practical observations is, that rice used alone, is not a substantial diet; and it must always be combined with nitrogenous aliments. It is evident that, in soldiers' rations, rice cannot be substituted for meat and dry vegetables. In fact, considering only the nitrogen, 550 grammes of rice would be necessary to replace 250 grammes of meat or 160 grammes of dry vegetables, such as lentils, peas, beans, or haricots. But this quantity of rice would contain so large a proportion of carbonaceous substances as would render this diet injurious to human health.

BARLEY.

Three analyses of barley (*hordeum vulgare*) gave the following results:—

Nitrogenous matters	.	.	.	.	.	10.655
Starch and dextrine	.	.	.	.	.	60.330
Fatty matters	.	.	.	.	.	2.384
Lignine	.	.	.	.	.	8.779
Mineral substances	.	.	.	.	.	2.623
Water	.	.	.	.	.	15.229

100.000

When the outer envelope of slightly moistened barley is carefully detached by the hand, we find that 100 parts of this grain give, on an average, 10 of envelope, which, like that of wheat, contains neither starch nor gluten, and which is, in great part, formed of lignine; still skilful experimentalists have only found in barley 25 per cent. of lignine and cellulose. But this error arises from the method of analysis employed, as I have remarked previously.

According to these experiments, barley contains, on an average, less nitrogen than wheat, and a larger proportion of fatty matters.

The nitrogenous matter which it contains is greatly in the state of albumen; whatever care may be taken in the preparation of barley flour, it never gives anything but a heavy and compact bread.

Some experiments made on animals prove that barley is less nutritious than wheat. Indeed, a cock weighing 2 kil. 635 gra., was fed for thirteen days with 1 kil. 300 grs. of barley, and lost 75 grammes of its weight. Another cock, weighing 2 kil. 434 grs., received during the same time 1 kil. 300 grs. of hard wheat. It had increased 37 grs. An inverse experiment on these two animals gave the same kind of result.

In a third comparative experiment, two cocks were fed for fifteen days, one with barley containing 10·855 per cent. of nitrogenous matter, the other with soft wheat, containing 13·250 per cent. of plastic principles. The first lost 23 grammes, the other increased 19 grammes in weight.

They had received the same amount of barley and soft wheat.

Nevertheless, barley forms a very important part of the food both of men and animals, especially in southern climates.

OATS.

Oats are composed, in 100 parts, of 78·5 of seed, and 26·5 of envelope. The latter contains no substance which would be nutritious for men, who only use the peeled seed. Some oats gathered in 1855, and peeled, gave on analysis:—

Nitrogenous matters	11·254
Starch and dextrine	61·850
Fatty matters	6·108
Lignine	3·460
Mineral substances	3·085
Water	14·243

100·000

Unpeeled oats contain 21·325 per cent. of lignine; 100 parts of envelopes contain 0·780 of nitrogenous matters, 2·87 of mineral substances, and 0·34 of fatty matters. This shows that the latter are poor in nitrogen, and fatty matters; we find in the seed, on the contrary, aromatic principles, a large proportion of nitrogenous substances, and especially of an oily, fatty matter. Oats are, consequently, very fattening for animals, and are fit for horses which, having to undergo a great deal of severe and regular labor, require a very substantial diet in small volume.

RYE.

Rye, likewise, forms a very important item in the nourishment of men; formerly the military bread was formed of equal proportions of rye and wheat. The soldiers' bread is still made of rye flour in some of the Northern States of Europe. Rye bread is brown, heavy and compact, with a peculiar odor; it is less nutritious than wheat bread, but remains longer fresh.

Rye gathered near Paris in 1855, contained :—

Nitrogenous matters	8.790
Starch and dextrine	65.538
Fatty matters	1.992
Lignine	6.388
Mineral substances	1.772
Water	15.530

100.000

This cereal contains less nitrogenous matter than barley, oats, and wheat; it differs from the latter chiefly in not giving gluten by washing.

MAIZE.

Some yellow maize collected in Corsica, in 1854, gave the following composition :—

Nitrogenous matters	9.905
Starch, dextrine, and sugar	64.535
Fatty Matters	6.680
Lignine and coloring matter	3.968
Mineral substances	1.440
Water	13.472

100.000

Other varieties gave nearly the same results.

Maize is distinguished from most other cereals by the large proportion of oily substances which it contains. Among natural productions there are few which so amply fulfil the requirements necessary for the nutrition of man.

It is an agreeable and substantial aliment, easy of digestion. It forms the chief nourishment of several American nations and the Danubian provinces; it is, likewise, used largely in Italy, Corsica, South of France, Spain, &c.

ALIMENTARY SEEDS OF LEGUMINOUS PLANTS.

The leguminous seeds which are used for human nourishment are haricots, peas, beans, lentils, grey peas, and lupins. The cotyledons of these seeds contain an abundance of a nitrogenous matter, analogous to caseine, and which Braconnot, who discovered it, called legumine. To obtain this substance the crushed seeds must be digested in water, they are then bruised in a mortar, water is added to the pulp thus formed, they are again macerated, the whole is thrown on a cloth and filtered. Starch is deposited, it is filtered again, and the legumine is precipitated by means of acetic acid diluted with water. Some chemists consider this principle as identical with the caseine of milk. According to M. Dumas, it is coagulated by heat, and when cold, by very dilute acetic acid, and is in great part dissolved by concentrated acetic acid.

The process indicated by M. Braconnot to isolate legumine cannot

be employed for the estimation of this product, especially as it appears probable that legumine is composed, like gluten, of several substances. But in a practical point of view this operation does not appear indispensable, because all the albuminoid principles of aliments present the same composition. It would, therefore, be sufficient to determine the nitrogen in the leguminous seeds, to have the proportion of nitrogenous matters which they contain.

Some observers have extracted from the leguminous seeds various principles, such as tannin, a very nauseous, bitter matter, a bitter extract, &c., which communicate a peculiar odor and taste to them; but these substances, the proportions of which are very small, are but of little importance to our present subject, the alimentation of man.

COMMON WHITE HARICOTS.

In 100 parts by weight, white haricots contain 7.5 of envelope and 92.50 of cotyledons, still retaining a second envelope, which is very thin and very adherent. The envelopes separated from the kernel contain only traces of starch, and on analysis we find but 0.2 per cent. of fatty matter, and 6.5 of nitrogenous matter, which is probably unassimilable.

The weight of the mineral substances is 5.815. It is well known that the envelope of haricots is refractory to the digestive action.

The following are the results of the analysis of the common white haricot:—

Nitrogenous matters	.	.	.	.	.	22.750
Starch, dextrine, and sugar	.	.	.	.	.	45.427
Fatty matters	.	.	.	.	.	2.750
Lignine	.	.	.	.	.	6.243
Mineral substances	.	.	.	.	.	3.560
Water	.	.	.	.	.	19.270

100.000

COMMON DRIED PEAS.

Two kinds of dried peas are commonly sold; one kind consists of whole peas, the other of split peeled green peas.

The whole peas are composed of 90.50 per cent. of cotyledons and 9.50 of envelopes containing neither starch nor legumine, and yielding to water 8 per cent. of their weight.

These peas when peeled gave on analysis—

Nitrogenous matters	.	.	.	.	.	21.670
Starch, dextrine, and sugar	.	.	.	.	.	57.650
Fatty matters	.	.	.	.	.	1.920
Lignine	.	.	.	.	.	3.218
Mineral substances	.	.	.	.	.	2.802
Water	.	.	.	.	.	12.740

100.000

The peeled peas contain a larger proportion of nitrogenous matters than the ripe peas. I have likewise observed that peas preserved by Masson's process contain more albuminoid substances than common

dried peas. Beans and scarlet runners have a higher alimentary power than the common white haricot, according to M. Payen's observations, and those which I have made myself. The following experiments on peas show the influence of maturity on the relative proportions of water and nitrogenous matters :—

			Water per cent.	Nitrogenous matter per cent. in dried peas.
1st Exp.	Very soft green peas	.	82.85	38.35
2nd "	idem	.	83.20	38.67
3rd "	idem	.	80.90	37.98
1st "	Green peas older than the above	.	76.14	34.17
2nd "	idem	.	75.20	34.48
3rd "	idem	.	75.36	34.46
1st "	Ripe green peas	.	70.62	27.72
2nd "	idem	.	70.49	27.43
3rd "	idem	.	70.80	27.21

Consequently the relative quantity of nitrogen diminishes in aliments as they attain maturity.

The following is the composition of beans, lentils, grey peas, and lupins :—

	Beans.	Lentils.	Grey peas.	Lupins.
Nitrogenous matters	24.210	29.055	21.775	38.350
Starch and dextrine	44.156	43.956	50.820	26.232
Fatty matters	1.418	1.484	5.320	7.854
Lignine	12.631	7.738	4.175	14.554
Saline substances	3.565	2.365	2.730	2.830
Water	14.020	15.402	15.180	10.180
	100.000	100.000	100.000	100.000

100 parts of beans contain 85 of cotyledons and 15 of envelopes, containing neither starch nor legumine.

Lentils are formed of 91.50 of cotyledons and 8.50 of envelopes, free from starch and legumine. According to my experiments, lentils are, of all leguminous seeds, the richest in nitrogenous aliments, except lupins, which contain 23.5 per cent. of envelopes, and which are distinguished by their very large proportion of nitrogenous matter, oily substances, and lignine.

Journal de Pharmacie et de Chimie, October, 1856.

CHEMICAL REPORT on the MODE of DETECTING VEGETABLE SUBSTANCES MIXED with COFFEE for the PURPOSE of ADULTERATION.

By Professor GRAHAM, Dr. STENHOUSE, & Dr. D. CAMPBELL.

(Concluded from page 113.)

6. Coffee and the various vegetable substances used in its adulteration, may be incinerated on a platinum or porcelain capsule, and leave an earthy ash, of which the composition is often characteristic of the plant. Valuable information may be obtained without making a formal analysis of the ash, by simply digesting it in strong hydro-

chloric acid. The earth which remains undissolved after this treatment is silica. Now coffee, we find, is remarkably distinguished from the roots and cereals, by the small quantity of silica it affords. The quantity of that earth found in coffee is so small, that it may be doubted whether coffee contains any silica, except what may accidentally adhere to the coffee beans when collected, in the form of sand. The proportion of silica found in the twelve samples of coffee of Table IV. was as follows:—

TABLE VII.
Silica in Roasted Coffee.

Sample No.	Per cent. in ash.	Sample No.	Per cent. in ash.
1.	0.	7.	0.
2.	0.	8.	0.45
3.	0.26	9.	0.
4.	0.2	10.	0.
5.	0.17	11.	0.
6.	0.28	12.	0.09

The only case in which the silica approaches to half per cent. of the ash is No. 8; and in another sample of the same coffee, which was properly screened before roasting, the silica of the ash fell to 0.

On the other hand the silica and sand, insoluble in acids, of four samples of roasted chicory amounted to so much as 10.69, 13.13, 30.71, and 35.85 per cent. of the ash. It may be added, that the portion of this silica soluble in alkali, was in the same samples 2.61, 8.81, 10.52, and 12.75 parts; the portion of silica insoluble in alkali 8.08, 9.32, 20.19, and 23.10 parts.

The whole silica in roasted dandelion root, amounted to 11.26 per cent. of the ash.

In other cultivated roots, the proportion of silica does not appear to be so large as in chicory. The silica is always expressed with reference to 100 parts of the ash. Messrs. Way and Ogston find in the root of the carrot, from 0.76 to 1.92 silica; in beet, from 1.4 to 4.11 silica; in turnip, from 0.96 to 2.75 silica.

The proportion of silica appears to be low in certain grains and seeds, although rarely descending to the insignificant proportion of the coffee bean, and to be very high in other grains; we find in the ashes of acorns, 1.01 per cent. of silica; in maize, 1.78 per cent. of silica; in the white lupin of the Levant, 0.87 per cent. of silica. Messrs. Ogston and Way report from 2.05 to 5.46 silica for wheat; from 23.6 to 70.77 silica for barley; from 38.48 to 50.03 for oats, and 9.22 for rye.

It appears, therefore, that the presence of 1 per cent. or upwards of silica in the ashes of coffee, is a proof of adulteration. That the adulterating substances which increase the proportion of silica most considerably are oats and barley, then chicory and dandelion, which are followed by rye; wheat, beet, turnip, and carrot would produce a small and less decisive effect.

There will now be presented complete analyses, made for this inquiry, of the ashes of seven varieties of coffee, and four different samples of chicory.

TABLE VIII.
Analysis of the Ashes of Coffee and Chicory.

[illegible]

The first difference which appears upon comparing the two sets of analyses, is the absence of soda in coffee, and its presence in chicory, to the extent of from 2.04 to 15.1 per cent. of the ash. The united amount of potash and soda does not differ much in the two substances. In chicory the lime is greater, and the magnesia less than in coffee. The sesquioxide of iron is strikingly different, being always under 1 per cent. in coffee, by our experiments, and ranging from 3.13 to 5.32 per cent. in chicory. The ash of chicory is on this account, red to the eye, when compared with that of coffee. The difference in chlorine is also important, the highest proportion observed in coffee ash being 1.11 per cent., and the lowest proportion in chicory ash 3.28 per cent.. Coffee gives an ash which is highly carbonated, the carbonic acid varying from 14.92 to 18.13 per cent.; while the ash of chicory is only slightly carbonated, containing from 1.78 to 3.19 per cent. of carbonic acid. The proportion of phosphoric acid is pretty similar in the two kinds of ash. The disparity in the silica has been already referred to.

The differences most available in the two kinds of ash as distinctive tests for coffee and chicory, appears to be the following:—

	In Coffee Ash.	In chicory Ash.
Silica and sand	0	10.69 to 35.85
Carbonic acid	14.92 to 18.13	1.78 to 3.19
Sesquioxide of iron	0.44 to 0.98	3.13 to 5.32
Chlorine	0.26 to 1.11	3.28 to 4.93

Another series of ash analyses was executed, comprising the ash of the white lupin of the Levant, a seed which from its chemical properties and low price, is not unlikely to be substituted for coffee, the ash of acorns, of maize, and of parsnip, and dandelion roots.

TABLE X.
Analysis of Ashes of certain Seeds and Roots.

	Deducting Silica, &c.					Not deducting Silica, &c.				
	Lupina.	Acorna.	Maize.	Parsnip.	Dandelion root.	Lupina.	Acorna.	Maize.	Parsnip.	Dandelion root.
Potash	38.83	55.49	31.28	56.86	20.22	33.54	54.93	30.74	56.54	17.96
Soda	17.90	.63	"	"	34.87	17.75	.63	"	"	30.95
Lime	7.61	6.98	3.11	6.88	12.67	7.75	6.01	3.06	6.85	11.43
Magnesia	6.28	4.36	14.98	6.52	1.47	6.18	4.32	14.73	6.49	1.31
Sesquioxide of iron	"	.54	.85	.58	1.42	"	.54	.84	.53	1.27
Chlorine	2.12	2.53	.50	2.10	4.32	2.11	2.51	.50	2.09	3.84
Sulphuric acid	6.85	4.83	4.20	4.09	2.66	6.60	4.79	4.13	4.07	2.37
Carbonic acid	.56	13.82	"	11.50	6.95	.56	13.69	"	11.44	6.21
Phosphoric acid	25.74	11.26	45.29	13.91	13.63	25.53	11.15	44.50	13.84	11.21
Silica, &c.	"	"	"	"	"	.87	1.01	1.73	.57	11.26
	101.04	100.44	100.21	102.89	97.45	101.09	99.58	100.27	102.43	97.80

In the ash of the dandelion root only, is the silica sufficiently large in quantity to make a good distinction from coffee. Lupins and maize are distinguished from coffee by their low proportion of carbonic acid.

The white lupin, both of the Levant and England, we find to contain a notable quantity of manganese. The oxide of iron is every where too low to be serviceable. Chlorine appears as low in maize as in coffee; but the proportion of that element is doubled in lupins, acorns, parsnips, and nearly quadrupled in dandelion; a high proportion of phosphoric acid distinguishes maize, and, we may add, all the other cereals from coffee.

The ash of the turnip root contains, according to Way and Ogston, from 9.54 to 14.82 per cent. of carbonic acid, which is very different from chicory, and nearly as high as in coffee. The sesquioxide of iron of turnip is also given, even lower than in coffee—from 0.14 to 0.66 per cent.—chlorine appears to be as abundant in the turnip, in the form of chloride of sodium, as in chicory root.

According to the same excellent authorities, in beet-root ash the carbonic acid varies from 15.23 to 21.61 per cent., and is therefore quite as high as in coffee ash. The sesquioxide of iron, from 0.52 to 0.74 per cent.; small as in coffee. The chlorine very large and distinctive, being represented by from 4.91 to 7.65 per cent. of chloride of sodium (Way and Ogston).

7. The action of the more ordinary chemical reagents upon infusions of coffee and chicory may now be noted. The indications thus obtained with coffee, are unfortunately rendered much less characteristic by the torrefaction of the seed.

(To be continued).

INSTRUCTION RESPECTING ERGOTISED RYE. By M. PATER.

THE disease of the grains which is known by the name of ergot, is evidently due to a cryptogamic production. It usually affects rye and maize in certain localities, and in seasons which are at once hot and moist; it is also found in wheat.

The consumption of the diseased grains may occasion serious injury both to men and animals.

It would however be very unfortunate, if the good grains found in large proportion among affected rye, maize and wheat, should be lost as alimentary substances.

The following are the characters by which the existence of ergot may be ascertained, the casualties which it may occasion, and the means whereby it may be removed from the grains.

CHARACTERS OF ERGOT.

The ears affected with ergot are easily distinguishable; many of the grains in it are replaced by a violet-brown substance, almost black, of larger volume, longer and frequently twisted, brittle, having a grey mass inside. The ergot may be distinguished even when no larger than the healthy grain, or when broken into several pieces, not only

by its external dark brown color, but also by its lightness; it floats on water, whereas the healthy grains sink to the bottom.

EFFECTS OF ERGOT IN FOOD.

The injurious and deleterious effect of ergot is, of course, more dangerous in proportion to its quantity; one-eighth to one-tenth per cent. in the bread occasionally causes serious consequences, inducing gangrene and loss of the limbs.

The poisonous action of the ergotised grains is more powerful on animals than on man.

Serious and rapidly fatal consequences manifest themselves when grains mixed with ergot, removed in cleansing grain, are given to animals. Gangrene is produced the same as on man, but more rapidly and with more extraordinary circumstances.

In poultry, the phalanges are destroyed and fall off, even the beak is detached; in pigs the nails fall off, and the animal perishes.

Consequently, we cannot take too many precautions to prevent the employment of ergotised grains, and their products, flour, bread and bran in the food of either man or animal.

PRECAUTIONS TO BE TAKEN TO AVOID THE DANGERS WHICH THE USE OF ERGOTISED GRAINS PRODUCES.

These dangers may be avoided by properly cleansing the grain affected by this malady.

It is not difficult, and would often not be too expensive, to cleanse the corn with the hand by passing over a table, as used for corn to sow, and thus remove any ergot which it might contain.

Carefully sifting with a sieve pierced with holes, that would permit the passage of the good grains and retain the ergot by reason of its greater volume; any which might escape could be moved by fanning; the ergot being blown away in consequence of its greater lightness, whereas the good grains would remain.

Before grinding, a careful cleansing of the corn by powerful ventilation, would eliminate the ergot and its debris, in consequence of their lightness. These various methods are not expensive, they would indeed produce some profit; for the ergot thus extracted may be sold for medicinal purposes, from 1.50 franc the kilo. to 5 francs, according to the quantity produced; always, as is evident, higher than the price of wheat and rye.

Journal de Chimie Médicale, December, 1856.

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS

On FUMIGATIONS of ESSENCE of TURPENTINE in ITCH.

By Dr. A. ANSELMIER.

TREATMENTS for itch are numerous, and we must acknowledge that they are all more or less successful, more or less easy of application

and, finally, more or less expensive; the last point being an important consideration in medicines for the poor.

I have tried several this year, and I do not hesitate to give the preference to the one which is probably the least known, that invented by M. Aubé; I speak of the treatment of itch by essence of turpentine in lotions over the whole body, and in fumigations. This treatment is first rate for rapidity, efficacy, simplicity and low price. The following is the method which I have successfully used.

The patient, on going to bed, sprinkles on the sheets and his usual daily clothes, about 50 grammes of essence of turpentine. When he wakes he is cured; his bed and his clothes are no longer infected. The odor of the turpentine passes in two or three days.

This treatment has several advantages; 1st, it attacks the parasites at the time that they are most accessible; 2nd, fumigation acting by substitution on secondary eruptions, are much less irritating than lotions and frictions, whether soapy, sulphurous or terebinthinated; 3rd, the treatment acts at the same time on all the contaminated objects; 4th, not only is it more rapidly efficacious and better than any other, it is likewise the cheapest.

Journal de Chimie Médicale, December, 1856.

On the ADVANTAGE of USING HONEY as an EXCIPIENT in PILLULAR MASSES. By M. C. J. THIRAULT.

THE number of officinal pills prepared in old times, has much diminished in our time. Among those once in general use few are now found in our establishments, for medical men seldom order them.

My intention is by no means to endeavor to re-establish these ancient panaceas, but I wish to trace the cause of the desuetude of many whose action must be considered indubitable, and I think that I have found it in the bad method in which they have been prepared. Freshly made pills are soft and can be dissolved by the juices of the stomach, and produce the action which should be expected from their ingredients; but they do not retain these properties, which last indeed but a very short time; a month, and far more, a year after, they will be found inactive. Whence does this proceed? It probably depends entirely on the fact, that these pills can no longer be dissolved in the stomach, and that they pass through the intestinal canal and into the dejections exactly in the same state in which they were taken.

Let us take pills of cynoglossum, for example, and we shall find that it is as I have said. Soon after their preparation these pills become so hard, that it is easy to imagine that they would resist the solvent action of the stomach; for this reason some medical men consider them valuable, and others think them useless, a contradiction of opinion depending solely on the time which has elapsed since the preparation of the pills.

With honey all these inconveniences disappear. Pills made with it retain indefinitely the qualities which they should have to dissolve in the stomach, for they remain always soft. I prepared some pills of cynoglossum two years ago, using honey as the excipient, and they are now in the same state in which they were when made, namely, quite soft.

Journal de Chimie Médicale, December, 1856.

IODURETTED GLYCERINE in SKIN DISEASES.

THIS preparation is recommended by Dr. Gage, of New Hampshire, and is made by dissolving one part of iodide of potassium in two parts of glycerine, and turning this liquid upon one part of iodine, which is thus completely dissolved. This solution has the advantage over alcoholic solutions of not drying. By this means the surfaces to which it is applied remain supple, and the action and absorption of the iodine remain for a long time. In employing it, the diseased parts to which the solution has been applied, are covered by paper of gutta percha, to prevent evaporation of the iodine.

IODINE in VOMITING in PREGNANCY. By DR. EULENBERG.

DR. J. B. SCHMITT some time since recommended the tincture of iodine in the vomiting of pregnant women, when this may be regarded as a neurosis. He gives one case in particular, in which a woman, who had, in consequence of vomiting, had four abortions, and became pregnant with her fifth child. Two drops of the tincture given every two hours completely kept the vomiting under, as long as it was continued, the vomiting only returning when it was suspended. At last, a dose two or three times daily sufficed. Dr. Eulenberg has also met with a remarkable case in a healthy woman, 35 years of age, who had suffered from sickness in every pregnancy, and on this occasion had become much reduced. As she was very susceptible to the action of medicines, he ordered her—tinct. iod. ℥j., sp. vini r. ℥iij., three drops to be taken in water every three hours. By the second day all the most distressing vomiting had disappeared, and she was again able to keep water on her stomach, and in a short time she quite recovered. The *modus operandi* of such small doses of the tincture is not very easily explained; but it is to be observed, that the iodide of potassium exerts scarcely any influence. It is highly probable that it only influences the vomiting, inasmuch as this is a neurosis, an abnormal condition of the nerves of the stomach. In proof of this, it is also administered with the greatest advantage to hysterical women suffering from abnormal sensations at the epigastrium as an insupportable feeling of emptiness, or a constant feeling of hunger without appetite. The iodine

also acts beneficially in the sympathetic nervous irradiations, even when not connected with pregnancy, but which may depend on suckling, &c. The cardialgia and nausea of pregnancy are speedily relieved by it, as well as the vomiting.

Berlin Medicin Zeitung, No. 25.

GLYCERINE; *Prompt CURE of chronic ECZEMA by means of, in UNION with the SOOT of WOOD.*

To the facts which prove the efficacy of glycerine as a medicament, we add the following, published by M. Bougard:—A young man, about thirty years of age, had been for several years afflicted by a chronic eczema, which attacked almost the entire body; but which appeared with great intensity on the legs and feet. These parts were the seat of intolerable itching; the skin was red, excoriated, and covered with slight crusts. Various remedies had been tried without success. M. Bougard tried the effect of wood soot, which he had occasionally found succeed in similar cases; the use of a decoction applied as a lotion, and of a pomade seconded by the action of two or three purgatives in the week, produced some amelioration, but the treatment then came to a stand. Thinking that wood soot would act better if mixed with wax, he ordered it in this form; no improvement followed. The continued purgatives had re-established the digestive functions; the patient's strength had gradually improved, but the disease remained in the same state. At length, after four months' trial of this treatment, M. Bougard commenced the use of glycerine, which produced a slight amendment. He had next recourse to a mixture of the two, glycerine and wood soot in equal weights. This he tells us had an astonishing effect. After fifteen days of its use, the eczema was almost cured, and the cure proved medical. The author has since made use of this mixture in the treatment of chronic eczema round the ears and in other localities, and has obtained very advantageous results.

Journ. de Med. de Bruxelles, Sept., 1856.

On LIQUIDAMBAR STYRACIFLUA.

By CHARLES W. WRIGHT, M.D., Professor of Chemistry in the Kentucky School of Medicine.

LIQUIDAMBAR STYRACIFLUA, commonly called *sweet-gum*, is indigenous to nearly every part of the United States, and constitutes one of our largest forest trees. When an incision is made through the bark of this tree, a resinous juice exudes, which possesses an agreeable balsamic odor. When this substance first exudes, it is of the consistence

of turpentine, and possesses a stronger smell in that condition than it does after it has become resinified. Contrary to the statements made by Wood and Bache in their *Dispensatory*, this tree furnishes a considerable quantity of resin in the Middle States, particularly in the States of Ohio, Indiana, and Kentucky, bordering on the Ohio River. It is annually collected in those States, and sold under the name of *gum-wax*. It is a much more agreeable masticatory than the spruce-gum, and is chewed in the West by nearly all classes. By proper incisions, one tree will yield annually about three pounds of the resin.

The chemical composition of the specimens collected in this latitude correspond with that given by M. Bonastre, of specimens gathered elsewhere, viz.:—benzoic acid, a volatile oil, a semi-concrete substance separated by distillation and ether, an oleo-resin, a principle insoluble in water and cold alcohol, termed *styracine*. The bark of the tree contains tannic and gallic acids, to which its astringency is due.

What I wish more particularly to call attention to is the employment of a syrup of the bark of this tree, in diarrhoea and dysentery, and more especially the diarrhoea which is so prevalent among children during the summer months in the Middle States, and which frequently terminates in cholera infantum.

The best formula for the preparation of this syrup is that given in the *United States Pharmacopœia*, for the preparation of the syrup of wild-cherry bark, of which the following is a copy, the sweet-gum bark being substituted for the wild-cherry bark:—

“Take of sweet-gum bark, in coarse powder, five ounces; sugar (refined), two pounds; water, a sufficient quantity. Moisten the bark thoroughly with water, let it stand for twenty-four hours in a close vessel, then transfer it to a percolator, and pour water upon it gradually until a pint of filtered liquor is obtained. To this add the sugar in a bottle, and agitate occasionally until it is dissolved.”

The dose of this syrup for an adult is about one fluid ounce, to be given at every operation, as long as the operations continue to recur too frequently.

One advantage which this medicine possesses over most astringent preparations, is that of having an exceedingly pleasant taste, and of being retained by an irritable stomach when almost every other substance is rejected. Children never object to it on the score of bad taste. The resinous and volatile bodies which it contains no doubt enhance its value. My brother, Dr. J. F. Wright, of Columbus, Indiana, has employed this preparation for the past three years in a great number of cases, with the most satisfactory results. He prefers it to any other article where there is an indication for astringent medication in the class of diseases before referred to. In the bowel complaints of children it has a decided advantage over all preparations containing opium, and I am always pleased with the happy results which follow its employment in that class of patients.

American Journal of Medical Sciences, July, 1856.

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MEMOIR of JOHN DALTON, and HISTORY of the ATOMIC THEORY up to HIS TIME. By ROBERT ANGUS SMITH, Ph. D., F.C.S., &c. London: H. Baillière. 1856.

DR. SMITH's object in the present work, has been to give a conjoint history of Dalton and the atomic theory, up to the time at which he published that theory, with which his name is now so intimately associated, and in connection with which it will probably go down to the latest posterity.

In the preface, the author points out as the distinct feature of his work, the history which it contains of "our ideas of matter bearing on modern chemistry, until the time when Dalton flourished." The first three chapters treat of Dalton's early life and labors; the general history is commenced in the fourth and continued to the tenth; in the eleventh, Dalton again appears on the stage, and his views on the great theory are given succinctly and with much care. The twelfth and thirteenth chapters are devoted to the later life and intellectual character of Dalton; the fourteenth (and last) consists of an arrangement of theories. The work concludes with a very useful index.

There have been other memoirs of Dr. Dalton, but this one, from the distinctive feature above indicated, has a peculiar attraction, and we recommend it to the perusal of every person engaged in chemical pursuits. The work is agreeably written, and is in all respects, worthy of a place in every library, which its moderate price will help to secure for it.

PROCEEDINGS OF SOCIETIES.

PHOTOGRAPHIC SOIREE at KING'S COLLEGE.

A SOIREE was held at King's College, Somerset House, on Wednesday, the 17th of December, 1856, under the auspices of the President and Council of the Photographic Society, with a view to the exhibition of a large and highly interesting collection of photographs, daguerreotypes, and scientific and chemical apparatus connected with the art. The large hall was selected for the purpose; the museum and library being also thrown open, the latter as a refreshment-room. Upwards of a thousand visitors were present, including Professor Wheatstone, Mr. Le Neve Foster, Mr. Barlow, and Dr. Booth of the Society of Arts, Dr. Livingston, the celebrated African traveller, whose hardships, privations, and heroic efforts have become so familiar to the public; Dr. Carpenter, Mr. Fenton, whose Crimean photographs have been so deservedly admired; and other gentlemen connected with science and the arts. The largest and most varied among individual collections was that of Bisson, Frères, of Paris, from the Crystal Palace

Exhibition, consisting of specimens of architecture, landscapes, copies after Rembrandt, Glacier Scenery from the Swiss Alps, &c., many of extraordinary size and exquisite manipulation. Similar specimens by Baldus, though fewer in number, were by no means inferior in execution, especially an Amphitheatre at Arles. Some cases of small untouched specimens by Mr. F. R. Williams, exhibited great merit and minuteness of detail, and a small collection of colored portraits by Lock and Whitfield, possessed a softness of tone and a delicacy and finish in the coloring peculiarly their own. Two cases of admirably executed portraits of living celebrities by Messrs. Mayall, including several members of the Cabinet, the Bishops of Oxford and Ripon, &c., were examined with great interest. A number of spirited portraits of Crimean heroes, bearded and stern, were exhibited by Mr. Cundall, and several similar subjects of equal merit by Mr. Howlett. Perhaps the most attractive feature of the evening, if numbers may be considered a test, was a series of photographic Views of the Moon, in various phases, by Mr. Bond of Cambridge, U. S., the same gentleman, we believe, who succeeded in the resolution of the great nebula in Orion, and the double-headed or "dumb-bell nebula," with the aid of the large refracting telescope of the Cambridge, U. S., Observatory, and a Single View of the Full Moon, taken at Liverpool by Mr. Crookes. These, as may be imagined, were "metal most attractive" to the scientific portion of the company, and scarcely less so to the general visitors. Stereoscopes and stereoscopic views abounded; those of Messrs. Murray and Leigh were remarkably fine, especially a set of snow pieces. The cosmorama stereoscope also, possesses a great superiority over the ordinary instrument, both in the quickness of its adaptation to the eye, and the absence of confusion in the objects. A large stereoscope on a stand, with revolving views of "Douro Scenery" by Baron de Forrester, was also well worthy of attention. A most interesting series of beautiful engravings was exhibited, the subjects taken by photography, and engraven on copper by voltaic electricity. This elegant process, combining as it does the minuteness and accuracy of a daguerreotype, with the finish of an engraving, seems likely to add greatly to the resources and popularisation of art. The shew of chemicals was small, consisting chiefly of some fine specimens of nitrate of silver, hyposulphite of soda and chloride of gold, from Messrs. Horne and Thornthwaite. Altogether the entertainment was of a highly interesting nature, and there can be no doubt that such reunions, by bringing under comparison the best productions of the best artists, must tend greatly to advance the science of sun-painting.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

*On the SEPARATION of the FIBRE of the FLAX PLANT for TEXTILE PURPOSES.** By JOHN F. HODGES, M.D., F.C.S.,

PROFESSOR OF AGRICULTURE IN THE QUEEN'S UNIVERSITY IN IRELAND; CHEMIST TO THE CHEMICO-AGRICULTURAL SOCIETY OF ULSTER; FOREIGN MEMBER OF THE ACADEMY OF AGRICULTURE OF SWEDEN, &c.

THE great extension of the cultivation of the flax plant in Ireland, and the importance of substituting for the rude and uncertain methods of preparing the fibre,—which from the most remote antiquity have prevailed, both in this country and on the continent of Europe—some more satisfactory and economical process, has for a number of years occupied the anxious attention of manufacturers.

An examination of the stem of the flax plant, especially after it has been softened by steeping in water, readily enables us to discover that it is composed of three parts,—viz., externally, a delicate investing membrane, the *epidermis*, under which is a layer composed of longitudinal fibres, capable, by mechanical means, of being split up into filaments of extreme delicacy, while the central portion of the stem consists of vascular tissue destitute of tenacity.

The longitudinal fibres composing the middle layer, and which, under the microscope, are recognised by the vegetable physiologist to consist of long tubular cells, with thick tenacious walls, forming what is termed *bast*, give to the stem of the flax plant its great commercial value; and the separation of these, in the most perfect manner, and with the least expenditure of time and labor, has been the subject of numerous investigations, both in the British Islands and in France and Germany. It is sufficiently remarkable that the composition of the stem of the flax plant has been almost entirely overlooked in the various attempts to resolve this important problem. To contribute in some degree to a more accurate knowledge of the materials upon which the flax steeper requires to operate, the author has for some time been engaged in the study of the proximate constituents of the plant. From these investigations it appears that the cementing materials by which the various structures of the stem are bound together are a substance belonging to the pectin series of compounds, albumen and a dark-green exceedingly adhesive resin, while the *bast* fibres probably owe much of their spinning and other valuable qualities, which our manufacturers express by the term “nature,” to the presence of a considerable amount of wax and fatty matter, which remain connected with the fibre, even after long immersion in water.

* Communicated by the Author.

The numerous processes which have been tried and patented in various countries for the separation of the textile fibre of flax may be classed under two divisions—viz., processes in which it has been attempted to effect the removal of the investing epidermis and adherent woody matter by mechanical means, without steeping, and those in which the action either of fermentation or of chemical solvents (dilute acid and alkaline solutions, soap, &c.) was employed for the removal of the adhesive substances of the straw. The dry process, though at one time warmly advocated and sanctioned by parliamentary grants to the proposer, was never to any extent adopted in this country, as the material produced from the imperfect removal of the foreign matters of the envelope and wood was found only suitable to the manufacture of inferior coarse fabrics. There exists at present only one locality in Ireland where it is to be found in operation. What may be termed the chemical processes (fermentation, &c.,) have, with numerous modifications, been introduced into every flax-growing district in Europe; and in the rude forms of "dew-retting," by the irregular and unmanageable fermentation induced by the exposure of the flax in heaps in the fields, and by steeping in water, in pits excavated in the farms of the cultivator, have existed from the most remote antiquity. In Ireland, where from the earliest times the growth and manufacture of flax have occupied the people, the fermentation of the straw in shallow pools was, until a few years ago, the only plan practised. But the inconvenience and delay of this method in our changeable climate was found materially to interfere with the production of a supply of fibre adequate to the yearly increasing requirements of our manufacturers, especially in districts in which the farmers were previously ignorant of the proper management of the crop. Fortunately for our manufacturers, in 1846, a method of treating flax, patented by an American named Schenck, led to the substitution of a method which appeared likely to relieve the flax-grower from the difficulties of the old process, and to transfer the technical preparation of the crop to the establishments of the manufacturer, and thus enable the farmer to confine his attention to his proper duty—the production of the raw material. So fully satisfied were both spinners and landed proprietors of the great advantages of the method introduced by Mr. Schenck, that, in a very short time, numerous steeping establishments were erected in this country; and in France and several Continental States his improvements were eagerly adopted. The process of Schenck is now so well known, that a brief description of the methods adopted in the works conducted on his system will be sufficient. The flax, previously deprived of the bolls or seed capsules, is placed in wooden vats or tanks constructed of brick, coated with Roman cement, and provided with false bottoms, perforated with holes. The vats are filled with water, and by means of a steam pipe which passes under the false bottoms, the temperature of the water is raised to 90° F., and maintained at this temperature about 60 hours. At the end of this period, the steeping is generally completed, and the straw removed from the vats and dried, either by exposure to the air in drying sheds, or, when

the weather is unfavorable, in chambers heated by steam. Such is an outline of the process as originally proposed by the patentee, and after his death, carried on by his successors, Messrs. Bernard and Roch.

As many erroneous opinions appeared to be entertained, even by scientific men who had only partially examined the subject of the nature of the new method, I was induced to make a series of investigations connected with it, the results of which were in part communicated at the Belfast Meeting of the British Association. These experiments were conducted chiefly at a large rettery near Belfast, and afford some useful information respecting the nature of the process and the precaution requisite for its successful management.

When the straw of the flax plant is placed in water gradually heated by pipes through which steam passes, so as to produce a temperature of 86° or 90° F. the water gradually acquires the color of pale ale, and as the process advances, becomes darker in color and somewhat muddy. In about eight hours a copious evolution of gas takes place, and a shiny matter collects on the surface of the liquid. The liquid at this period is found to be strongly acid, but the red color of the litmus paper almost completely disappears in drying. When the flax is removed from the vats, in which the temperature has been steadily preserved at 90°, after steeping about 60 hours, the bast fibres can be readily separated from the worthless woody matters and epidermis, and will be found uninjured; but if allowed to remain in the fermenting liquid for a longer period, a new series of decompositions ensues, tending to decompose the fibre itself, and to destroy its tenacity. The fermentation, when properly conducted, I have found, from numerous analyses, to resemble the so-called butyric acid fermentation. The investigation of the gaseous products of the fermentation was conducted by filling wide jars with the warm water of the vats, and supporting them over the surface of the fermenting liquid; in this way I was enabled to collect the gases and to convey them to Queen's College, where, without delay, their quantitative examination was performed by the methods proposed by Bunsen for the analysis of gaseous mixtures. These analyses proved that the gaseous mixture consisted solely of carbonic acid, hydrogen, and nitrogen; no traces of carburetted hydrogen, carbonic acid, or sulphuretted hydrogen were detected. The evolution of sulphuretted hydrogen in Schenck's process having been asserted by some French chemists, and regarded as an objection to its employment, from the insufficient evidence afforded by analyses of partially decomposed steep water brought from Lille to Paris, special means were taken to ascertain whether that gas was produced during the fermentation, but strips of lead paper suspended in jars over the steep water were not at any period discolored. Schenck's process may therefore be regarded as merely the ordinary fermentation of the steeping pool accelerated, and rendered subject to scientific control.

In the *Annalen der Chemie* for September 1852, Professor Dr. C. Schmidt of Dorpat, has published some interesting analyses of the

waters employed in steeping flax in the neighborhood of that city, which are useful contributions to the statics of flax cultivation in Russia. Such investigations however, have not, as Dr. Schmidt supposes, been overlooked by other chemists. In Ireland, several analyses, both of the waters of the ordinary flax pools, and of the warm water reterries have been published by the author. The results of these investigations confirm the accuracy of the opinion expressed by the author in communications to the Chemico-Agricultural Society of Ulster, that the idea which had been at one time entertained of the value of flax water as a manure was incorrect, and that in the present condition to which the science of agriculture has been raised, mainly by the labors of Baron von Liebig, it would be going backwards to advise the farmer to expend money in the effort to fertilise his fields by the application of these liquids, when all the valuable ingredients contained in them could be obtained in a concentrated portable form at the cost of a few shillings.*

Like all innovations on ancient processes, the method of steeping introduced by Schenck was received with much hesitation in this country, and the most contradictory statements respecting the quality of the fibre produced were circulated; thus, in some spinning establishments it was asserted that it gave a strong and harsh fibre, while in others its fineness was admitted, but its strength was stated to be inferior to that obtained by the common method. Additional confidence was for some time given to purchasers, by experimental trials of both methods, which were made by some of our most experienced spinners. In one of these trials it was shewn that there was a gain of twenty per cent. of fibre on the flax prepared on Schenck's system, and that with respect to quality, two samples of hot steeped flax spun respectively to 70 and 101 leas, while two samples of the same parcel of flax cold steeped spun to only 60 and 96 leas.

In several of the Irish steeping establishments, however, the result failed to realise the confident expectations which were excited by the earlier trials, and during 1851 and 1852 so much dissatisfaction was excited, both with respect to the quality of the fibre and the cost of production, that in some of the works the process was abandoned. In the present year the original plan of Schenck has been considerably modified, and the improvements which have been introduced in connexion with it, which will be hereafter described, now enable our

* The following artificial manure, recommended by the author, has been applied with success by the Irish flax-growers:

		£	s.	d.
Chloride of potassium	44lbs. at 12s. per cwt.	0	4	8
Common salt	27 " " 1 "	0	0	3
Sulphate of magnesia	35 " " 8 "	0	2	6
Ground bones	88 " " 6/6 "	0	4	10
Sulphate of ammonia	112 " " 17 "	0	17	0
		1	9	3

The quantities given are sufficient for manuring a statute acre.

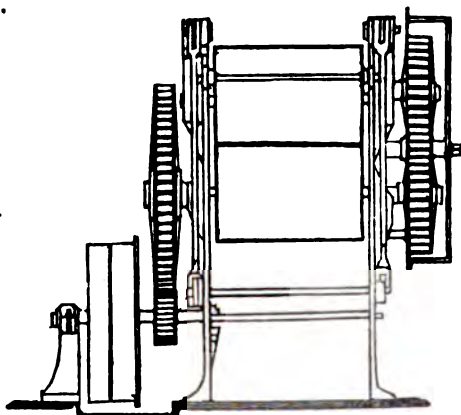
steepers to produce fibre of excellent quality, commanding the highest price from the spinner.

The interest which was created by the partial success of the earlier trials of the American system, and the desire of rendering the production of the fibre entirely a factory operation, led to several inventions directed to the same purpose; of these, only the method of Mr. Watt, of Glasgow, which was introduced to the public at the Belfast Meeting of the British Association, has been adopted in this country.

The process patented by Mr. Watt, is entirely different from the methods hitherto in use, the separation of the textile fibre being effected neither by fermentation nor by the solvent action of chemical agents, but by exposing the straw in a chamber of peculiar construction to the action of steam and condensed water, and subsequently submitting it, in the softened state in which it is taken from the chamber, to the pressure of heavy rollers.

The first operation in Watt's method consists in placing flax-straw deprived of the bolls by the action of the rollers, for some time in use in the hot water steeping establishments, but without the root ends being cut off, as usual in these works, in chambers formed of cast iron plates. Each chamber measures about 12 feet in length, and is 3 feet broad and 6 feet in height, and contains from 18 to 20 cwt. of seeded flax. At each end of the chamber is a door, through which the straw can be introduced, and which can be closely secured by screws. A false bottom, formed of perforated iron plates, such as are used in malt-kilns, is raised about 6 inches from the bottom of the chamber, and under this bottom passes a horizontal pipe, which conveys steam from the engine. Outside, where this pipe enters, there is a stop-cock for regulating the supply of steam, and attached to the side of the chamber, a water gauge shows the quantity of liquid collected in it. The top of each chamber is constructed so as to form an open tank, about 18 inches deep, for containing cold water, and the plates of which this tank is composed on the lower side, or that which forms the roof of the chamber next the flax, present a corrugated surface, the object of which will be presently described. The steam chambers being filled with flax, and the doors secured by screws, steam is admitted, which, passing through the mass of straw and coming into contact with the water tank forming the roof of the chamber, is condensed, and made to descend in showers of distilled water, the corrugated surface of the plates causing it to descend in drops. By this arrangement, therefore, the softened straw is exposed to the action of distilled water, by which its soluble matters are dissolved and washed into the lower part of the chamber. In districts where the only water accessible to the steeper (as is frequently the case) is so strongly impregnated with lime as to be rendered unfit for steeping by the ordinary way, or by Schenck's method, this new process may be advantageously applied. From time to time, the hot liquid which accumulates at the bottom of the chamber, is pumped into a small reservoir placed on the top of the tank, and discharged over the straw. In from 12 to 18 hours the steaming process is completed, and the straw, when with-

drawn from the chamber, is immediately subjected to the successive pressure of two or more pairs of heavy iron rollers, each about 16 inches in diameter and 27 inches long, and somewhat resembling seeding rollers.



SECTION OF WATT'S ROLLERS.

Between these rollers, the straw, collected in small bundles, is passed three or four times, by which means it is deprived of nearly all the moisture which it contains, and also of much of the nitrogenous and other matters which have been softened, but not dissolved out by the treatment with water.

An interesting feature in this new process, is the means which it affords of economising the

matters which are dissolved from the flax plant. In the ordinary methods of treating, the substances extracted by the steeping process undergo decomposition, and the liquid acquires an offensive odor; but in Watt's process there is no alteration effected in their properties; the dark liquid which accumulates in the lower part of the chambers is merely a strong decoction of the plant. Neither its taste nor its smell is disagreeable, and it has been used, mixed with other food, with advantage in feeding pigs. A gallon of this liquid, obtained from the works of Messrs. Watt and Leadbetter in Belfast, was found to contain, of—

Organic matters	353.97 grains
Earthy and saline matters	161.49 „

Giving a total amount of 515.46 grains of solid matters, which afforded when burned with soda lime, 14.79 grains of nitrogen, which may be regarded as representing 110 grains of substances possessing a composition similar to the nutritious nitrogenous compounds of our ordinary crops.

Watt's method has excited great interest in the flax-growing districts in Ireland, and a number of establishments have been organised for the purpose of applying it, in this country, in England, and in Scotland. I find, however, that among practical men, there exists considerable doubt as to the quality of the fibre which has been produced in these new works; and even those who admit that it exhibits a superior appearance complain that, in its after management, —in spinning and boiling—considerable difficulties are found, owing to the only partial and incomplete removal of the cementing matters of the straw.

As already mentioned, the hot water system of Schenck failed in satisfying the expectations which were excited on its introduction. It was found that the fibre prepared by the American system, like that treated in Watt's chambers, remained impregnated with a portion of the gum and other cementing matters, which obstinately adhered to the fibres when dried, binding them together so as to occasion considerable loss in hackling, and otherwise lessening the value of the material. These objections to Schenck's process have however lately been removed by allowing a stream of water to run into the fermenting vats, at the conclusion of the process, so as to float away the scum which collects on their surface and to wash the fibre, and by the employment of heavy iron rollers, as used in Watt's process, to press out the matters which adhere to the straw, or are confined within its cells on its removal from the vats. The application of mechanical pressure has also been rendered more effective in the purification of the fibre, by a stream of water being made to fall upon the flax while passing through the second pair of rollers. Experiments conducted on a considerable scale in several establishments, have shewn that, by adding to the dissolving action of fermentation at 90° F., the use of Watt's rollers, we obtain a most valuable means of expediting the separation of the fibre by the hot water process. A fermentation continued for only 20 or 30 hours, followed by the use of the rollers, affording results far more satisfactory than when fermentation for the usual time (66 hours), but without rolling and washing is adopted.—From the *Kunst und Gewerbe-Blatt des Polytechnischen Vereins für das Königreich Bayern*.*

On the DETERMINATION of the STRENGTH and MONEY VALUE of COMMERCIAL CYANIDE of POTASSIUM, and on a METHOD of ESTIMATING CYANOGEN and HYDROCYANIC ACID.

By THORNTON J. HERAPATH.

In a paper, published some years since in the *Mining Journal*, Mr. Parkes gave a description of a process by which copper may be readily estimated, by means of a standard solution of cyanide of potassium; and a short time ago (*THE CHEMIST*, N. S., vol. iii, No. 81, May, 1856), I showed that a simple modification of Mr. Parkes' process would enable us to determine the strength and purity of commercial cyanide of potassium, and, in many cases, to estimate quantitatively cyanogen and hydrocyanic acid. Owing to the temporary loss of my note book, however, I was prevented at that time from giving full particulars with regard to the method in question, and was compelled

* We are indebted for the accompanying important memoir to the kindness of our highly esteemed member, Baron von Liebig, who, at our request, induced his former pupil, Professor Hodges, to forward to us an authentic account of some of the investigations in which he had been engaged in Ireland in relation to this subject, which is of such great importance to this country (Bavaria); and who has kindly complied with our wish, for which we desire to return him our best thanks.

to confine myself to a simple description of the *modus operandi* pursued. Since my arrival in this country, however, my memorandum book has again come to hand, and I now hasten to furnish your readers with all necessary information on these points. In testing a sample of commercial cyanide of potassium by the process I have alluded to, when the operator wishes to ascertain the actual proportion of cyanide of potassium existing in the drug, the first step to be taken is to reduce the cyanide to a fine powder, and weigh out accurately 50 or 100 grains. This quantity is dissolved in a little distilled water and introduced into a graduated burette, together with the washings of the vessel in which the solution has been effected, and a sufficient quantity of water is added to fill up the instrument to the top of the scale. The whole contents of the tube are then well mixed up together by agitation, so as to render the liquid of uniform strength. A certain known quantity, say 5 or 10 grains of pure electrolytic copper, or an equivalent quantity of pure crystallised sulphate of copper,* is next dissolved in a flask in dilute nitric acid, and the solution so formed is supersaturated with *liquor ammonia*. The blue cupreous solution so prepared, is afterwards raised to a temperature of about 90° or 95° F. on a sand bath, and the cyanide solution is added gradually from the burette, until the liquid has become absolutely decolorized. From the proportion of cyanide solution that has been taken, the per centage of pure cyanide of potassium existing in the sample may be easily calculated by a simple Rule of Three sum. Every 24·61 grains of copper are equivalent to 100 grains of cyanide of potassium; one equivalent of copper in an ammoniacal solution, requiring two equivalents of cyanide to produce decoloration. Let us suppose, therefore, that 75 measures of a solution of cyanide of one per cent. (i. e. 100 grains of cyanide to the 100 measures) are taken to decolor 10 grains of copper.

As 75 : 10 :: 100 to 13·33 and

As 24·61 : 100 :: 13·33 to 54·16.

It is clear, therefore, that the cyanide operated on contains 54·16 per cent. of pure cyanide of potassium.

Five samples of commercial cyanide of potassium, which I tested by this process, were found to contain respectively :—

No. 1.—57·9	per cent. of pure cyanide.
No. 2.—60·1	" " "
No. 3.—40·5	" " "
No. 4.—64·7	" " "
No. 5.—49·8	" " "

In ascertaining the strength of officinal hydrocyanic acid by this

* Care must be taken to purify the copper from its coat of oxide, by first digesting it in dilute nitric acid; when sulphate of copper is employed, the crystals must be crushed and reduced to a fine powder, and then pressed thoroughly between two pieces of bibulous paper, in order to get rid of the interstitial moisture. 390·6 grains of pure sulphate are equivalent to 100 grains of copper.

process, I operate in a very similar manner; I weigh out in a Schuster's alkalimeter a certain known quantity of the acid to be tested, and add it carefully to a warm ammoniacal solution of copper in a small flask, until the blue color is entirely removed, taking care to agitate the vessel now and then, so as to avoid any hydrocyanic acid. 100 grains of copper are equivalent to 168.75 of hydrocyanic acid, or 162.50 grains cyanogen. Sometimes the operator will find it more convenient to employ a graduated burette in place of a Schuster's alkalimeter. In this case, it is only necessary to determine the specific gravity of the hydrocyanic acid solution, to make the necessary corrections in the calculations.

Herradura, Chili, S. America, Oct. 29, 1856.

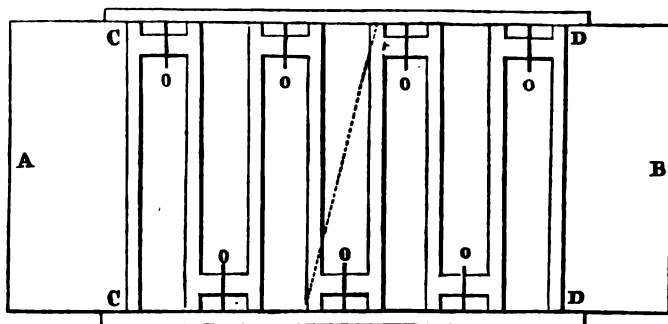
SOME REMARKS ON THERMO-ELECTRICITY.

It has been usual to include mercury in the thermo-electric series of metals, and Cumming, and many others, probably copying from him, place it near bismuth; but De la Rive excludes it from the list given in the second volume of his "Electricity," and says—"contrary to the opinion of many philosophers, mercury cannot become thermo-electric." To an outer-court worshipper in the temple of philosophy the obvious conclusion from such a statement is, that mercury cannot be used in the construction of thermo-electric batteries. Perhaps the initiated and more favored votaries of science may perceive a meaning in the words which would not lead to the same conclusion: it is not, therefore, without trembling, that a mere plebeian tyro dares to question the assertion of a prince and veteran in science. That mercury can be used as an efficient element in a thermo-electric battery, the writer thinks he has proved in a paper in the *Pharmaceutical Journal* for November. The object of the present paper is to question a more general assertion, in the same paragraph, of the above writer, who says, "one of the conditions essential for the development of thermo-electricity, is that the heated body shall be solid."

It is well known that, in a metallic wire, all that is required to make it capable of developing thermo-electricity is to destroy its homogeneity, by hammering, softening, or straining a portion of it; and it is a natural thought, that the reason currents cannot be developed in mercury is that its fluidity renders it difficult to produce a similar change in it. In the following experiment, it was thought that perhaps the pressure of a column of mercury 27 inches high might have some effect similar to straining or hammering on solid metals. A glass tube was bent at right angles, one arm being 27 inches long, the other 13—the shorter one was stopped with a cork through which passed an amalgamated copper wire;—the tube was supported so that the long arm was perpendicular, and connection with the galvanometer was first made with a small iron wire twisted on the end of one of the copper. The heat of the finger caused a considerable deviation of the needle; this was to prove

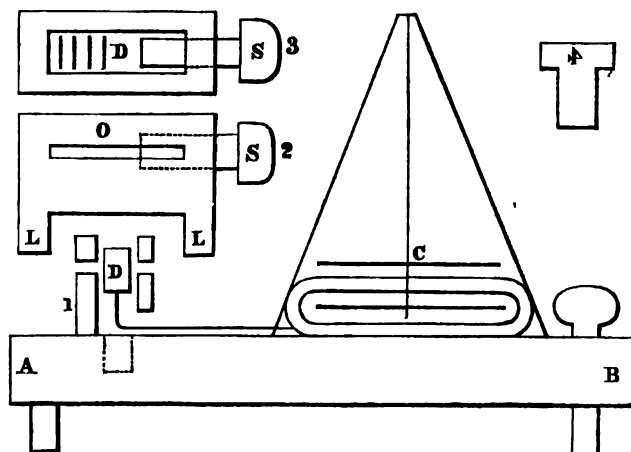
the connections good. The iron wire was then removed, and the tube of mercury connected directly with the galvanometer, and heat applied at the bend; but in three or four different experiments no current was perceptible—thus proving that the pressure of the column of mercury did not sufficiently modify the molecular arrangement of the metal to cause a sensible thermo-electric current.

The following experiments, however, appear incompatible with the law of De la Rive—that the heated body must be solid. A B is a piece of close grained wood, about 4 inches long, 2½ inches



wide, and ¼ an inch thick; across it are cut 8 or 10 cells about 1/10th inch wide, 1/4th inch deep and 3/8th inch apart; they are connected with each other at the alternate sides of the board, but not quite at the margin. In the middle of each connection, at O, the board is cut through with a very fine saw, about 3/8th inch deep. In these cuts are inserted small pieces of common tin-plate, amalgamated. C D are pieces of copper, 1 inch broad, the side towards the cells being covered with paper and well secured to the board. In filling the whole of the cells with mercury, and applying heat to the copper not on the side of the connecting wires, no current will be apparent; but if in the alternate cells a liquid amalgam of bismuth and mercury be used, a current is at once perceptible; in the apparatus used, with only a moderate heat, it was 6°, and with two pairs, 3°, a piece of iron wire amalgamated at the ends being placed in two cells, as shown by the dotted lines, the needle deviated 19° when the two currents were in the same direction, but when opposed to each other, only 9°. The small pieces of tin occupy the place of solder in ordinary batteries, and that the current is not produced by the contact of these plates and the mercury, is proved by the first experiment; for when the mercury was of a uniform character, no current was produced, but when the two liquid elements differed in their composition, a current immediately was manifested. The bismuth amalgam was quite liquid,—in one case being pressed through leather and in another, strained through a very small hole in a glass tube. Does not this battery prove that De la Rive's assertion cannot be accepted as a law? In a paper recently printed in your pages, the author stated that the galvanometer used

was a very sensitive one, with 200 turns of wire. It is not perhaps, going too far to say, that in any thermo-electric experiments whatever, at least, unless with a battery of several pair, such a galvanometer would be much improved by being divested of at least two-thirds of its wire; the intensity of the currents being so feeble, the multiplication of coils does not compensate for the increased resistance of the wire. The galvanometer used in the above experiments is, perhaps, about the best size for ordinary thermo-electric studies; it has astatic needles, and two lengths of No. 20 copper wire, with 28 turns each, and can be arranged so as to have the two wires, either as a double one of 28, or a single one of 56 turns, or as a differential galvanometer. Several experiments were made with four different arrangements of metals, and in every case the deviation of the needle was considerably greater with the double wire of 28 turns, than with the single wire of 56, in fact, the single wire of 28 was nearly equal to the 56; had the wire been smaller than No. 20, the difference in favor of the shorter wire, would of course have been greater. As the author believes the mode of making and varying the connections in this galvanometer is new, simple, expeditious and cheap, a description is added; it is borrowed from the galvanic battery described by him at the last Meeting of the British Association. A B is the stand, C is



the coil and needles, suspended in the ordinary way by a single film of silk; 1 is a section, 2 a front, and 3 a top view of the connection apparatus, which is made of wood; D 1 and 3 are the four ends of the wires, to which are soldered small pieces of thin copper, about $\frac{3}{8}$ in. square; S is a wooden screw, O is a narrow slit, to admit the wires to be connected with the galvanometer; L are legs, which fit rather tight into the support A B, but should be moveable, to clean the coppers occasionally; four or five small thin pieces of wood, fig. 4, to fit into the top 3, will enable the connections to be varied as required,

and all the work can be done much easier and the connections be better made than with a binding screw attached to each wire, which would be the ordinary plan. While on the subject, it will not be out of place perhaps, to mention the absence of a table of errata, in Walker's translation of De la Rive, and this is to be the more regretted, as there are some errors in figures, which are not so obvious at first sight as mistakes in words; there is a considerable error of the kind in page 544. There are also some similar blunders in Lardner and Walker's Treatise on Electricity. There is another rather tantalising oversight in the former; De la Rive having added a very useful table of writers at the end of every subject, in which many English authors are included; this list is only literally copied in the translation, without any reference to the English works in which the information can be found; whereas, probably, in many instances, the French authority is at the same time a translation from an English original, and no doubt many of the foreign articles have appeared somewhere in an English dress.

W. SYMONS.

Dunster, January 21st, 1857.

CHEMISTRY *as a* SCIENCE.

By DR. H. C. JENNINGS.

FALSEHOOD, in the case of chemistry, as in that of astronomy, has been the parent of truth and science; the astounding and impudent pretensions of the alchemists led to investigations which gradually delivered chemistry from the most degrading errors, and introduced it to the philosopher, until at length it has become established as the chief science. Astronomy was ushered into existence by the artful astrologer; it was delivered from the hands of the venal charlatan, and is established upon the highest order of mathematical formulæ and power. These sister sciences arose in the infancy of the human race, when ignorance and superstition were dominant and universal. They now dwell in a temple, whose only bounds are those of space, and whose duration will be coequal with that of time. The number of the votaries of chemistry is increasing every year, and amongst them we count some of the most illustrious and virtuous of mankind. Each succeeding year decorates her temple with new trophies which dignify and contribute to the comfort and happiness of the whole human family. Chemistry presents no Royal road, either to fame or to fortune; the patronage of the aristocracy is here at least impotent. It can only offer its homage, or enlist with the true nobility of nature and become a fellow laborer—become a Stanhope—we will not offer a catalogue of the imperial names that have worn the diadem, and wielded the sceptre of chemistry. They are stereotyped upon the page of eternity, and will be remembered long after royal and noble names have vanished from the records of the past.

We cannot place astronomy at the apex of the scientific pyramid. It is a luxury which few minds can enjoy, which few heads are capable of

exercising; it is doubtless the most unlimited in relation to space and time, and requires the most powerful efforts of human genius; but, *qui bono*, after its application to navigation, and the solution of the problem of the eternity of the systems of the universe, it is limited to the few, and uninteresting to the many, while chemistry is lavish of bounties, splendors, and wonders. We cannot look around us without beholding some immediate beneficial advantage, and acknowledging some admirable result. Chemistry is operating silently, but surely, the most prodigious changes in the political economy of the world; it will vanquish the desponding and desolating chimera of the Malthus doctrine, which states that the supply of food is bound in the Adamantine limits of geographical space, while the law of human propagation and animal life follow arithmetic progression that must at last overstep the possible maximum of supply.

Chemistry has not, however, yet acquired that legitimate empire over the human mind to which it is so well entitled by its universality and importance. It is still regarded by many as nothing more than a system of ingenious tricks and experiments, but little more respectable than those that are exhibited to amuse or surprise the vulgar and the idle. It shall be our task to offer some facts that will correct this error of the ignorant and careless.

Chemistry embraces within its range the three kingdoms—the animal, vegetable, and mineral; and all the other laws of the Deity, that is, all physical sciences and natural philosophy, contribute to its practice and development. Astronomy could never have advanced as it has done, without the perfection and improvement of glass, and the combinations of the speculum; without these combinations (strictly chemical) thousands of nebulae would have remained undiscovered, or continued to be regarded as luminous mists, instead of becoming resolved into suns—each nebula being a distinct solar system—many of them greater than that of which our sun forms the centre.

The chemistry of nature is on the grandest scale; its operations are calculated by leagues and centuries. It is active in the whole atmosphere that envelopes our planet. Its energies are constantly effecting permutations on the surface of our globe; everything is changing, yet not an atom is ever lost or annihilated. The ocean and its most profound vallies are also its theatre, and it is highly probable, that the central solid contents are also subject to its influences and laws.

The atmosphere presents the magnificent phenomena of meteors, incalculable in number and in the rapidity of their motions, and of aerolites from the size of dust to the weight of several hundreds of pounds. It contains iodine and carbonic acid, in addition to its constituent elements and aqueous vapor, and is suspected to contain other chemical forms of matter, which as yet have proved too subtle to yield to the coercion of mind; we need hardly allude to the electric chemistry of our atmosphere, whose stupendous effects in the form of lightning, affect most powerfully both vegetable and animal organizations.

Terrestrial chemistry extends over spaces which have as yet been deemed immeasurable. The earthquake is the result of the reciprocal action of iron, sulphur, and water. The volcano owes its prodigious activity and duration to the same cause; we have evidence that water is resolved into its two elementary gases; one a combustible, the other a supporter of combustion—here we have two giant powers contributing to produce the same effect.

The coal formations reveal the history of forests which have once been upon the surface of our earth; they contained animals, and seemed to have possessed a greater degree of atmospheric caloric, electricity and aqueous vapor than at present; these immense plains became displaced and levelled, or they became covered with alluvial earth; sulphurous vapors and a moderate excess of heat, while under compression, converted them into the crystalline forms which are now so important to our national and commercial prosperity and progress.

Next we have the chalk formations; consisting of the *debris* of shells, whose substance (carbonate of lime) remains; and when we consider that these strata are 1500 to 3000 feet in thickness, and extend over several counties, we obtain but a feeble glimpse of the magnitude of these operations, or of the possible extent to which they may reach in other portions of the universe, or in other planets. To allude to all the combinations we find, would be to give an analysis of mineralogy and geology.

The ocean presents in its profound and silent depths, operations which rival any of the terrestrial formations. The leagues of coral islands, the work of countless myriads of marine animalculæ, at work night and day, forming numerous tissues of solid carbonate of lime and magnesia, which frequently serve the purposes of man; the ocean is one vast laboratory of chloride of sodium, iodine and magnesia, and probably contains many substances which have not yet been recognised by the genius of investigation. The perpetual equality of its temperature, its constant supply of aqueous vapor, to furnish the atmosphere with clouds and rain, tend to render it an inexhaustible mine of chemical force, activity, and wealth.

We now direct our mental vision to practical or scientific chemistry. In this field it exerts an influence so infinite and varied, that we can only afford a glance at the more salient items of this master science. Gunpowder has rendered war less tedious and brutal, and is used with much advantage as a power in the operations of blasting rocks, and perhaps it will ere long, figure as a cheap and valuable motive power; but chemistry has given birth to an agent 3000 times more potent—chloride of nitrogen;—as yet the chemist rarely desires to handle this terrific substance; but the day may yet dawn upon its introduction as one of the obedient servants of mankind.

Behold yon graceful group, arrayed in tissues of silk, wool, and muslin! what kaleidoscopic variety of tints! all the mutations of the prismatic colors are there in bright display, and these colors are produced by simple animal and vegetable infusions, with the addition of a few metallic salts.

Illuminating gas; a true product of chemistry, and a vast branch of industry; see that spark, as faint and diminutive as the love signal of the humble glow-worm; at the touch of a finger it bursts into a volume and intensity which almost rival a beam of the brightest sun light.

The electric telegraph, which in a few minutes can "waft a sigh from Indus to the Pole," which annihilates time and space; is mainly an application of chemical action upon metals, by which the electric current is determined.

Shall we avow the dependence of medicine on the chemist's art?—rather, can it exist, but as empirical, without being governed by the laws and products of the laboratory? Whether we view the subject in relation to its curative and alterative properties, or its toxicological powers, it is sufficient that, could we deprive the physician of our results, we should leave him a field of operation little above that of the quadruped tribes. The chemistry in constant activity throughout the organic systems, both vegetable and animal, is of such importance that physiology and hygiene cannot advance or become a science without an intimate knowledge of its peculiar actions—the action of the gastric fluid—the formation of bile, gall, and urea—the consumption going on through the pulmonary tissue; its production of heat and electricity—the formation of animal poisons of the most intense malignity—the production of colors by fish and insects—of lac and musk, and an infinite variety of oils that surpass calculation and defy description—the vegetable kingdom, with her endless creations of organic matter, whose utility is the theme of our daily experience, which are daily crowding upon us, and fall upon our senses and admiration from every part of the globe.

Artificial chemistry has elicited the sweetest tastes and odors from the most foetid of our residual matter—the feints or fusel oil, an excrement of fermentation, in the chemist's hands, is transmuted into the flavor and odor of the pear, the apple bergamot, and many other substances, giving equal pleasure to the senses with those produced by horticulture and the fairest effects of nature.

Agriculture begins to blush and relinquish its stolid obstinacy, and bows to chemical sovereignty and laws. It recognises ammonia, chloride of sodium, silica and super-phosphates, as not only auxiliary to good crops, but as absolutely necessary, and as an instrument of success and a source of profit.

The brewer, distiller, soap-maker, and candle manufacturer, dyer, sugar-refiner, baker, varnish-maker, glass-maker, alum manufacturer, soda-maker, &c., &c., are all practical chemists; and if one individual could perfectly perform all these different operations, it would be hardly possible to deny that he possessed considerable chemical knowledge. It is empirical, it is true; but in every one of the above businesses chemical substances are employed or chemical actions produced. It is true that these persons can render no account of the causes and phenomena, or perhaps discover why certain operations fail, or partially fail; because they do not possess the scientific knowledge necessary to enable them to penetrate the source of the failure. Therefore

each trade should at least make a point of studying and acquiring the real nature of the chemical operations they are called upon to exercise; this cannot fail to ensure success, economy, and improvement, and must, in the end, prove of advantage to the whole community; for we are all interested in the saving of raw material, and obtaining every degree of perfection that the laws of science can offer.

These are feeble attempts toward the productions of a general and popular consideration of chemistry, and to attract towards this science the serious attention and zealous devotion to which it is so highly entitled; we may go yet further, and show its connection with every other science. We cannot investigate the nature of gaseous bodies without gasometers, pneumatic troughs, condensing and exhausting pistons and pumps; and a great variety of other mechanical instruments and contrivances; hence mechanics, hydraulics and pneumatics become a necessary part of the chemist's education.

No chemical combination can be called into action without the production of electricity, caloric, or light, and perhaps, in some cases, magnetism. The infinite permutations taking place amongst the atoms or molecules of matter, when in chemical activity, and these affecting a definite attraction or affinity, compel us to submit chemical agency to mathematical formulæ; it is hardly possible to appreciate the more delicate affinities, and judge correctly of the results obtained, without submitting our operations to the rules and laws of mathematics. Nomenclature, the most fruitful and positive aid in chemical advance, compels us to draw (at least partially) from the fountain of classic lore.

All the productions of the animal and vegetable kingdoms are principally composed of carbon, hydrogen, oxygen, and nitrogen, and we are entirely destitute of any knowledge of the ultimate constitution of these substances; we can only grasp their physical properties according to the action they may have upon our senses; they combine frequently in binary, tertiary and other proportions, so that the permutations or relations they enter into with each other are absolutely inexhaustible, especially if taken with relation to the effects of caloric, light, and electricity; these are found to influence the modes of corpuscular attraction or atomic combination, and singularly modify the product.

Nature is the art of God; science is the knowledge of the instruments by which He accomplishes His ends. The study and practice of chemistry must necessarily lead to the best comprehension of the operations of the Deity; for all there is effected by weight and measure, in numerical proportions, by the most rigid attraction or the most impassable repulsion. If we sometimes encounter small portions of matter not in true atomic or chemical affinity, we have no difficulty in determining that they are fortuitous, possessing no inherent quality essential to the substance in a chemical or natural sense. We find iodine in sponge; but when it is removed, the zoophite remains as perfect as when it was present.

We need not allude to the chemical nature of the panary, alcoholic,

vinous and acetous fermentations, and the vastness of their commercial and beneficial results—all must see and feel these in their daily journey through life.

We admit every creed and every nation into our fraternity; we invite the attention and invoke the devotion of all mankind to this science; it cannot fail, if zealously and carefully pursued, to assist in the attainment of fortune if applied to industrial purposes. Here we are subdued by no tyranny but that which charms and elevates; no despotism, but that which instructs and embellishes. It plays its part in sustaining life and health, and in administering to comfort; and in these the inferior animals which depend on man, participate. It imparts to its eager votary mental refreshment, vigor and enjoyment, which defy satiety. He rises from the repast a wiser and a better man.

January 12th, 1857.

On the ELECTRO-CHEMICAL DEPOSITION of ZINC.

By ALEXANDER WATT.

DURING the last few years many processes for the electro-deposition of zinc—with a view to remove some of the evils and prejudices of the *galvanised iron*—have been introduced; and most of which have been patented by the various inventors.

Nearly all the electro-metallurgists who have experimented on the deposition of this metal, have employed acid solutions; and I need hardly say that all who have employed such solutions have failed to obtain a proper reguline deposit of zinc upon surfaces of either iron or steel. The principal cause of this failure being due to the action of the acid contained in the salt of zinc, with which the bath is prepared, independent of any influence of the battery.

Having had my attention called to the importance of a process by which a good reguline coating of zinc could be deposited upon iron or steel, as a substitute for galvanised iron, I made a series of experiments upon an extensive scale; and taking advantage of the failures which other experimenters had unfortunately met with in employing acid solutions, I turned my thoughts in another direction, and sought to make a bath from an alkaline solution of zinc. I made many solutions, employing in various ways all the known salts of zinc, and in the course of my labors I was thwarted by many failures, and led astray by many apparent successes. Determined not to be beaten if possible, I pursued a fresh course of experiments, based upon a different principle. I had employed, during my investigations, many different forms of battery, some yielding considerable *quantity* of current, and others of great intensity, which of course gave me very variable results. If the battery were too intense, the zinc was deposited in the form of a dark grey, or nearly black powder; and with a battery yielding great quantity with feeble intensity, the deposit, although of good character, was partial, taking place only or chiefly at the edges of a

flat surface. After repeated experiments—which traversed many months—I found that the carbon and zinc battery, carefully constructed, and the zinc being well amalgamated, gave me the best results. But I had a difficulty yet to contend with; the solution which I had formed, being either too strong or too weak; I therefore determined on ascertaining, as nearly as I could, the proper strength which the solution should possess in proportion to the battery power employed. In the following specification, which I filed on the 3rd of June, 1856, will be found a faithful description of the solution I employ, and with which I have obtained some exceedingly beautiful results, I give the specification entire, in order that the readers may be made fully acquainted with the details of the process.

I have found that articles of steel may be coated by this process with great success, and, when removed from the bath, they may be scratch-brushed, and thus rendered exceedingly bright. Zinc, when deposited in a reguline form, has a very pleasing appearance, it is also tolerably tough; being removed from the surface on which it is deposited by a chisel or knife, it will *curl* up, which would not be the case if it were deposited from an acid solution; but, on the contrary, would *crumble* when an attempt was made to remove it from the surface on which it was deposited.

SPECIFICATION.

My Invention consists, firstly, in forming a solution for the purpose of coating iron or steel with zinc by galvanic agency. To form the solution I proceed as follows:—I dissolve 200 ozs. of commercial cyanide of potassium in twenty gallons of water (rain water or distilled water being preferable) in a suitable vessel; I then pour into this solution 80 ounces by measure of strong liquid ammonia (of the specific gravity of 880 I prefer). Having stirred these compounds together, I place several large porous cells, such as those used in forming Daniell's batteries, in this solution, and pour into each of these porous cells as much of a strong solution of a cyanide of potassium (say, about 16 ozs. to the gallon), as will be equal to the height of the solution in the larger vessel; I then attach several pieces of metal, copper or iron by preference, to pieces of copper wire, which are then to be attached to the negative pole of a galvanic battery. These pieces of copper or iron are to be placed in the porous cells. I next attach a piece or several pieces of zinc to the positive pole of the battery, and I then immerse these pieces of zinc in the solution of cyanide of potassium and ammonia. For the above purpose I prefer using good milled zinc, the weight of which is to be ascertained before immersion; and I think it better to "pickle" the zinc slightly previous to immersion in the cyanide solution with dilute hydrochloric acid, after which process it should be well rinsed in clean water. The galvanic battery is now to be set in action, and allowed to continue in action on the above materials until the solution of cyanide of potassium and ammonia has taken up about sixty ounces of zinc, that is to say, about three ounces to the gallon of solution.

As soon as the pieces of zinc have been weighed to determine the amount dissolved into the cyanide solution, I dip them into dilute hydrochloric acid, and then rinse them, when they are placed aside for future operations, if necessary; the porous cells are then to be removed. I now dissolve 80 ozs. of a carbonated alkali (I prefer the carbonate of potassa) in a portion of the above solution, and when dissolved I add it to the original solution, and stir the whole together for a few moments, after which I allow the solution to stand undisturbed until the sediment formed has subsided; I then transfer the clear solution to another vessel, when it is ready for use.

The above solution may be made in a more concentrated form, if required, say, with half the quantity of water, and it may be diluted down to the required strength by adding more water when wanted to be worked. I prepare cast or wrought iron or steel to be coated with the above solution in the following manner, having first made a pickle composed of, say,—

Sulphuric acid	.	.	.	.	.	1 lb.
Hydrochloric acid	.	.	.	.	.	$\frac{1}{2}$ lb.
Water	.	.	.	.	.	2 gallons.

The articles to be coated are first plunged into the above pickle, and allowed to remain until the oxide of iron is easily removeable with a brush, sand, and water. As soon as the articles are sufficiently pickled, they are to be rinsed in clean water, and are then to be cleaned with a hard brush, sand, and water, and any oxide which may not have been quite removed by the pickle may be scraped or otherwise removed, or the article be returned to the pickling bath until these parts yield to the brush and sand; or the iron or steel may be cleaned by the processes ordinarily used at the "galvanised iron works." When the articles to be coated are quite free from oxide, they are to be well rinsed in clean water, and immediately placed in the zinging bath, in connection with the negative pole of the battery.

As soon as the articles have received a slight coating they should be removed from the bath and examined, in order to ascertain if there are any parts remaining unclean, in which case those parts should be cleaned, and the whole article to be once more brushed all over as before and then returned to the bath, where it is to remain until sufficiently coated. It is as well, however, to move the article about in the solution occasionally, in order to insure an uniform deposit. As soon as the articles are sufficiently coated they are to be removed from the bath and rinsed in clean water (hot water being preferable), and they may then be placed in sawdust to dry them. The articles may be rendered bright either by means of the scratch brush, or by gently scouring with silver sand, water, and a soft brush. When the above solution has been in use some time it will be necessary to add occasionally a little cyanide of potassium and liquid ammonia, so as to keep the solution at as near as possible the original strength; and if the solution, from being worked with too small a surface of positive electrode, or if from other causes it has become deprived of a portion

of its zinc, I place in the solution (by suspension or otherwise) several porous cells, which I fill with strong solution of cyanide of potassium, and into which I place pieces of copper or iron as before, in connection with the negative pole of the battery, and the zinc electrodes I attach to the positive pole of the battery, by which means I am enabled to keep up the strength of the solution. The above solution is to be worked with zinc electrodes (milled zinc being preferable), and it will be necessary in coating flat surfaces especially to place a piece of zinc on each side of the article to be coated; for instance, if sheets of iron are to be coated, they are to be placed in the solution alternately, that is to say, sheet zinc, sheet iron, sheet zinc, and so on, (the sheets of iron and zinc exposing about the same area of surface,) otherwise the surface opposite the zinc electrode will receive the greatest amount of deposit. I prefer using a battery which yields a considerable quantity of electricity, and the action of which can be maintained for a considerable time without losing its power, by which means I not only secure a good deposit, but uniform results. The battery which I prefer is that known as Bunsen's battery, or a battery composed of carbon and zinc elements. Two or more 4-gallon cells of this battery may be used when large articles are to be coated, or when a considerable quantity of work is to be done in the bath at one time. When cast or wrought iron or steel has become much rusted, it may be cleaned with strong hydrochloric acid, or with a strong pickle composed of hydrochloric acid and water, but it must not be allowed to remain too long in this pickle, or the iron or steel will be acted upon.

Articles of cutlery may be coated by the above process to preserve them from oxidation or rust in damp climates or during sea voyages, &c., and as they will only require a slight coating for this purpose, they will not require to remain long in the bath. In pickling bright steel articles, I should not recommend the use of any hydrochloric acid.

Having thus described the nature of my invention, and the manner of performing the same, I would have it understood that I do not confine myself to the exact details or quantities described; but what I claim is, the method of preparing a solution of zinc (suitable for coating iron and steel with zinc) by means of electricity, as herein described.

TRANSLATIONS AND ABSTRACTS.

BIOGRAPHY.

ROBERT BOYLE: *a* BIOGRAPHICAL STUDY. By M. CAP.

(Continued from page 206.)

IN the year following the appearance of his first researches in physics, Robert Boyle made his *debut* in chemistry, by the publication of a very remarkable work entitled: *The Sceptical Chemist; or Chemico-Physical*

Doubts and Paradoxes, &c., (Oxford: 1661, 8vo. London: 1662, &c.) In this work he put forth this doubt relative to the facts and principles of which the science—which was still left, in a certain degree, in the hands of the last alchemists—then consisted. The work was preceded by a *preliminary discourse*, in which the author explained the large and elevated views which should preside over the development of sciences. He frankly announces that he rejects the erroneous traditions of the scholiast; he declares that, deserting the narrow paths to which chemistry was restricted—such as the transmutation of metals, the universal panacea, and the preparation of medicaments,—he wished to consider it only as a philosopher—that is to say, in the same manner as he treated the physical questions. Men of science, says he, jealous of their renown, as of the advancement of science, should above all repel vague theories, and refer only to observation. He relies only, for the progress of truth, on the experimental method, and admits as real only the data which result from it. He is astonished and distressed at the variance which exist between the schools, and can find the explanation of it only in the insolidity of the facts observed. Although his first object was to establish the immense power of experimentation, he believed with Paracelsus and Van Helmont, that more than one science, but especially medicine, would derive great assistance from a more profound knowledge of chemical facts, and he wished that men of science would unite, at least for the purpose of establishing the fundamental points on which they should agree, in order to form from them the definitive bases of the science.

He began in the same work to rise against the doctrine of the elements. This was the first point on which the philosophers could not be understood. Each school had a different opinion on this subject. The peripatetics, with Aristotle, admitted four elements; the spagyrist, with Basil Valentine, acknowledged only three; and the chemists, with Nicholas Lefebvre, held that there were five. Each of them made one or other of them play a superior, and, in some measure, universal part. The followers of Empedocles and Anaxagoras placed fire in the first rank; Van Helmont and Beccher regarded earth as the principal element; in the time of Galileo and Pascal, as later under Hales and Priestly, the air was regarded as almost the only element. Electricity, in our day, plays a nearly analogous part; it is often suggested as the cause of phenomena of which no natural explanation can be found. Boyle asserted that none of them were elementary, that is to say, undecomposable, bodies; that the true elements were more numerous than was supposed; and that time would infallibly lead to the discovery of new ones. We know how largely this instinctive view, which was so greatly in advance of the period, has been realised.

Further on, Boyle attacked that systematic obscurity under which the alchemists obstinately concealed what they knew, or rather what they did not know. He said that their figurative, enigmatical language serves principally to hide the emptiness of their knowledge, and the uncertainty of their processes. He applies himself to show the vague-

ness of their theories, their contradictions, the inaccuracy of their experiments, which, in the state in which they were presented, could not serve as the basis of any truly rational doctrine.

Entering upon the question of combinations, he regards them as an association of atoms of different forms and sizes, animated with various movements, and the arrangement of which may vary, without the mixture changing in composition. This is evidently the doctrine of isomerism. He believed that fire often operates these changes of order among the elements of a mixture, and that it gives rise to as many new combinations as decompositions. Thus, guaiacum, burned in the fire, and in the open air, says he, is converted into ashes and soot; whilst heated in an alembic, it is resolved into spirit, vinegar, water, and charcoal. This shows how *calcination* differs from *distillation*, and we are surprised that, notwithstanding this ingenious view, the chemists of the seventeenth and eighteenth centuries should have so obstinately pursued their vegetable analyses by incineration. Boyle also distinguishes in the most precise manner the simple *mixture*, in which we find the properties of its principles, from the compound mass (chemical combination) which does not retain any of the characters of its constituents. He cites, as an example, the sugar of lead, whose elements have not a sweet taste, and he recognises in this result an order of phenomena peculiar to the chemical forces.

The Sceptical Chemist now opened to science a quite new route, already traced, indeed, by Van Helmont, whom Boyle resembled in so many respects. This route necessarily deviated from the way followed by chemists, for, as a sincere and disinterested man, Boyle had none of the sentiments which animated most of the adepts. His work was the most fatal blow which the spagyric doctrine could receive. It was the most complete and the best reasoned which had been written on the matter. In it he combatted error with force, but with dignity and moderation; knowing well that violence and wrong injure self-esteem, without causing conviction, and relying for the triumph of his views on the ascendancy of truth.

The persevering study which Boyle made of the physical properties of the air led him to study its chemical composition, and consequently the question of gases. This was the subject of various special publications; but most of his researches were included in his *Physico-Mechanical Experiments*, to which he constantly made new additions. Galen, of old, had said that flame was an inflamed air, and that the reed burned only because it contained much air susceptible of being inflamed.* Drebbell, at the beginning of the seventeenth century, had observed that one portion only of the air served for respiration and combustion. Hawksby had remarked that the air, after having passed over incandescent metals, became unfit for respiration and extinguished the flame of a taper. Boyle confirmed all these experiments, and concluded from them that atmospheric air contained some vital substance which performed a part in combustion, fermentation, in

* De Simpl. Medic. Facultat. L. 1, 14.

the formation of nitre, and in the respiration of animals. He had even remarked, that the air which had been used for combustion had diminished in volume. It is evident that he was on the way to the discovery of oxygen, and even of nitrogen; and it is so much the more astonishing that he did not arrive at it, that he had just made an important step in the history of the gases, in devising a kind of apparatus for the purpose of collecting them, by means of a true pneumatic trough.

It is known that Van Helmont, and to whom we owe the term *gas*, had not arrived at collecting them. Boyle was more fortunate. He placed in a small glass matrass a mixture of sulphuric acid and common water. After having thrown into it some small iron nails, he immersed in another vessel, also containing acidulated water, the extremity of the neck of the matrass. He immediately saw æriform bubbles rise, which, on collecting, depressed the water, and occupied its place. The water of the upper vessel was in a short time completely expelled, and replaced by a body which had all the appearance of air. This was evidently hydrogen; but Boyle still thought he had produced in this experiment only *artificial air* (*air generated de novo*).

There was only wanting in this apparatus a *bent* tube, independent of the generating vessel, in order that Boyle might have studied the gases collected, at his ease, and that he might have anticipated the savants of the eighteenth century in the career of pneumatic chemistry. The construction of this apparatus, appears now very simple; but we know that simple ideas often come last. After him, Mayow, his pupil, used an empty bladder, adjusted to a matrass for collecting gases; Hooke employed a flask with two apertures, one of which was furnished with a bladder. Bernouilli, in the eighteenth century, collected carbonic acid gas by immersing a tube in a vessel filled with an acid liquor, and into which he had introduced chalk. Hales filled with water a matrass inverted over a basin likewise full, and into which the gas was conducted by the long neck of a retort, the point of which had been bent up. Finally, later, he replaced this point by an S tube, which was fitted at one end to the retort, and at the other to the inverted matrass. Thus, it was more than a century before this apparatus arrived at the degree of perfection in which it was found by the chemists of the last century, that is to say, capable of serving so advantageously in the researches of Priestley, Black, Cavendish, and Lavoisier.

With regard to the action of the air on metals, Boyle, who, like every one else, at that time, was ignorant of Jean Rey's instinctive theory, sought the cause of the increase of the weight of metals by calcination in the fixation of the molecules of fire, which the scientific men of the period still regarded as a ponderable matter.* However, he advanced, that verdégris and iron rust are engendered by the corrosive effluvia of the air; and, he added, that the study of these efflu-

* *New Experiments to make Fire and Flame stable and ponderable.—Fire and Flame weighed in a balance, &c.*

via and of their action on the metals, will lead to the knowledge of the composition of the air. He had even devised for this purpose an apparatus known by the name of *Boyle's Hell*. This was a matrass with a flat bottom and a very long neck, in which he boiled mercury, which after a few days was converted into precipitatum per se.

Boyle doubted the composition of water, because, says he, it serves as aliment for vegetables, and gives rise to very various products; sea water greatly attracted his attention. Aristotle had said that the saltiness of the sea was caused by the action of the sun, and that sea water was salt only at the surface. Scaliger had renewed Aristotle's assertions in this respect. Boyle, to ascertain the fact, constructed a ship with a valve, by means of which he procured sea water from various depths, and he ascertained, not only that this water is equally saline throughout, but that its specific density does not vary perceptibly, at least at a point at a distance from the currents and springs which exist near the coasts.* He thought that the salt existed in considerable masses at the bottom of seas, as it is found sometimes in the bosom of the earth in which it gives rise to salt springs. He wished that the degree of saltiness of the different seas might be compared, and, for that purpose he proposed the employment of a solution of silver in aqua fortis, a reagent so delicate, he says, that it would detect one grain of common salt in 3000 grains of distilled water, giving rise to a very distinct white cloud. Finally, he proposed to render sea water drinkable by distillation. He remarks, however, that it then retained a disagreeable taste, which he attributed to the presence of a bitumen.

He likewise studied mineral waters and their analysis.† He studied their density by comparing it with that of distilled water, and gave a table of this density for the principal mineral waters then known in England. With regard to their analysis, it appears to us very curious to know the reagents which science possessed two centuries ago. Boyle used infusion of Brazil wood, or syrup of violets, for detecting in them the presence of acids or alkalies; tincture of nut galls enabled him to discover the salts of iron, and ammonia, the salts of copper; the solution of nitrate of silver detected traces of common salt; spirit of wine (carbonate of ammonia), oil of tartar and corrosive sublimate indicated the presence of arsenic, and *fuming liquor* (hydrosulphate of ammonia) showed the existence of various metals. Finally, he announced that they contained organic matters and living beings, which could be detected by means of the microscope.

Boyle had not well-defined ideas concerning the nature of the metals. He thought that they were composed of an universal matter, and at most of two or three elements which, combined in another order or in various proportions, might change their physical properties,

* Tracts consisting of observations about the saltiness of the sea. London: 1674. Tracts about the bottom of the sea. Oxford: 1670.

† Short Memoirs for the natural experimental history of Mineral Waters. London: 1685.

weight, form and structure. It is evident that he was not very far from admitting the possibility of transmutation.

It is not without interest to the history of the science, to determine the part which Boyle may have taken in one of the most important chemical discoveries of the seventeenth century, that of phosphorus. The following is, in a few words, the history of this discovery.

The name of pyrophorous (fire bearer) had long since been given to bodies capable of giving light in the dark, as the glow-worm, touch wood, the diamond, &c. Since the year 1667, Boyle had been occupied with this subject, and had studied the phenomena of certain phosphorescences in the air and *in vacuo*. At the commencement of the seventeenth century, an alchemist of Bologna had discovered this property in a stone very common in the environs of that town, the sulphate of baryta, which he had calcined with charcoal and white of egg, and consequently reduced to the state of sulphuret of barium. This product, exposed for some time to the rays of the sun, became, indeed, phosphorescent in the dark.

Some years later, a Saxon, named Baldwin, likewise devoted to alchemical pursuits, having calcined chalk with spirit of nitre, discovered in this product the same property, after it had been exposed to the solar rays. Kunckel, a learned chemist of Dresden, to whom Baldwin had said a few words concerning his discovery, without revealing to him the secret, discovered in his turn, in calcined nitrate of lime the artificial *phosphorus*, for it is to Baldwin that we owe this name, which signifies *light-bearer*.

Shortly afterwards, Kunckel, having been to Hamburg, showed to a friend, a chemist, the new phosphorescent product, and learned from him that an empiric of that town, named Brandt, had discovered an analogous substance, possessed of even more remarkable properties. He went to see Brandt, who after having shown him his product, would not tell him anything relative to its preparation. Kunckel wrote on this subject to Krafft, another chemist of Dresden. The latter, without replying to him, went to Hamburg, put himself in communication with Brandt, bought his secret without giving any share to Kunckel, set out for England, where he rapidly made a kind of fortune, by showing and selling his phosphorus as an object of curiosity. However, Kunckel was not beaten. He knew that Brandt had made researches on urine; he suspected that this was the source from which he derived his phosphorus, and, setting to work, he very soon discovered in his turn the secret which had been concealed from him.

Boyle, on his part, having seen the new product in the hands of Krafft, and not having been able to obtain a knowledge of its preparation, had the same idea as Kunckel, and after numerous attempts, ultimately obtained the same results. Sometime afterwards, he published his first memoir on phosphori.* This work comprised all phosphorescent matters. Boyle divided them into natural and artificial.

* The aërial noctiluca, or some new phenomena and process of a fictitious self-shining substance. London: 1680.

He ranked among the first, the glow-worm, touch wood, phosphorescent fishes, &c. The second comprising Bologna stone, Baldwin's phosphorus, and he placed in a subdivision phosphorus, properly so-called, that of Brandt, Kunckel, and his own, which shines in the dark, without requiring, like the others, to be exposed to the solar rays.

What Boyle called *ærial noctiluca*, was the carburetted hydrogen, which he obtained by acting with spirit of nitre on alcohol; a sort of nitric ether, capable of inflaming by contact with a lighted taper. With regard to the true phosphorus, which he prepared by distilling concentrated urine with three times its weight of white sand; he describes its physical and chemical properties very well; he discovered the gas to which it gives rise (phosphuretted hydrogen), and which inflames spontaneously in the air; finally, he formed with phosphorus and sulphur a mixture capable of exploding with the slightest blow. This was evidently the first origin of what are now so well known as lucifer matches.

This singular circumstance of the almost simultaneous discovery of phosphorus by Brandt, Kunckel and Boyle, was almost identically repeated in the eighteenth century, with regard to oxygen, which predicted or foreseen at various periods by Cardan, Jean Rey, and Robert Boyle, was finally discovered, almost at the same moment, by Scheele, Priestley, Bayen, and Lavoisier.

Robert Boyle paid much attention to distillation.* He concentrated spirit of wine by distilling it over calcined tartar (carbonate of potassa) or over quick lime. He was aware that this product does not exist ready formed in the juice of the grape; that all saccharine or amylaceous fruits give alcohol by fermentation and distillation; that spirit of wine yields water in burning. He taught the art of preserving anatomical specimens in alcohol.

By the distillation of wood he obtained vinegar and alcohol, which he called *inflammable or adiphoretic spirit of wood*. He separated the alcohol by a second distillation, after having added lime to saturate the vinegar. The alcohol being collected, he pushed the distillation further, and obtained an acid spirit, which he called *radical vinegar* (*acetum radicum*), a name which this product has retained.

He simplified the manufacture of the mineral acids, and the preparation of aqua regia. He obtained aqua fortis by distilling a mixture of saltpetre and oil of vitriol. He demonstrated the composition of nitre. After having separated the acid by distillation, he discovered in the *caput mortuum* the presence of calcined tartar (potassa), and reproduced the nitre by synthesis, by reuniting these two products.

Boyle studied the gems and natural crystals.† He examined the causes of their transparency, their texture, and their forms. He showed that their colors are due to metallic particles, whence he concluded that the precious stones had once been in the liquid state. He ex-

* *Some Considerations touching Experimental Natural Philosophy*, &c. Oxford: 1683.

† *Essay about the Origin and Virtue of Gems*. London: 1672.

amined the general conditions of *precipitation* and had glimpses of several laws of chemical statics.* He remarked, for example, that the precipitate is often heavier than the body which remains dissolved. He accounted for precipitation by saying, that the solvent force yielded it to the weight of the precipitated bodies. He could seek its explanation only in the physical laws then admitted, for those of affinity had not then been recognised. He made frequent use of the balance in his researches.

He also considered the phenomena of coloration in a chemical point of view. He showed that the essential oil of aniseed is colored red by the action of oil of vitriol; that sugar of lead reddens essence of turpentine; and that chloride of silver is blackened by light. Boyle admitted the doctrine of *points*, which was generally adopted by the chemists of that period. He accounted for the neutralisation of saline substances, by the more or less powerful faculty which the acids possessed of penetrating into the pores of the alkalis.† This idea, which goes back to antiquity, was, moreover, very specious, and it must be admitted that we have not yet conceived a more satisfactory explanation of this phenomenon.

Boyle published a great number of industrial processes which he invented, or which he bought, often at a very high price, of certain itinerant physicists who made secrets of them. Sometimes, he gave others in exchange, as also did Van Helmont, Homberg, and Lémery. We cannot too highly praise these disinterested savants, who thus rescued chemistry from the paths of darkness, and exhibited in the light of day the first materials of the present science. It is thus that we owe to Boyle the process of engraving on metal with *aqua-fortis* which is now employed. He taught the formula for the alloy of bismuth, lead, tin, and mercury, used for silvering glass. He proposed, for silvering without mercury, a mixture of equal parts of common salt, crystals of silver dissolved in nitre, and lime, or calcined tartar, with which the metals are rubbed, after having been well scoured. It was he who first gave the composition of the ink which we now use, formed of green copperas, nut-galls, gum, and water,

The art of painting on glass, likewise owes to him some valuable improvements. He discovered, by the analysis of a piece of ancient glass, that the amalgam of gold gave to glass the color of the ruby, and that manganese colored it black, violet, or red, according to the proportions used. He ascertained and determined the best quantities of silica, potassa, and lead, for the composition of crystal. The name of Boyle remains attached to his *fuming liquor*, which he called *volatile tincture of sulphur*. It was a hydrosulphate of ammonia, which he prepared by distilling a mixture of sulphur, quick lime, and sal-ammoniac. He thus obtained a reddish liquid, which gave off white vapors, and which gave a black precipitate with the solutions of lead and silver.

* *The Mechanical Causes of Precipitation.*

† *Some Reflections upon the Hypothesis of Alkali and Acid.*

Boyle was a doctor of medicine; he could not therefore fail to make some applications of his chemical knowledge to therapeutics. He taught that it was reserved for this science to solve several problems of physiology; but he disputed the two general consequences which Van Helmont, Sylvius, and Wallis had deduced, and showed that the explanation of the vital functions by chemical laws was erroneous. He wished that researches of this kind should be elucidated by accurate experiments, and on this subject he gave judicious precepts, and at the same time, useful examples.

It was in this spirit that he studied the blood.* He proved that, whatever may be the external temperature, the heat of the blood remains uniformly between 38° and 40° C. (100° $4'$ and 104° F.); that its density varies under the influence of various causes; that black blood turns red by contact with the air; that this fluid contains common salt; that it is coagulated by the acids, alcohol, and heat. He suggested the idea that certain diseases are analogous to poisoning, in which the poisonous principle is diffused in the air. He made with Wren some toxicological experiments, by injecting poisons into the crural veins of dogs, and then administering antidotes. He studied the transfusion of blood, experiments on which were then the subject of many controversies. Finally, he studied the respiration of animals, and the part which the air performs in this physiological act.

By placing organic matters in vacuo, he observed that they were preserved better than in the air, and from this he concluded that the air likewise played an important part in fermentation. He attached much interest to this phenomenon; for he said that the knowledge of ferments and fermentation would ultimately lead to the solution of many pathological questions. He was among the first to recommend actual cautery in the bite of the viper. Most of these facts are published in the various editions of his *Considerations on the Utility of Natural Philosophy*.

We have seen that Boyle frequently accompanied Sydenham in his visits to patients: this shews that he must have applied his rare sagacity to the employment of medicines. He devoted, indeed, a special memoir to the use and properties of simple medicaments.† We find also, among his last works, one entitled, *Receipts sent to a Friend in America* (London: 1688). This was a collection of medical formulæ which had a rapid success, and which passed through several editions. He proposed several means of dissolving stones in the bladder; he analysed vesical calculi, and demonstrated the presence of calcareous salts in them. He rendered opium more active by dissolving it in spirit of wine and treating it with calcined tartar. This was, in fact, separating the meconic acid with potassa, whilst the morphia remained in the alcohol. When Talbot sold to Louis XIV. the secret of the cinchona, we are assured that Boyle made a similar

* *Memoirs for the Natural History of Human Blood*. London: 1684.

† *Discourses about the Advantages of the Use of Simple Medicines*. London: 1685.

bargain with his countryman. It is certain that he largely contributed to the diffusion of the knowledge and use of this valuable medicine.

Finally, vegetable physiology likewise attracted his attention. We have said that he studied the preservation of fruits and other alimentary substances; he also examined the part which the salts perform in the act of vegetation. He affirmed—and this is worthy of remark—that *the fertility of the soil depends upon its richness in alkaline salts*.* He proved especially the importance of carbonate of ammonia in manures; he prepared this salt by distilling wood ashes with concentrated urine.

This multitude, this variety of researches astonishes us, and makes us often desire to see so great a number of new facts serve as the basis of some general theory. It has been said that a work of details, in which generalities are wanting, resembles a tree whose branches are regarded, whilst the trunk is neglected; but this could not be applied to Robert Boyle, who only prepared and collected the materials with which the edifice of science should afterwards be constructed. However, he gave a very great impetus to theory by distinguishing mixture from combination—an enlightened idea which prepared the way for the doctrine of affinity, by separating chemical from physiological action. He also very clearly distinguished the incineration from the distillation of vegetable matters. It is from his works that we date the employment of the humid way, and of solvents in organic chemistry. The severity of the method which he applied to his researches gave to chemistry a new and original character. His example spread the taste for rational chemistry, and raised it in the estimation of serious men of science, who, hitherto, had regarded it as a fantastical art—a tissue of absurd practises, deceptions, or juggles. Cuvier goes so far as to regard Boyle as the chief of a school, and asserts that it is only in his writings, and those of Stahl, that we must seek for chemical generalities during the period extending from the close of the seventeenth century to the middle of the eighteenth.

We will not go so far. We will even agree that his influence did not arrest in England the progress of the school of Paracelsus. This was probably owing to the authority of Nicholas Lefebvre, then demonstrator to the laboratory of St. James's whose influence counterbalanced that which Boyle enjoyed at the Royal Society. Be this as it may, it is certain that this doctrine subsisted in England until the advent of the theories of Beccher and Stahl. It was not until the time of Hales that this science began to take a more rational direction, precursory to the high destinies which the close of the eighteenth century reserved for it.

(To be continued.)

* This idea had previously been conceived by Bernard Palissy. (See his complete works, edition of 1854, p. 23.)

MINERALOGY.

COMPARATIVE EXAMINATION and ANALYSIS of EUDIALYTE and EUKOLITE; REDUCTION of these TWO MINERAL SUBSTANCES into ONE SPECIES. By M. A. DAMOUR.

EUDIALYTE, a mineral which derives its name from the facility with which it is dissolved by the acids, has hitherto been observed only in one locality; it was first found, 40 years ago, by Dr. Giesecke, at Kangerdluarsuk, on the west coast of Greenland. The numerous specimens since collected at the same place, have shown this substance to be associated with sodalite and arfwedsonite.

There has been found, more recently, in the zirconian syenite of Brevig, in Norway, a mineral, whose physical and chemical characters are very analogous to those of eudialyte. This new mineral was at first classed as forming a distinct species, and designated by the name of eukolite. It was afterwards analysed by Scheerer, who thought it had some relation to wohlerite.

The similarity of characters observed between eudialyte and eukolite, giving rise to the presumption that these minerals might be identical in composition, I thought it useful to submit them to a comparative examination, and to analyse them again. For the specimens of eudialyte employed in my experiments, I am indebted to the kindness of Professor Forchhammer, of Copenhagen.

It is already known that eudialyte occurs in crystals, derived from a rhombohedron of $73^{\circ} 30'$; it is also in crystalline masses, shewing sometimes a very clear double cleavage, which leads to an hexagonal prism of 120 degrees. Indices of cleavages parallel to the faces of the primitive rhombohedron, and to those of a more obtuse rhombohedron have likewise been noticed. Its color is rose or violet-red, presenting tints identical with those of some varieties of almandine garnet. Eudialyte is sometimes transparent; but ordinarily it is furrowed in every direction, and is translucid only at the edges of its thinnest portions. It scratches apatite, and is scratched by felspar. I found its density equal to 2.906.

This mineral fuses before the blowpipe into a transparent glass, of a dull green color; it is readily attacked by the acids, and is reduced by them to a thick jelly.

The eukalite of Norway occurs in small vitreous masses of a brownish-red color, presenting sometimes two very distinct cleavages which, as in eudialyte, lead to a regular hexagonal prism. This mineral also shows, in several specimens, a certain degree of transparency; but it is ordinarily in masses furrowed in every direction. Its hardness is the same as that of eudialyte; I found its density to be 3.007. In examining it by transparency in polarised light, it shows, like eudialyte, a single optical axis, with this slight difference, that the axis is positive in eudialyte, and that in eukolite, it is negative. The chemical characters of eukolite are identical with those of eudialyte.

The following are the results which I obtained as the mean of several analyses, of both eudialyte and eukolite.

Eudialyte from Greenland.

	Oxygen.		Proportions.
Silica	0.5038	0.2616	} 0.2620 6
Tantallic acid	3.0035	0.0004	
Zirconia	0.1560		
Oxide of iron	0.0637	0.0141	} 0.0775 5
Lime	0.0923	0.0262	
Oxide of manganese	0.0161	0.0036	
Soda	0.1310	0.0336	
Chlorine	0.0148		
Volatile matters	0.0125		
	0.9937		

Eukolite from Norway.

	Oxygen.		Proportions.
Silica	0.4570	0.2372	} 0.2399 6
Tantallic acid	0.0235	0.0027	
Zirconia	0.1422	0.0364	
Oxide of cerium	0.0249	0.0050	} 0.0414 1
Oxide of iron	0.0683	0.0152	
Oxide of lanthanum	0.0111	0.0016	} 0.0792 2
Lime	0.0966	0.0274	
Oxide of manganese	0.0235	0.0053	
Soda	0.1159	0.0297	
Chlorine	0.0111		
Volatile matters	0.0183		
	0.9924		

With the sole difference of a few hundredths of oxides of cerium and lanthanum, which exist in eukolite, and which I have not found in eudialyte, it is evident that these two minerals are identical in composition. If we unite the oxygen of the tantallic acid of the silica, on one hand, and that of the oxide of cerium with that of the zirconia, we find for eukolite the same proportions as M. Rammelsberg had already assigned to eudialyte; these matters may thus be represented under the same general formula.

Comptes Rendus, No. 26, December 29, 1856.

ORGANIC CHEMISTRY.

NOTE on SOLANINE, and its DERIVATIONS. By M. A. MOTTESSIER.

THE divergence of the results obtained by the different chemists who have made analyses of solanine has led us to resume the study of this alkaloid, and to give here the result of an investigation which we

made at the Faculty of Sciences of Montpellier in the laboratory of Professor Chancel.

Several species of the genus *Solanum* (*S. nigrum*, *S. dulcamara*, *S. tuberosum*, &c.,) furnish alkaloids, which are generally considered as identical; we have not yet been able to compare these various substances analytically, but we may affirm that in their physical characters they differ perceptibly from each other, and in the confusion of these various products might exist the cause of the different formulæ, which have been assigned to solanine. Our present paper relates to the alkaloid of the *dulcamara*. The following is the result of several analyses of this substance :—

Carbon . . .	60.73	60.73	60.91	
Hydrogen . . .	8.43	8.39	8.69	
Nitrogen . . .	„	„	„	3.61

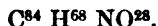
These numbers enable us to establish the following formula :—



which agrees very well with calculation :—

Calculation.		Experiment.
$\text{C}^{42}=252$	61.0	60.91
$\text{H}^{35}=35$	8.5	8.69
$\text{N}=14$	3.4	3.61
$\text{O}^{14}=112$	27.1	26.79
	100.0	100.00

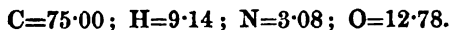
M. Blanchet has arrived at a very different formula :—



According to him, this alkaloid would contain per cent.—

Carbon	62.0
Hydrogen	8.9
Nitrogen	1.6
Oxygen	27.5
	100.0

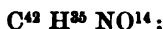
M. O. Henry has arrived at the following composition :—



It therefore became interesting to determine the equivalent of solanine, in order to establish its formula. Not having been able to arrive at this result by the analysis of the salts, which are all amorphous and gummy, we deduced it from that of its derivatives.

Ethylsolanine is obtained by heating to 120° C. (248° F.) in a sealed tube, an alcoholic solution of solanine and iodide of ethyle, and

precipitating by the product of the reaction with ammonia. Representing the formula of solanine by—



that of ethysolanine would be—



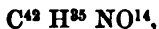
Adopting the formula proposed by M. Blanchet, that of ethysolanine would be—



The following are the results of the analyses of ethysolanine, with regard to the theoretical data furnished by our formula, and by that of M. Blanchet:—

	Experiment.	Calculation. ($C^{46} H^{39} NO^{14}$)	Calculation. ($C^{68} H^{73} NO^{28}$)
Carbon . . .	62.26	62.58	63.12
Hydrogen . . .	8.85	8.8	8.59
Nitrogen . . .	"	3.17	1.67
Oxygen . . .	"	25.39	26.62

According to these numbers, which agree much better with our formula, we think that—



should be adopted as the formula of the alkaloid of dulcamara.

Ethysolanine is a white substance, amorphous in appearance, but observed with the microscope, it appears to be composed of a mass of tangled crystals. It is without odor, has a bitter taste, is insoluble in water, and soluble in alcohol. All the salts are very soluble and gummy; they are very poisonous, and give rise to the same symptoms as solanine.

Amylsolanine is obtained like the foregoing body, substituting iodide of amyle for iodide of ethyle. Its formula is—



Amylsolanine and its salts greatly resemble ethysolanine in their physical and chemical properties.

Ethylamylosolanine is obtained by the reaction of iodide of amyle on ethysolanine, or iodide of ethyle on amylosolanine. Its formula is



- This body greatly resembles the two former in its properties.

It would be interesting to push these substitutions further, and to see whether a third equivalent of hydrogen may be replaced by a radical; but the want of materials compelled us to suspend this work, which we intend to resume, in order to arrive at the constitution of this alkaloid.

Comptes Rendus, No. 20, Nov. 17th, 1856.

INORGANIC CHEMISTRY.

RESEARCHES on the PRODUCTION of NITRIC ACID.

By M. S. DE LUCA.

I HAVE on a previous occasion shown that by passing humid ozonised air over potassium and pure potassa, we obtain nitrate of potassa, which may be separated from the alkaline solutions by means of crystallisation. Having obtained these results, which agree with those which M. Schönbein obtained by a different process, I wished to ascertain whether the oxygen disengaged from the leaves of plants by the action of solar light, or the air which surrounds plants during vegetation, presented the characters of ozone.

I have not obtained results which agree in many trials and experiments made with leaves whether detached or not detached from several plants, or with the entire plants, or in the neighborhood of extensive vegetation; generally the litmus paper has become decolorized, but starched or ioduretted paper only takes a blue color in certain circumstances. Thus, with many of the cactus family the starched ioduretted paper does not become colored; it is sometimes colored by the action of light in the presence of the green leaves of herbaceous plants, more rarely with the leaves of rose trees, frequently in contact with or in the neighborhood of moss, very rarely in an inhabited locality.

Not being able to draw any certain conclusion from these results and as the ozonometrical paper is a very unfaithful reagent and liable to become colored in the most various conditons, I wished to try some comparative experiments upon the air surrounding a great many plants kept in a hothouse, and the free atmospheric air in a locality far removed from vegetation. For this purpose I arranged an apparatus in a hothouse in the Botanic Gardens of the Faculté de Médecine at the Luxembourg. An aspirator of 140 litres caused the air to pass through slowly during the day, first into two glass tubes full of carded cotton, then into sulphuric acid, then over potassium, and finally into dilute solutions of pure potassa. In a fortnight the potassium was changed into a syrupy solution of potassa, which gradually became less concentrated. This apparatus was worked for about six months from last April. The entire volume of air which traversed the apparatus might be reckoned at above 20,000 litres. The examination of the acid and alkaline liquors gave the following results: the sulphuric acid contained ammonia, the presence of which was easily ascertained by means of potassa and lime, from its characteristic odor and the blue coloration of reddened litmus paper. In the alkaline solutions, to the number of three, were found: in the first, the reactions of nitric acid, and there were some small crystals of nitrate, and in the other two the reactions of nitrates, but no crystals.

To test this experiment I at the same time arranged two apparatus in the court of the laboratory of the Collège de France. They consisted of, an aspirator of from 150 to 160 litres capacity; two flasks,

one containing a solution of pure potassa and the other potassium in small globules; the second, a similar aspirator, a flask containing a solution of soda and another flask with pieces of sodium. Each apparatus was supplied with a long glass tube containing carded cotton. The air passed through the cotton in the tube first, leaving in it the matters in suspension, it then passed over the sodium or potassium and finally reached the solution of potassa or soda. This apparatus worked continually in the day time. The air employed in these two experiments may be reckoned for one apparatus at 17,000 litres, for the other at 19,000 litres. During one month only I, in each apparatus, interposed between the tubes of cotton and the flasks containing the potassium or sodium, a tube with five bulbs containing dilute sulphuric acid. The following are the results of these two experiments:—

I have ascertained the presence of ammonia in the sulphuric acid of each apparatus. This ammonia evidently proceeded from the atmosphere. It was impossible to find the least trace of nitric acid in the solutions of potassa and soda and the liquors proceeding from the potassium or sodium.

These facts show that the alkaline solutions do not produce nitrates during the day with a current of air containing ammonia, when this current is far away from vegetation, and that on the contrary the air of a hothouse in which are a great number of plants of all kinds, produces nitrates with alkaline solutions, even after passing through sulphuric acid and thus deprived of ammonia. Is this because plants act like porous bodies on the elements of nitric acid contained in the atmosphere? Direct experiments made far from vegetation with porous bodies of mineral origin prove the contrary, for they do not produce nitrates.

The beautiful experiments of Messrs. Andrews confirm the opinion that ozone, far from being a peroxide of hydrogen, is only modified oxygen, capable of being estimated with the utmost precision. On the other hand the phenomena of oxidation which ozone produces are not rare, and we know how to take advantage, for chemical analysis, of ozonised essence of turpentine, of the ozone produced during the combustion of ether in contact with platinum, etc. We know likewise that urea is formed in the animal economy, and M. Béchamp has proved (see page 150) that this body may be produced artificially by the oxidation of albuminoid substances, by means of hypermanganate of potassa.

It is not improbable that the oxygen of the air introduced into the economy by the phenomenon of respiration and retained condensed or modified by the globules of the blood, in the presence of an alkaline matter, is found in it, at least in part, in the state of ozone, like oxygen dissolved in essence of turpentine, and consequently in a state to produce the same phenomena of oxidation. These views are supported by some experiments made with permanganate of potassa, the oxygen of which being disengaged by sulphuric acid presents the properties of ozone, even at a low temperature, and the latter investigations of

M. Schönbein relative to the property presented by the juice of certain champignons to transform oxygen into ozone.

If we now wish to examine these facts so as to explain the results which I have just obtained, we should be tempted to imagine that the oxygen which is disengaged from the leaves of plants by the action of light contains ozone, or that the air which surrounds plants is partly ozonised, and that this ozone, although in small quantity, produces the oxidation of the nitrogen of the air to form nitric acid in the same way that ozone artificially prepared produced nitrates with the alkalies. The question of the absorption of nitrogen by plants would consequently be reduced to a pure and simple absorption of a nitrogenous compound, such as nitrate or carbonate of ammonia, this carbonate being formed in the atmosphere and the nitrate being produced under the influence of vegetation. But the above facts are not sufficiently numerous to render them indisputable facts. They require repetition under different conditions and longer and unremitted study.

M. Montagne has undertaken the microscopical examination of the cotton, and should he find anything worth describing will acquaint the Academy with the results.

Comptes Rendus.

NOTE ON IODISED ACETATE OF LANTHANUM. M. A. DAMOUR.

EVERY one knows the property which starch possesses, of being colored blue by iodine; I have ascertained that this property likewise belongs to the sub-acetate of lanthanum, recently precipitated from a solution in acetic acid. To obtain this coloration, we operate in the following manner: oxide or carbonate of lanthanum is dissolved in an excess of acetic acid; the acid solution is diluted with much water, and is supersaturated cold with caustic ammonia. The sub-acetate of lanthanum is precipitated in semi-transparent gelatinous flocks, which remain for some time in suspension in the liquor. As soon as it begins to deposit, it is collected in a filter, washed with cold water, and when the greater part of the acetate of ammonia is removed by washing, a few pieces of iodine are projected on the surface of the matter left on the filter. A violet color at first, and then a deep blue appear almost instantaneously at the points touched by the pieces of iodine, and spread to each other throughout the mass of gelatinous hydrate. This effect of coloration may be rendered more active, by pouring alcohol over the iodine, which is then dissolved, and thus penetrates more rapidly through the gelatinous mass, which may afterwards be washed with cold water, without any danger of injuring the color.

The iodised sub-acetate of lanthanum, retains its deep blue color when dried cold; the color disappears when the matter is heated to 80° C. (176° F.) It then takes a yellowish white tint, or brownish yellow at some points, retaining a considerable proportion of water, which is disengaged only at a higher temperature. When the sub-

acetate of lanthanum, which has been dried and decolorized, is treated in a glass tube to a dull red, the matter disengages a considerable quantity of water, turns white, and is then carbonised, taking a deep black color. When it is strongly treated in contact with the air it becomes brown, and disengages the odor peculiar to the vapor of iodine, which is strongly perceived only at the moment when incandescence has just ceased. We reserve the study of the compound produced in these circumstances.

The sub-acetate of lanthanum being brought to the pasty state by incomplete dessication, is easily mixed with water, and remains in suspension in it, in a state of very fine division, so as to color the liquor of a deep indigo blue; the blue color disappears by prolonged boiling. The sub-acetate of lanthanum then resumes the whitish color which is proper to it. If iodine and a small quantity of ammonia be added to the liquor holding in suspension the decolorized salt, the blue color reappears, and blue flocks of sub-acetate of lanthanum are deposited.

Acetic, hydrochloric, nitric, and sulphuric acids, added to the liquor rendered blue by the iodised sub-acetate of lanthanum, cause the color to disappear; the same effect is produced by an excess of caustic ammonia.

To obtain the blue coloration of the sub-acetate of lanthanum by iodine, it is necessary that the oxide of lanthanum should be quite free from oxide of cerium, with which it is constantly associated in the mineral kingdom. The acetic solution of the oxide which I have employed in this operation presented a slight rose tint; it is probable that it contained a little oxide of didymium.

The sub-salts of lanthanum precipitated by ammonia from their hydrochloric, nitric, sulphuric, and hydriodic solutions are not colored blue by iodine; I have obtained this coloration only by precipitating the oxide from its acetic solution.

Alumina, yttria, and oxide of cerium, treated in the same way, are not colored blue by iodine; they take only a more or less deep yellow tint.

Should we regard this blue coloration of the sub-acetate of lanthanum as a true combination of the two substances, or as a simple diffusion of the iodine among the molecules of the gelatinous salt? This last opinion appears very probable, when we consider that the blue color is identical with that which iodine communicates to starch, and that it disappears readily by the action of a moderate heat.

Since I first made these observations, M. Henri Deville and I have undertaken a series of researches on the properties of cerium, lanthanum, didymium and their compounds; I wished that the detail of the facts which I have just mentioned should be included in our entire work; but M. Deville having desired that these observations should be published separately, and having already presented them in his lectures at the Sorbonne, as being the result of experiments made by me, I decided on communicating them as I have now done.

Comptes Rendus, No 20, Nov. 17th, 1856.

INDUSTRIAL CHEMISTRY.

THEORETICAL and PRACTICAL STUDIES ON FIXING COLORS in DYEING.

By M. F. KUHLMANN.

THE numerous facts given in my former memoir (See THE CHEMIST, Vol. iii. 1855-56, page 537) show indubitably that the fixation of colors in dyeing depends, if not entirely, at least in great measure, on a chemical action between the coloring matters, and the stuffs either in their natural state, or variously modified, either by combination with the bodies, or by a peculiar molecular arrangement of their constituent principles. To establish this proposition in an incontestable manner, as far as concerns the combination of cellulose with nitric acid, it is necessary to demonstrate clearly that this acid does not intervene in the dyeing by being set at liberty, and in this manner reacting on the coloring matters. To remove all objection in this respect, it will be sufficient to argue that the tissues nitrated to different degrees, do not lose during the dyeing, their property of being more combustible than non-nitrated tissues, in the same manner that pyroxyline loses none of its characteristic properties, while undergoing the operation of dyeing. But other circumstances are opposed to the admission of any foreign influence on the nature itself of the tissue to be dyed. Thus, I have ascertained that the pyroxylated stuffs take no more color in dyeing baths whose reaction is acid than in alkaline baths, and that the spontaneously decomposed pyroxyline attracts the colors in both cases much more energetically than natural cotton. I have put these facts beyond doubt, by dyeing natural cotton, pyroxylated cotton, nitrated cotton, and spontaneously decomposed pyroxyline, without the aid of any mordant, in an acid solution of indigo, and an alkaline solution of orchil; the characteristic properties of the vegetable fibre in its various states of combination are manifested. I will, moreover, add that pyroxyline, deprived of a portion of its nitreous principles by spontaneous decomposition, and the nitrated cotton behave in carthamus dye exactly in the same manner as with Brazil-wood, madder, &c., whereas, the gun-cotton took no color in either case. It is, therefore, evident that by its combination with a certain proportion of nitrogenous principles, cellulose resembles natural nitrogenous matters in its property of absorbing colors. There is one point on which I must insist, that we cannot absolutely establish as a principle in dyeing, that natural nitrogenous tissues, or those of animal origin have with respect to all coloring matters, a greater affinity than non-nitrogenous tissues. It is well known that wool does not take the color of carthamus as well as cotton. It is the same with nitrated cotton; I have proved that if silk treated with nitric acid, although perfectly deprived of free acid, attracts the color of the flower of carthamus more powerfully than silk in its natural state, giving a scarlet color like nitrated cotton, this property does not extend to wool. In all my previous experiments, I have always found that wool is of all

textile fabrics the least apt to acquire, by immersion in nitric acid, a greater power of absorbing coloring matters.

We must likewise avoid imagining that all artificially nitrated bodies possess the property of attracting coloring matters, or the power of aiding in fixing them on tissues. Experiments made with uric acid, nitrate of urea, and urate of potassa, have given me no result. If, with carthamum, the resistance of wool to absorbing this color results from peculiar properties, foreign from its composition, we ought also to attribute to the peculiar properties of uric acid, the fact that it does not aid in fixing colors like the nitrous compound.

I likewise had to examine, to what point simple modifications in the molecular arrangement may occasion modifications in the readiness of the threads and tissues to attract coloring matters, and to form with them a true chemical combination.

Some years ago an English dyer, Mr. Mercer published that cotton fabrics gave better results and richer colors both in printing and dyeing, if previously immersed in a concentrated solution of caustic soda.

I have confirmed the truth of this assertion by several experiments, but I must add that the results obtained were not comparable, as to the intensity of the colors, to those produced by the combined action of nitric and sulphuric acids.

To explain the phenomenon observed by Mr. Mercer, the greater depth of color produced on a fabric has been attributed to a somewhat mechanical effect, to a greater approximation of the fibres of which the tissue is composed. This opinion was easily arrived at by the examination of tissues treated with caustic alkalies; these tissues, in fact, contract every way under the influence of these alkalies. This explanation, which appeared to me a guess, when speaking of the treatment of tissues by alkalies, and of a slight augmentation in the intensity of the colors, is entirely inadmissible when the intensity of the colors is produced by other reactions, especially by that of the spontaneous decomposition of pyroxyline.

However this may be, the interesting observation of Mr. Mercer, fixed my attention all the more because other observations tend to establish that this property of the caustic alkalies is shared by other bodies.

Without attributing in an absolute manner to the cause signalled, the intensity of the colors taken by cotton fabrics, prepared by potassa or soda, we may easily admit that many modifications of the nature of textile fabrics by powerful chemical agents will give to these fabrics a greater aptitude for the absorption of colors.

With the view of giving fixed ideas on this point, I have made numerous experiments of dyeing with cotton fabrics altered by the action of various chemical agents, with or without the aid of heat; I affected cotton fabrics by means of chlorine, hydrochloric acid, and hydrofluoric acid. In dyeing, I observed no result which enables us at once to conclude that all kinds of alteration do not render more powerful the absorbant powers of cotton as regards the colors. More favorable results were obtained by the action of concentrated sulphuric and phosphoric acids. By the action of these acids, the tissues con-

tracted as with caustic alkalis, and assume a certain translucency, a circumstance that may in some respect explain their more ready penetration of the colored solutions, but in the presence of the numerous facts mentioned in this work, facts in which these effects are not produced, it would not be logical to admit, as a general rule, that a different molecular arrangement in the matter to be dyed, even when no change has taken place in its chemical composition, is the essential cause of the results observed. In this case the tissues contracted and became slightly translucent; it is evident that in these circumstances the chemical nature of the cellulose is modified, it tends to a transformation into dextrine and glucose; and even if we do not admit of a change in the composition of the cellulose, isomeric bodies sometimes have very different properties. A different molecular arrangement may cause a fresh chemical combination; and the resulting greater intensity of color in dyeing, instead of being explained by the purely physical state of the matter, such as a species of contraction of the fibres of cotton or linen, should rather be attributed to a different chemical combination. How often among organic substances, do we not find isomeric bodies which exhibit very different properties when combining with other bodies.

(To be continued.)

On the PROXIMATE COMPOSITION of LEATHER.

By M. PAYEN.

FROM the commencement of a long study on starch and cellulose, I was struck with the remarkable differences introduced into the properties of these elements of vegetable organisation by simple modifications in the state of aggregation of their particles.

From that time, I have had the idea that the animal kingdom would present analogous facts; I found a curious example of this in the dermis of an ox, and I demonstrated, before the *Société Philomathique*, the spinal properties dependent on cohesion, manifested by two combinations formed in the same tissue by the action of tannin.

The first of these is easily disaggregated, and soluble in liquor ammoniac; the other retaining its resisting fibrous texture after contact with the reagent many times repeated.

A short time ago, I resumed this study, and before completing the long work which must result from it, I ask permission of the Academy to communicate to it the first results of these researches, to submit to it the views which will guide me in the examination of interesting processes, some which are still very obscure and difficult of elucidation, to which skilful manufacturers have long since called the investigations of science.

The comparative analyses which I have made with the aid of M. Billequin, in confirming my first observations, have shown that in the skins of oxen treated by the ordinary processes of tanning, and maintained in contact with the solution of tannin of oak-bark, or in

pits, from eight and twelve months for some, to seven years for others, there remains interposed an excess of tannin, which pure water may extract by dissolving a portion of the combination which it forms with the slightly aggregated substance.*

The quantity of soluble matter thus obtained from leather which had been seven years in the pits, formed 6.469 per cent. of the tanned dermis, and gave by analysis only 1.548 per cent. of nitrogen.

We found in some leather well tanned by M. Ogereau 8.09 of soluble matter, giving in hundredths 2.8 of nitrogen; finally, a piece of ordinary Paris leather yielded to water 7.87 of matter, from which 3.98 per cent. of nitrogen was obtained. The excess of tannin seemed then less, and the quantity of the nitrogenous substances carried away by the water greater in these last two specimens than in the former, as proportionate to the duration of the tanning.

In the three specimens, the fibrous portion of the dermis resisting the action of water and ammonia formed respectively in 100 parts 58.88, (mean of two analyses, 58.76 and 59) in the seven years leather, and 46.60, and 48.80 in the other two. These quantities, which are found to be in direct ratio with the duration of the tanning, appear to depend on the gradual waste of the slightly aggregated portion of the dermis, susceptible of forming with the tannin of oak-bark a compound soluble in ammonia. We might see in it an indication of instability, which is manifested in and under different reactions.

The estimation of the nitrogen, which might show what were the proportions of tannin combined in the resisting substance of the three origins, presented the following result in hundredths.

	I.	II.	III.
Nitrogen	18.272	12.285	13.59
Ash	0.783	0.660	0.606

The differences in the quantities of nitrogen are slight, and have no relation to the duration of the tanning: it would seem to result from this, that the saturation of the existing matter by the tannin occurs long before the ordinary term assigned to tanning.† At all events, it remains certain that it is the proportion of the resisting fibrous substance, and not its degree of tanning, which causes a very considerable difference between these products, and probably a real superiority in those in which the resisting portion predominates.

After having directly determined this latter portion, and estimated the nitrogen of the entire dermis, we might obtain by the difference,

* The preliminary operations of tanning having extracted the hair and almost all the fatty matter, in order to render the composition still less complex, I removed with the epidermis a portion of the subjacent tissue, so that the greater part of the hair follicles, and sudoriferous canals are found to be eliminated, and the remaining mass of the dermis corresponded to the parts almost entirely transformable into gelatine before tanning.

† The greatest difference is observed in No. II, the analysis of which could not be repeated, from want of matter.

the proportion of the soluble part extracted by ammonia, and the amount of nitrogen contained in it, we should thus find :—

	I.	II.	III.
Resisting fibrous substance	58.88	46.60	48.80
Soluble matter	41.12	53.40	51.20
	100.00	100.00	100.00

If the matters dissolved by the ammonia were not altered during the saturation and evaporation, it should be found entire in the dried residue: it was not so, for we obtained by experiment only—

	I.	II.	III.
	18.46	29.77	28.75
Instead of	41.12	53.40	63.17
Difference or loss	22.66	23.63	34.42

This considerable loss must be a consequence of the instability of the elements of the matter dissolved, susceptible of being disengaged in the state of ammoniacal compounds, under the influence of water, ammonia and heat.

Such was in effect the conclusion to be drawn from the analysis of the dry results of the evaporation of the soluble matter compared with the total composition of the tanned dermis, and of its resisting portion.

The estimation of the nitrogen of these residues, for the three specimens, gave the following quantities of nitrogen in hundredths :—

I.	II.	III.
5.112	6.006	7.151

Thus, then, the loss of nitrogen corresponded with the loss of weight. We have seen above the importance of this loss; to prove the loss of nitrogen, it was necessary to estimate that element in the dermis, to deduct from it that contained in the fibrous part, and to calculate, by the difference, what the matter dissolved ought to contain, and finally, to compare this quantity with that really found.

The estimation of nitrogen for the dermis of specimen.

No. I, having given per cent. 12.7680

If we deduct the nitrogen of the resisting portion $58.88 \times \frac{12.768}{100} =$ 7.8145

We see that the soluble matter should have been estimated in 100 parts, 12 of nitrogen, instead of 6.469, or for the total quantity dissolved $41.12 \times \frac{12}{100} =$ 4.9535

We found by analysis that . . . $18.46 \times \frac{5.112}{100} =$ nitrogen 0.9437

The total waste is then in weight 22.66 and in nitrogen= 40.0098

In other terms, the loss of weight was nearly half, and the waste of nitrogen more than four-fifths.

For No. III. the dermis gave of nitrogen	12.420
The resisting portion	$48.8 \times \frac{13.5}{100} = 6.668$
The soluble matter should have given for the	
51.2 dissolved=	5.757
11.244 per cent. Analysis gave $28.75 \times \frac{7.151}{100} =$ nitrogen	2.056

The total loss is therefore in weight 34.42, and in
nitrogen 3.701

In other terms, the loss in weight is nearly half, and that of the nitrogen nearly two-thirds.

CONCLUSIONS.

From the facts which I have just detailed, the following conclusions may be drawn.

1. The dermis of the ox contains powerfully resistant parts, and parts endowed with more feeble aggregation and special properties.

2. The tannin during its action combines with the less aggregated parts of the dermis, and with the most resistant parts; at all events, the saturation occurs a very long time before the term assigned to a good tanning; it requires, indeed, for each of the two parts, quantities of tannin considerably less than gelatine.

3. The less aggregated parts of the dermis form with tannin a compound soluble in ammonia, altered by this solution, and which undergoes a considerable loss of nitrogen during its evaporation to dryness.

4. The effects of a long continued tanning, cause the gradual solution of the feebly aggregated parts united by the tannin, and consequently, the relative increase of the quantities of resistant fibrous matter. The product, in the latter case, should therefore be both more supple, (or less brittle) and more tenacious.

5. The friable soluble portion which remains interposed in tanned leathers is unstable; in being dissolved, it may carry away considerable proportions of the nitrogenous substance. It is thus doubtless that the less aggregated portion of the dermis is gradually removed during the long operation of tanning.

Plan of further Researches on the questions to be Solved.

Can we find a means of removing economically a portion of the friable matter by means of slightly ammoniacal liquids, as soon as the tannin has saturated the skin throughout its thickness, in order to avoid long continued tanning?

Would not the remains of leather be fit for use as manure with the addition of an ammoniacal liquor, which would greatly hasten their decomposition?

Unless I am mistaken, the slightly aggregated portions of the dermis ought, on account of their more easy alteration, to be attacked, combined, and partially dissolved, in contact with the agents which are employed in the various branches of industry exercised on skins.

Touching tanning, properly so called, what are the effects produced in this respect by lime, soda, by the ammonia of which these bases determine the formation, by dilute sulphuric and lactic acids?

With regard to the ordinary operations of chamoising, the ordinary mechanical and chemical actions in presence of oil and weak solutions of potassa, have they not the effect of dissolving the part capable of being disaggregated, of thus allowing the resisting portion to predominate, of interposing a saponaceous matter which prevents adhesion between the fibres, and of thereby rendering the skins more supple, and more spongy, at the same time leaving them sufficient tenacity?

With regard to the operations of tawing, the acidulated bath and lactic fermentation should remove a portion of the more slightly aggregated substance; the solution of the albuminous salts seems to have the effect of contracting and preserving the resisting parts, and what remains of the slightly aggregated portion; finally, the mixture of amylaceous, albuminous, and fatty substances would leave between the fibres lubricating matters, which would allow of their slipping over each other, thus completing the means of softening and preserving all kinds of leather.

It will be understood that analogous reactions, after the extraction of a small quantity of the slightly aggregated part, would leave in this kind of leather less suppleness, and more density.

Taking into account likewise, the constitution of the dermis, it would be easy to explain how in skins dried directly, or submitted to the simple preparations of parchment manufacture, the portion endowed with a slight aggregation assists in causing all the parts to adhere together, in reducing thickness, and in maintaining a certain rigidity, suitable for the special purposes of these products.

I might, relying on the first results of my experiments, interpret also some of the phenomena observed in the preparation of morocco, Russia leather and varnished leathers. But in place of the hypotheses, which must be added, I prefer to introduce the results, which are doubtless more conclusive, of the experimental researches to which these new notions have led me.

Comptes Rendus, No. 20, Nov. 17th, 1856.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

ACTION of CURARE on the NERVOUS SYSTEM.

By M. A. KOLLIKER.

EVERYONE is acquainted with M. Bernard's interesting experiments, showing that in a frog poisoned by curare the nerves no longer act on the muscles, whereas the latter retain all their ordinary irritability. As the deductions which M. Bernard draws from these experiments, namely, that the muscular irritability or contractile faculty of the muscles is independent of the nerves, have been doubted by several authors, especially by M. Eckhardt, of Giessen, because M. Bernard's

experiment does not prove that curare paralyses the nerves in the interior of the muscles, I thought it advisable to undertake some fresh researches on curare, and to endeavor to determine the action of this poison on the various parts of the nervous system.

The experiments which have best shown me the peculiar action of curare are *partial poisonings*, which no one had hitherto attempted. In some of these experiments made on frogs, I tied the two arches of the aorta, and I poisoned the animal in the head, so that the legs received none of the poison; in others, I cut the leg after having tied the artery and crural vein, leaving the ischiatic vein intact, and then poisoned the animal in the back. This last experiment in particular proves that curare does not affect the nervous fibres of the nervous trunks, but merely the nerves of the muscles themselves; for, in this experiment, these trunks (the lumbar nerves of the leg operated on) remain active for two, three, and four hours after the total paralysis of the other extremities. Moreover, it proves that curare does not act upon the nerves of sensation, nor on the spinal marrow, for it is easy to produce reflex movements in the unpoisoned leg by acting on the parts which have been subjected to the action of curare.

I would therefore say:—

- 1.—That curare does not act on the nerves of sensation.
- 2.—That it has but little effect on the spinal marrow.
- 3.—That it has scarcely any influence on the nervous trunk.
- 4.—But that it suddenly paralyses the nerves of the muscles themselves.

This last point is the principal point, and I will mention an experiment in support of it. I took a very strong frog and tied all the vessels of the gastrocnemius muscle, then I poisoned the animal with curare in the usual manner. In three minutes the animal was perfectly paralysed, with the exception of that one muscle, which showed reflex movements, and which could be set in action by irritating the lumbar nerve. I do not think that any other proofs are necessary to demonstrate that curare acts chiefly on the nerves of the muscles themselves, and it appears to me that no better proofs can be given of the irritability of the muscles than the experiments which I have just described.

Comptes Rendus.

PHYSIOLOGICAL ANALYSIS of the PROPERTIES of the MUSCULAR and NERVOUS SYSTEM by MEANS of CURARE.

By M. CLAUDE BERNARD.

M. KÖLLIKER's communication has led me to reconsider my experiments on curare, and the pathological conclusions to which the study of this singular substance has led me.

In 1844, M. Pelouze gave me some curare from New Granada, and possessing the same characters as the curare examined by MM. Bous-singault and Roulin.

In making experiments on living animals, I was struck with a new fact; I found that in animals poisoned by curare the nervous system loses, immediately after death, the property of reacting on the muscular system to produce convulsions. If, for example, we poison a frog, by placing a little dry or dissolved curare under the skin of the back, we see that the reflex movements cease entirely in proportion as the poisoning extends, and if, soon after, the frog is prepared in Galvani's method, by removing the posterior members and isolating the lumbar nerves, we obtain no contraction in the members by electrical excitation carried directly on the nerves, whereas the same excitation applied on the muscles produces violent convulsions. Not only have the trunks of the nerves lost their excitability, but the nervous fibres, as near the muscles as they can be taken, are in the same condition.

The physiological consequences which I have drawn from this experiment are, that the muscular contractibility is entirely distinct and independent of the nervous property which sets it in action; because curare permits the first to continue and annihilates the second completely. I have therefore concluded, that the question of the independence of muscular irritability, which has been discussed ever since the time of Haller, will be set at rest by means of this peculiar physiological analysis operated by curare. This experiment was described in a memoir by M. Pelouze and myself (see *THE CHEMIST*, vol. ii. 1850, 51, page 185).

In 1852, to give another proof of this physiological separation of the properties of the nervous and muscular systems, I showed that in animals poisoned by curare, the muscular irritability has not only not diminished, but has on the contrary augmented. I observed, that the muscles of frogs killed by curare, which are generally redder, remain excitable by the direct action of electricity, for a longer time than the muscles of other frogs which have not been poisoned. But as these animals may present individual differences in their muscular irritability, it is necessary, for the purpose of rendering the experiment conclusive, to make a comparative experiment on the corresponding muscles belonging to the same animal.

I obtained these conditions in the following manner:—I tied the vessels of one of the hind legs of a large frog, taking care to leave the sciatic nerve intact, after which I poisoned the animal by introducing a little curare, by an incision made in the skin of the back. In this manner all the muscular system of the frog received the influence of the curare by means of the circulation, except the muscles of the member, the vessels of which have been tied, and which may thus be considered normal muscles in relation to the others. I thus found that the muscles of the member which had not received the poison, always lost their irritability much sooner than the muscles of the poisoned limbs.

In pursuing these investigations, another new fact offered itself to my notice; I found that the limb which had not received the curare remained perfectly sensitive, and that reflex movements could be pro-

duced by pinching it. But the most remarkable thing was, that when the skin of other parts to which the poison had penetrated was excited, the reflex movements were produced only in the unpoisoned member.

It was evident that these reflex movements in the healthy member, produced by irritating the poisoned parts, must be transmitted by the nerves of sensation, which remain intact; this led me to the opinion, that curare only annihilates the properties of the nerves of motion, leaving untouched the nerves of sensation, and that if we do not obtain any reflex motions when the animal is completely poisoned, that does not prove that the animal has no sensation; but only that the nerves of motion are become everywhere unable to react on the muscles by reflex sensitive excitation, as well as under the influence of the will. The loss of motive power may proceed from one of two causes:—1st. An animal executes no movements because it is not induced to do so, and because neither the will nor any sensation transmitted by the nerves produces them; it is their immobility by reason of paralysis of the nerves of sensation; 2nd. The animal may remain motionless, although it may have the will to move, and although it is induced to do so by external causes; this is because the nerves are powerless to transmit to the muscles the motive influence; this proceeds from paralysis of the nerves of motion.

Curare acts in this last manner. We can convince ourselves of this by observing the phenomena of poisoning on the higher mammifera, dogs for instance. The members are found to refuse to obey the will long before the sensation and intelligence are lost. When a dog is called whose limbs are paralysed, we see by the motions of certain parts, such as the tail and eyes, which resist the action of the poison for the longest time, that the animal hears, although he cannot execute the motions to come to the person who speaks to him.

I have directly shown this singular property of curare of destroying the nervous properties of the nerves of motion, whilst the nerves of sensation are unaffected, by the following experiments: I make an incision in the lower part of the back of a frog, so as to isolate the lumbar nerves. I then pass below them a ligature with which I tie the whole body of the animal, except the lumbar nerves, which are already arranged. In this manner the front part of the frog does not communicate with the back part, except by means of the lumbar nerves, for the ligature compresses the aorta and all the blood vessels. I then poison the animal by placing a little curare under the skin of the back, near the head, and by degrees the half of the body above the ligature shows the poisonous effects, and becomes immoveable. But if we pinch the skin of this part, movements are immediately produced in the back part, which sometimes are so violent that the frog jumps, pushing before it the anterior portion of its body, which is poisoned and inert.

From this second series of experiments, I conclude that curare produces a physiological analysis which is not confined to isolating the properties of the nerves of motion and sensation, because we find that it

preserves the properties of the nerves of sensation, and annihilates those of the nerves of motion. Curare acts on the motive nervous system of the life of relation more rapidly than on the nervous system of the organic or sympathetic life. But it destroys the latter likewise, when the poisoning is complete. I have finally ascertained that this action of curare is exerted on the motor nerves, so as to annihilate them by proceeding from the periphery to the centre, which is the inverse of the ordinary paralysis of these nerves.

In the lectures which I have given in the Collège de France, which are now in the press, on the *effect of poisonous and medicinal substances*, I have fully developed all these remarkable properties of curare. The experiments which I now mention, as well as the conclusions which I have deduced from them, are well known throughout France from the accounts in the journals, and from the persons who have attended my courses, or frequented my laboratory.

M. Kölliker's experiments consequently agree exactly with mine. It is evident that M. Kölliker was not aware of my latter experiments, which renders their similarity another proof of their accuracy.

I would, in conclusion, call the attention of physiologists to this species of physiological analysis of the organic systems, which may be produced by poisonous agents. I have developed this idea in my course at the Collège, in which I have studied with the same view several other substances, such as strychnine, sulphocyanide of potassium, oxide of carbon, &c., which act, not on organs, but on organic systems, such, for example, as the motive or sensitive nervous system, the muscular system, the globules of the blood, &c. These substances thus considered, are true reagents of the life, which being carried into all parts of the organisation by the torrent of the circulation, exert their action on certain tissues, isolate them and produce death by a mechanism which shows the physiological rôle of the tissue attacked. With these agents we can study, not the death of organs, as Bichat had done, but the death of organic systems. This study would be most interesting as regards general physiology.

Comptes Rendus.

On the PRESENCE of FLUORINE in the BLOOD.

By M. J. NICKLES.

I HAVE been lately able to ascertain the truth of the much contested fact, that there is *fluorine in the bones*. Having proved this, I proceeded to seek for fluorine in the blood; the only means by which it can reach the osseous tissue. I have found it in considerable proportions, not only in human blood, but likewise in that of other mammifera (pigs, sheep, cattle, dogs,) and of many birds (turkeys, ducks, geese, fowls).

These accordant results appear to give to fluorine an importance which it has not hitherto had either in medicine or physiology; they decidedly disprove the opinion of Berzelius, that the presence of fluorine in the bones is quite accidental, and is never necessary.

If other proofs were required of the necessity of revising this judgment of that illustrious chemist, we should find them in the following facts; there is fluorine in the bile, in the albumen of the egg, in gelatine, in the saliva, in the urine, in the hair; it is in the hair of animals (bull, cow and calf); in one word, the entire animal organism is penetrated with fluorine; we may, therefore, expect to find it in the liquids which traverse it.

I shall, in a very short time, publish the very simple processes by means of which I have found fluorine in all these matters.

Comptes Rendus.

EXPERIMENTS upon DIGESTION.

By FRANCIS G. SMITH, M.D., Professor of the Institutes of Medicine in the Medical Department of Pennsylvania College.

THE next question to be decided was, in regard to the influence of the gastric juice upon various alimentary substances, to wit: *albuminous, amylaceous, and oleaginous.*

It is very commonly accepted among physiologists, that the main business of the gastric juice is to act as the solvent of the albuminous, or nitrogenised articles of food, all the other alimentary principles either undergoing no change, or at farthest only a mechanical subdivision. It was reserved for Mialhe to show, that whatever albuminous aliment was presented to the solvent, it not only underwent solution, but was converted into a thoroughly new substance, a low form of albumen, and named by him *albuminose*, or, as Lehmann has proposed to call it, *Peptone*. The formation of peptone depends solely on the action of the gastric juice, and occurs without the evolution or absorption of any gas, and without the production of any secondary substance.

Whatever article may have been selected for the production of peptone, the following properties may always be recognised. When reduced to the solid form by careful evaporation, peptone is a white or yellowish-white substance; almost tasteless and inodorous; very soluble in water; but insoluble in alcohol of 83 per cent. Its watery solution reddens litmus, and is precipitated by chlorine, tannic acid and metallic salts, but is unaffected by boiling, by acids, or by alkalies. No precipitation or turbidity is produced by the addition of mineral or organic acids, either in a concentrated or in a very dilute state; even chromic acid fails to produce any appreciable effect.

The *albuminose*, or *peptone*, thus formed by the action of the gastric juice, is not only soluble independently of the gastric juice, but is also capable of absorption, a circumstance which does not obtain in regard to albuminous substances generally, which penetrate animal membranes with difficulty, their diffusion equivalent, according to Graham, being exceedingly low, sugar being $8\frac{1}{2}$ times and kitchen salt 19 times greater than albumen. On this lies the ground for the observation, that protein compounds, apparently all ready for use as nutriment, require a

change by the digestive fluids previous to absorption. Without the gastric and intestinal juices, even soluble albumen and caseine are taken up in far too little quantities to suffice for the nourishment of the frame.*

Ex. 10. As the representative of the *albuminous* class, four ounces of rarely done beefsteak were given to St. Martin at 10 A. M., May 5th, after a light breakfast of bread and coffee at 6 A. M., of the same day. No fluid was allowed to be taken in connection with the beef, nor any other article of food.

At 12 M., of the same day, St. Martin was subjected to examination. On pushing back the fold of mucous membrane which acts as a valve to the fistulous orifice, a considerable amount of fluid was readily distinguishable in the stomach, mixed with bubbles of air; but no solid matter was visible. About a fluid ounce and a half of this fluid was withdrawn from the stomach by a catheter, with the effect of producing nausea, which precluded the possibility of obtaining more.

The fluid presented the same reaction as all the others experimented upon; s. g. 1009. Numerous flocculi were visible to the naked eye, looking like the debris of food and mucus. It was almost entirely inodorous, viscid, and to the taste decidedly acid.

The microscope, as in the other cases, revealed amorphous granular matter, mucous corpuscles, granular cells, and a few epithelial cells; a few transversely striated muscular fibres were also beautifully displayed, some almost uninjured, some broken down and with the sarcoous elements liberated. Numerous oil globules were also distinctly visible, and a few fibres of yellow elastic tissue. The bulk of the material consumed as food had undergone entire solution, and had wholly lost its characteristic appearance.

A portion of the supernatant fluid was boiled actively, without, however, presenting the slightest trace of coagulation. The mineral acids had no effect upon it while cold, but when boiled with strong hydrochloric acid, the purple color of the protein bodies was distinctly manifested. The addition of acetic acid rendered the fluid rather more clear than before. The action of alkalies upon the fluid was not tried; but Trommer's test gave no evidence of the presence of glucose.

A portion of the dissolved material had doubtless been absorbed, but the quantity that still remained, over and above that withdrawn, was not the least remarkable circumstance in connection with the observation. All recent observers agree in stating, that the quantity of gastric fluid poured into the stomach is many times greater than that of the substance to be dissolved; this fluid, however, it must be remembered, is rapidly absorbed again, taking into the blood with it those alimentary matters which it has just dissolved and converted into peptone.

The conclusion from this observation is, that *the gastric juice is a true solvent for animal food.*

The *oleaginous* class of aliments was but imperfectly represented by the fat more or less commingled with the muscular fibres of the food

* Lehmann, quoted by Chambers, "Digestion and its Derangement."

administered as above. The number of oil globules visible under the microscope seems to confirm the observation of Bernard, that fatty matters undergo no change in the stomach beyond that of disaggregation.

Opinions are more divided as regards the influence of the gastric juice upon the *amylaceous*, or starchy articles of food. It is stated by Mialhe,* that this class of alimentary principles is converted into glucose by the digestive process, and that the saliva is the agent concerned, the conversion being continued even after the starch has descended into the stomach. In this opinion he is sustained by Lehmann, who asserts that grape sugar may be detected in the stomach in fifteen minutes after swallowing balls of starch, or after their introduction through fistulous orifices. † Carpenter also states, that the conversion of starch into sugar may go on in the stomach, but he quotes Frerichs as authority for the belief that it depends on the action of the saliva swallowed, the change not taking place when the œsophagus was tied so as to prevent the deglutition of that secretion. (*Human Physiology*.)

Bernard, on the other hand, denies that any such conversion takes place in the stomach, asserting that the acidity of the gastric juice arrests it, if it have already commenced in the mouth during mastication, or prevents it if it be introduced through a fistulous orifice. ‡ Dalton holds the same opinion, which he maintains by experiments with gastric juice obtained from dogs, and by the introduction of starch into the stomachs of these animals through fistulous orifices. § Both these latter observers, however, state that starch when acted upon by saliva out of the body, is converted into glucose, provided the saliva retain its alkalinity, but that the admixture of gastric juice arrests or prevents it if mixed with it. The following experiments may throw some light upon the subject, the amylaceous materials being represented by wheaten bread.

Ex. 11. On the sixth of May, a portion of wheaten bread was given to St. Martin while fasting, which he masticated deliberately and swallowed. In two hours and a half afterwards a portion of the contents of the stomach was removed for examination. The reaction of this fluid was acid, and the microscopic appearances have been detailed already. (*Ex. 1st.*) Suffice it to say, that in addition to epithelial and mucous cells, starch granules, some whole, some broken, were distinctly recognisable.

After allowing the fluid to stand until it had settled, a portion of the supernatant liquid was tested with iodide of potassium and nitric acid, with the effect of manifesting decided evidence of the presence of starch by the production of the characteristic blue color. The same reaction was produced with the tinct. of iodine.

Another portion of the same fluid was subjected to Trommer's test; (solution of sulphate of copper and liquor potassæ;) the result showed

* *Mémoire sur la Digestion*, &c.

† Dalton on Gastric Juice, in *Amer. Journ. Med. Sci.*, Oct., 1854.

‡ MS. Notes of Bernard's Lectures, also Donaldson on Bernard's recent discoveries, *Am. Jour. Med. Sc.*, Oct., 1851.

§ *Am. Jour. Med. Sc.*, Oct., 1854.

the brick-dust red precipitate from the reduction of the oxide of copper, in very considerable quantity.

It may be objected to the above experiments, that dextrine or other organic matters of the fluid were instrumental in producing the reduction of the oxide of copper. ("Cette réaction est une oxydation également commune au sucre, à la dextrine, à la gomme, et à l'alcool. Il est utile d'ajouter encore que l'acide urique, l'urée, et l'albumine peuvent réduire ce réactif.")*

To meet this objection the fluid of digestion of bread was carefully filtered through animal charcoal, by which, as Bernard has shown, all matters, except glucose, are detained. The filtrate when subjected to Trommer's test, again afforded the brick-dust precipitate in large quantities.

Ex. 12. In order to ascertain, if possible, what effect the saliva might have had in producing the glucogenic change in the bread masticated, a portion of bread moistened with water was introduced through the fistulous orifice, and St. Martin was requested to swallow as little saliva as possible, which, as he used tobacco, he had little difficulty in complying with. In an hour and a half afterwards, the contents of the stomach were withdrawn. The same acid reaction was manifest, the same microscopic appearances, and the same solution of the materials were present, although not to the same degree as when the bread was masticated.

The fluid was carried through the same test with a like result, viz: faint evidences of starch and decided evidences of glucose.

Those who have read the admirable paper of Dr. J. C. Dalton, on "Gastric Juice and its Office in Digestion," † will remember that he states, that "the presence of gastric juice interferes with Trommer's test for grape sugar," and further, that in animals fed with starch, "no sugar is to be detected at any time" in the product of digestion, and still further, that if gastric juice be added to a mixture of saliva and starch, "there is no trace of sugar, even at the end of three hours, and the starch retains its usual properties." Dr. Dalton's experiments were performed upon dogs, in whom he had established fistulous communications with the stomach. It is possible that the greater acidity of their gastric juice may have prevented the metamorphic change, as the writer has witnessed in saliva acidulated with hydrochloric acid; but in the human subject, if we are to accept St. Martin as evidence, his observations have not been verified, indeed they have been disproved.‡

The *conclusions* from the foregoing experiments are, that starchy materials are digested in the human stomach; that human gastric juice does not prevent the conversion of starch into grape sugar; and that this conversion may take place in the stomach, independently of the

* Bernard, Leçons de Physiologie Experimentale Appliquée à la Médecine, 1855.

† Amer. Jour. Med. Sc., Oct., 1854.

‡ It may be mentioned in passing, that cane sugar was also converted into grape sugar in St. Martin's stomach. In the product of digestion of calves' feet jelly, glucose was distinctly recognisable. The gelatine was unchanged.

action of the saliva, for, as Bernard has shown, any mucous membrane and some alkaline fluids, as the serum of the blood, possess the same power.

In presenting these experiments to those interested in such matters, it is not claimed for them that they are decisive; they are frankly offered as a contribution towards settling a "*quæstio vexata*." Of their value, others must judge. Their prosecution was a subject of deep interest to the writer, whose chiefest regret in relation to them is, that the opportunity was too fleeting to enable him to obtain more decided and extensive results.

Philadelphia Medical Examiner.

PHYSICS.

On the ELECTRICAL PROPERTIES of TOURMALINES.

By M. J. M. GAUGAIN.

NUMEROUS studies have been made on the electrical properties of tourmalines; but the physicists who have examined this subject, have generally operated on isolated crystals (not communicating with the earth), and the result has been that they have only shown a very small portion of the electricity which tourmalines are capable of developing.

It is well known that the tourmaline becomes electrical when cooled, or heated between certain degrees of temperature; but I have in the first place examined only the electricity developed during cooling. The mode of experimentation which I adopted was very simple; I suspended to two isolating supports the tourmaline which I intended studying, by means of two fine platinum or copper wires twisted over the ends of the crystal; then having put one of these wires in communication with an ordinary electroscope of gold leaf, and the other wire in communication with the earth, I heated the tourmaline in any convenient manner, and left it to cool in the open air. If the crystal experimented on had been brought to a very high temperature, the gold leaves remained motionless for a few moments; but as soon as the temperature of the tourmaline had fallen below a certain point, they began to diverge, separate more and more, and at length reach the metallic rods intended to discharge them, then having been deprived of the electricity which they contained, they fall back into their vertical position, to diverge anew. This movement continues until the cooling is complete, and the number of discharges effected enable us to measure with great accuracy the quantity of electricity developed in determined conditions. If we repeat this experiment after removing the communication between the ground and the tourmaline, the leaves of gold do not move, or if they diverge at all, their angle will not be more than five or six degrees.

M. Becquerel, in one of his memoirs on pyro-electricity, expressed the opinion that the tourmaline does not give out electricity, nor derive it from surrounding objects, and he founds this view upon the fact that he was unable to charge an excellent Volta's condenser by means of a tourmaline which he heated. We shall not, indeed, suc-

ceed in charging with any considerable quantity of electricity even an excellent condenser, if we confine ourselves to putting one of the poles of the tourmaline in connection with one of the plates of the condenser; but we should easily charge, not only a Volta's condenser, but even a condenser with an isolating plate of glass, when the two poles are put in connection with the two plates; we can even, by means of a combination which I am about to explain, obtain such powerful charges as to produce sparks of two and three millimetres in length.

When we operate on an isolated tourmaline we find perceptible electricity only at the two poles, all the middle region of the crystal appears to be in the natural state; but things are very different when a communication is established between the tourmaline and the earth. If a tourmaline is put in connection with an electroscope, by means of a metallic wire twisted round the middle of the crystal, no electricity will be perceived as it cools, if the tourmaline remains isolated; but on touching either of the poles, the electroscope may be charged either with vitreous or resinous electricity.

The analogy which exists between a tourmaline when cooling and a thermo-electrical battery, has naturally led me to attempt the formation of tourmaline batteries. I first connected a certain number of tourmalines, joining them end to end by their contrary poles, so as to form what is termed a tension battery; I found that this species of battery produces about the same quantity of electricity as one of the elements composing it, whereas these elements, taken separately, will themselves furnish equal quantities of electricity, but that when we associate tourmalines whose electrical properties differ very much, the battery gives less electricity than the most powerful of its elements.

In the second place I formed *batteries of quantity* by uniting a certain number of tourmalines by the poles of the same name: in this manner I obtained a very considerable augmentation of effects. When the number of tourmalines combined is only three or four, the quantity of electricity developed by the battery is clearly equal to the sum of the quantities which would be furnished by the separate elements. When we combine a more considerable quantity of tourmalines, the quantity of electricity continues to increase; but it ceases to be equal to the sum of the quantities which the separate elements would produce, and goes farther from that sum as the number of elements employed is augmented.

As the result of the association of several short tourmalines may be considered a long tourmaline, it appears from observations on tension batteries that the length of the crystal would be without influence on the quantity of electricity produced; this conclusion has not been justified by direct experiment. I broke four samples of tourmalines belonging to different varieties, and I always found that the fragments of a crystal developed less electricity than the entire crystal. Moreover, I ascertained that when the fragments of a crystal were of unequal size, the longest gave the most electricity; consequently, it appears to me certain that the amount of electricity developed increases with the length of the crystals.

The quantity of electricity developed by a tourmaline, or a battery of tourmalines, does not depend solely on the variation of temperature to which it is subjected. I have ascertained by numerous observations that the quantity of electricity corresponding to a cooling of n degrees may vary between very extensive limits, according to the rapidity of the cooling.

It has been said that brown tourmalines are the most electrical; on the contrary, the green or blue ones from Brazil are those which, all other things being equal, have given me the far greatest quantities of electricity, and I have generally found that, in the samples of the same tint, the most limpid and most voluminous were those which gave the most marked effects.

I have made some experiments with the view of appreciating the conductivity of tourmalines at various temperatures, and I have found that this conductivity, which is nearly *nil* at the ordinary temperature, increases rapidly as the temperature rises. When a tourmaline is heated to such an extent that it no longer gives signs of electricity, its conductivity is so great that it will instantly discharge a previously charged electroscope, when employed as intermediary to establish a communication between the electroscope and the earth; this fact explains, in a very natural manner, the disappearance of electrical signs at high temperatures. In fact, supposing that at a high temperature, cooling continued to develop electricity, it is clear that this electricity would be no longer manifested by the electroscope, as it would immediately escape into the earth.

I have endeavored to compare the quantity of electricity resulting from heating n degrees, with that proceeding from an equal cooling; but this comparison offering very great difficulties, I propose entering on it in another note.

Comptes Rendus.

PUBLIC HEALTH.

SANITARY IMPROVEMENTS in the MANUFACTURE of SEVERAL CHEMICAL PRODUCTS. By M. KUHLMANN.

BEING the proprietor of several manufactories of artificial soda, all situated in very fertile spots, and one of which is in the vicinity of a large town, it was, in a peculiar manner, my duty to lessen, as far as possible, the injuries to the neighborhood inherent to this species of manufacture.

Having established by numerous and direct experiments, first, that the vapors of water charged with hydrochloric acid, when cooled gradually, condense first the acid until the mixture is composed of steam and acid, giving by complete condensation an acid of 30° Baumé; second, that when the vapors contain a larger proportion of hydrochloric acid, an acid at 30° Baumé is condensed first by the gradual lowering of the temperature, and that the acid continues still longer; I thought that all the acid might be retained by a careful condensation of the vapors escaping from our apparatus, by causing these vapors to

pass into condensers, subjecting them, with an excess of steam, to a gradual cooling before they reach the chimney.

I had grounded these trials on the principle on which rests the rectification of alcohol, in which the mixture of steam and alcoholic vapors is subjected to a gradual cooling, intended to separate the steam first, as being the more easily condensable body.

This system, although favorable, only gave results which were insufficient to destroy entirely the acid emanations from the apparatus in which the salt was decomposed. The gases heated by the injection of steam were condensed too slowly, and a considerable portion still passed through the chimney.

I then proceeded in a different way, and I think that I have completely solved the problem, by making this condensation of the acid vapors, the basis of a manufacture, on the large scale, of barytic salts.

To complete the condensation of the acid vapors, I place after the ordinary condensation apparatus, large earthen vessels containing native carbonate of baryta, in large pieces, and constantly moistened with a small stream of water.

The acid vapors or gases charged with acid, after leaving the condensation apparatus, traverse five or six of these vessels or cascades, in which the whole of the uncondensed hydrochloric acid is retained under the form of a solution of chloride of barium, whose density may be brought to 25° Baumé in the vessels themselves.

I have in this manner so much improved my working, that the same apparatus which with 100 kilos. of common salt used to give me 122 to 125 0/0 of hydrochloric acid at 22°, now gives me 156 0/0, reckoning the quantity fixed in the state of chloride of barium.

In my factory at Amiens, where every precaution with a sanitary view required peculiar attention, because of the proximity of a large town, I have completed my system of condensation by causing the vapors, when passing from the apparatus of barytic condensation, before entering the chimney, to pass through a large earthen cylinder, containing a wheel with flat arms, whose motion causes the vapors to traverse a fine rain composed of a clear mixture of carbonate of baryta and water. Certain arrangements permit the renewal of the mixture of carbonate of baryta, and the removal of the chloride of barium formed. I still have to find a use for large quantities of chloride of barium.

In 1841, when I proposed the employment of alkaline carbonates to prevent the incrustation of steam boilers, to be filled with water charged with carbonate of lime dissolved by means of carbonic acid, I added when concluding my publication, "For selenious waters and sea waters, it would be preferable to introduce chloride of barium than to use carbonate of soda. This chloride might be manufactured very economically, if any large use for it could be found."

I think that my method of condensation of hydrochloric acid, will bring chloride of barium into the market at a very moderate price. At present I sell it at 40f. the 100 kilos.

But the above will not form the great use for the chloride of barium which I manufacture. This salt, well separated from iron by a little lime, is put in contact, while in solution, with sulphuric acid in lead

chambers, in sufficient proportion to precipitate the baryta, forming artificial sulphate of baryta, and the hydrochloric acid is thus regenerated in a sufficiently pure state, if the operations are well performed, to be employed for all the usual purposes of commercial hydrochloric acid. The acid thus produced marks 6°; it is directly applicable to the manufacture of gelatine from bones, and, if required, I replace it in the condensation apparatus. The great insolubility of sulphate of baryta renders this use very easy, as the least trace of sulphuric acid in excess, is a perfect guarantee against the existence of the poisonous salt in the regenerated acid.

In my workshops, the free muriatic acid, reconstituted by the displacement of the baryta by means of sulphuric acid, is generally employed to decompose carbonate of baryta to meet the increased demand for artificial sulphate of baryta, whether in the state of paste or dry. This product, which is known by the name of fixed white, is used for the manufacture of stained papers. In consequence of its extreme state of division, its mixture with white lead gives better results for painting, than pulverised natural sulphate of baryta. I have likewise found that this salt, mixed with zinc white, forms the best basis for my siliceous paint.

I have not confined myself to condensing hydrochloric acid by the above described process; I have also applied the same means for the condensation of the acid vapors which escape from the leaden chambers. For this purpose it is necessary to use whiterite (native carbonate of baryta) as pure as possible, for in the first absorption vessels sulphate of baryta is directly formed, which would be injured by using whiterite charged with silica, and principally of sulphuret of lead.

When I have no sufficiently pure whiterite, I introduce into the first cascades a solution of nitrate of baryta, proceeding from the condensation performed in the last vessels, into which the nitrous vapors have reached, already free from sulphuric acid. I have also successfully used an earthen stirrer in the first vessels, in which the barytic condensation is performed.

The condensation of the hydrochloric acid is facilitated by a current of steam, injected into the various condensing apparatus; but this precaution is not necessary to arrive at sufficiently satisfactory results. It is not the same with the vapors from the leaden chambers; then a slight injection of steam into the first vessels for barytic condensation is very useful, doubtless producing a more rapid decomposition of the hyponitric acid.

The nitrate of baryta in solution which results from this condensation, marks from 5° to 6° Baumé; its concentration in leaden vessels gives us this product in beautiful crystals, and the calcination, in earthen cylinders, of these crystals after dessiccation, enables us to prepare baryta on a manufacturing scale and at very little expense, by taking care to catch the nitrous vapors and oxygen which result from this decomposition in leaden chambers, in which the elements of the nitric acid of the nitrate decomposed replace exactly an equal quantity of nitric acid which had been used, or the product of the decomposition of an equivalent quantity of nitrate of soda.

The only obstacle which presents itself to the general adoption of my method of condensation, is the high price of the natural carbonate of baryta, or rather the difficulty of procuring sufficient quantities of the salt.

This being the fact, I shall resume a work already undertaken on a large scale in my factories and in some sugar works, at the time when the extraction of sugar from molasses by the baryta process, presented some chance of development. This is the transformation of native sulphate of baryta into sulphuret, and the treatment of that sulphuret by the carbonic acid proceeding from the combustion of coke. All was prepared at my factory at Loos, to join to this operation the combustion in leaden chambers of the hydrosulphuric acid disengaged, when an act of the Legislature caused the abandonment of the extraction of crystallisable sugar from molasses by means of baryta.

In the circumstances of which I write, it is indispensable to utilise the hydrosulphuric acid, whether by forming carbonate of baryta as an intermediate product, or by the direct absorption of the acid vapors with sulphuret of barium; otherwise we should only displace them, and aggravate the unhealthy state of the factory. Doubtless, some practical difficulties will be met with, in the arrangement of the apparatus and working it; it will especially be necessary to avoid the danger of explosion; but all these difficulties may very readily be surmounted.

I have also applied a system of condensation to the chemical reaction in my manufacture of animal black. The vapors on leaving the ovens in which the bones are calcined, containing the ammonia produced by the calcination mixed with the *burnt air* of the furnaces, are directed first into a large stone trough, in which moves an iron mill with wings. The trough is covered by a semi-cylindrical cover of metal. While the vapors pass freely through this cylinder towards the chimney, the mill throws into the space occupied by the gases a shower of fine drops of a solution of chloride of manganese, the residue of the manufacture of chlorine.

I thus succeed in condensing in the state of muriate, not only the ammonia produced by the calcination of the bones, but likewise that given by the combustion of the coal. The liquid obtained contains carbonate and sulphuret of manganese, coal soot, &c.; it is easily used in the preparation of sal-ammoniac, and may be used at once in the manufacture of artificial manures.

I have tried the adoption of this last system, with the single view of diminishing the smoke of the furnaces; my results are favorable, as far as regards the condensation of a portion of the soot; but they have not proved that the quantity of ammonia condensed would be sufficient to repay the expense, as that varies with the quality of the charcoal. This is a very important point, for the adoption of this method of condensing the smoke would be more difficult to manufacturers if it added to their expenses, in however small a degree.

In all these circumstances mechanical power may be profitable to facilitate the chemical action of bodies which react on each other chemically, and will be very useful in rendering manufactures in general more healthy. These two actions combined, by causing the conden-

sation of all the deleterious vapors enable us now to say, that our chemical manufactories will soon cease to be liable to any accusations from neighboring proprietors. There will be a gain both to manufacturing productiveness and to public health.

Journal de Pharmacie et de Chimie, November, 1856.

CHEMICAL REPORT on the MODE of DETECTING VEGETABLE SUBSTANCES MIXED with COFFEE for the PURPOSE of ADULTERATION.

By Professor GRAHAM, Dr. STENHOUSE, and Dr. D. CAMPBELL.

(Concluded from page 249.)

TABLE X.

Action of certain Chemical Reagents upon Infusions of Coffee and Chicory.

	Raw Coffee.	Roasted Coffee.	Raw Chicory.	Roasted Chicory.
Potassa	{ A bright reddish yellow liquid and no precipitate.	Brownish yellow liquid and no precipitate.	Not altered.	Not altered.
Lime-water	{ Pale yellow liquid, on standing becoming green at the surface. No precipitate.	Reddish brown, with shade of purple, and no precipitate.	Not altered.	Not altered.
Acetate of copper	{ Dirty green precipitate.	Brownish green precipitate.	Pale green precipitate.	{ Gelatinous precipitate of a reddish br. color.
Perchloride of iron	{ Deep greenish black precipitate.	Very dark greenish black precipitate.	Blackish br. colored liquid.	No effect
Nitric acid	{ Bright red colored liquid.	Clear port wine colored liquid.	No effect.	{ No effect at first—on standing, port wine col.
Sulphuric acid.	{ Dirty blackish brown colored liquid	Dark blackish brown colored liquid.	Deep blackish brown col. liquid.	Brownish black liquid.
Hydrochloric acid	{ Pale brownish yellow liquid	Port wine colored liquid.	No effect.	{ Slightly darkens the liquid.

In allowing a solution of unroasted coffee to stand, its green color becomes gradually deeper. This change is due to oxidation, and the

formation of the viridic acid of Rochleder. It is greatly promoted by the presence of an alkali; an excess of lime brings out this color very strongly, in a day or two, giving at first, however, a bright yellow color. Sub-acetate of lead gives in unroasted coffee a yellow precipitate, which does not become green. An excess of acetate of copper, gives a green precipitate in abundance, which is brightened by the addition of an alkali. This copper precipitate has been used as a green pigment. The reactions above described are, however, much altered and obscured by the roasting of the coffee, and are therefore of little service for our present purpose.

It has already been stated that iodine produces no blue coloration, in the infusion of either coffee or chicory. If the reagents named, act clearly in a different manner upon any infusion from what they do upon pure coffee, a presumption of adulteration is obtained; but the indications must be of a positive and specific nature fully to establish adulteration.

8. Coffee was submitted to the usual process of distillation with soda lime, for the determination of its nitrogen. The proportion of nitrogen per cent. was in coffee, No. 1, roasted 2.93; in No. 2, roasted 2.62; in No. 3, raw 2.53, roasted 2.70; in No. 4, raw 2.71; in No. 5, raw 2.50, roasted 2.49. The proportion of nitrogen in roasted coffee appears therefore to lie between $2\frac{1}{2}$ and 3 per cent.

The nitrogen in a specimen of foreign chicory, amounted in the raw chicory to 1.51 per cent., in the same roasted 1.42 per cent.; the same of English growth gave in the raw state 1.86 per cent. of nitrogen, and in the roasted state 1.74 per cent.

The proportion of nitrogen in coffee is therefore greater than in chicory, but the difference is not sufficiently marked to distinguish the two substances easily from each other. The conclusion may, however, be drawn, that less than 2 per cent. of nitrogen in coffee is a strong presumption of adulteration.

We may now advert to the peculiar and characteristic substances found in coffee, and the aid to be derived from them in the discovery of adulteration.

9. Professor Rochleder, who has devoted great attention to the analysis of coffee, gives the following enumeration of the substances found in the raw coffee bean, with the formulæ of their elementary composition:—

		C.	H.	O.
Woody fibre		12	10	10
Cane sugar		12	11	11
Palmitic acid		32	32	4
Fat { Oleic acid		36	34	4
Glycerine		4	8	6
		C.	H.	N. O.
Legumin		48	36	6 14
		C.	H.	O.
Cafeic acid		16	7	6
		C.	H.	N. O.
Cafeine		16	10	4 4

To these Rochleder had lately added citric acid ($C^{12}H^8O^{11} + 3H^1O$) in the small proportion of two grains in a pound of coffee, also a trace too small to be estimated by weight of viridic acid ($C^{14}H^6O^7$). This last substance is the acid obtained, by exposing solutions of the neutral and basic cafeates to the influence of the air. The green color of raw coffee is believed by Rochleder to be owing to a small quantity of viridate of lime.

The evidence upon which Rochleder rests the existence of palmitic and citric acids in coffee, does not appear to us quite decisive. The formulæ given by that chemist for both cafeic and viridic acids are doubtful.

It is also stated by Rochleder, that when dried cafeic acid is submitted to destructive distillation, a small quantity of crystals were obtained, which he considered pyrocatechine. The experiment repeated by us on a considerable scale gave a negative result.

Rochleder finds cafeic acid in paraguay tea (*ilex paraguagensis*), as well as in coffee. This statement we must also doubt. The acid of the paraguay tea has been examined by us, and found to have a certain resemblance to cafeic acid, but not to be identical with it. Free cafeic acid, when strongly heated in an open vessel, emits the peculiar odor of roasted coffee; but the acid from paraguay tea, emits a perfectly different odor when similarly treated.

The sugar of coffee may be inferred to exist in a peculiar condition, as was before hinted, from the fact that when the coffee is roasted, none of its sugar appears to be converted into grape sugar, as it does not affect Trommer's test; while, when seven grains of cane sugar were added to 100 grains of coffee, and the whole roasted in the usual way, abundant indications of grape sugar were obtained by the same test.

M. Payen gives the proportional quantities of the different substances which he finds in raw coffee, as follows:—

Cellular tissue	34
Hygroscopic water	12
Fats	10 to 13
Starch, sugar, dextrine and vegetable acids	15.5
Legumin	1
Chlorogenate of potash and caffeine	3.5
Nitrogenous portion	3.0
Free caffeine	0.8
Thick insoluble ethereal oil	0.001
Aromatic oil	0.002
Mineral constituents; potash lime, magnesia, phosphorus, sulphur, silica, and traces of chlorine	6.697
100.000	

Payen's chlorogenic acid is the same as the cafeic acid of Rochleder and Pfaff. Payen believed that he had obtained from coffee a crys-

talline double salt of this acid, containing potash and caffeine; but this observation has not been confirmed.

The proportion of fat in the coffee bean is remarkably high—being generally stated at 10 or above 10 per cent. We found at least 8·9 per cent. of fat readily extracted by ether. In chicory, the proportion of natural fat is scarcely appreciable; but it is brought up by the fat in the process of roasting the chicory.

10. Some uncertainty existing respecting the proportion of the active principle caffeine in coffee, the point was particularly enquired into. The following process was adopted:—The raw coffee was ground fine,—having been previously well dried at 212° to facilitate that operation. A decoction was then made of 1,000 grains, by the repeated application of boiling water, so as to exhaust the coffee of all soluble matter. The solution was concentrated a little by evaporation. The acid of the coffee and certain other substances were now entirely precipitated by the addition, first, of the neutral acetate of lead, and then, of the sub-acetate of lead. These insoluble matters were removed from the liquid by filtration. The excess of lead in solution was then thrown down by means of hydrosulphuric acid.

The liquid, after this preparation was evaporated to dryness and the dry matter left, was exhausted by means of strong spirit of wine (sp. gr. 0·840). The alcoholic solution was concentrated by evaporation, and allowed to stand in a nearly syrupy state for about ten days, in order to crystallise. The crystals, which are caffeine, were collected upon a small filter, and compressed powerfully to remove the mother liquor. These crystals were re-dissolved in a small quantity of water, the solution evaporated, and crystallised anew. It gave almost nothing but caffeine, in long silky white needles, with little or no color. The proportion of caffeine obtained in five experiments made upon different samples of coffee, was as follows:—

CAFEINE IN RAW COFFEE.

In Native Ceylon	0·80 per cent.
In Native Ceylon	0·80 " "
In Native Ceylon	1·01 " "
In Plantation Ceylon	0·54 " "
In Plantation Ceylon	0·83 " "

The caffeine of the wild coffee appears to exceed that of the cultivated plant; the mean proportion of caffeine in native Ceylon being 0·87 per cent., and in plantation Ceylon 0·69 per cent.

The mean average of the whole five samples is 0·80 per cent. of caffeine. Probably the actual quantity of caffeine in the coffee bean is from 0·75 to 1 per cent., allowance being made for losses in the separation.

The proportion of theine (or caffeine) in tea is considerably greater and more easily extracted. Two pounds of fine strong congou yielded 2·93 grains of theine, or 2·09 per cent.

When it is merely desired to extract caffeine from raw or roasted

coffee, without reference to quantity, the general process for the extraction of organic bases by means of ether suffices. Lime is added to the infusion of coffee, which is then evaporated to dryness upon a water bath. The extract may be divided by means of clean sand, and then agitated with ether. The caffeine crystallises as the ether evaporates; or it may be redissolved in water, and crystallised again. We believe that the caffeine, from 10 per cent. of coffee in a mixture, might be extracted in sufficient quantity for its identification, by the preceding simple process. Caffeine, when once obtained, is fully recognised by its easy sub-limitation, and also by its action with nitric acid, in which it resembles uric acid. When the solution of caffeine in nitric acid is evaporated to dryness, and exposed to ammoniacal gas, it is covered by a pink blush, like murexide.

The only other substances besides coffee, in which caffeine is known to exist are, tea, paraguay tea, a species of chocolate made from the *gaurana officinalis*, or *paullinia sorbilis*.

11. Chemists generally are disposed to refer the flavor and peculiar properties of coffee, as a beverage, more to its acid, the *cafeic acid*, particularly after that substance is modified in its properties by roasting, than to any other constituent of the seed. Rochleder considers this acid as belonging to the tannin class, of substances, and calls it *tanno-cafeic acid*. But as cafeic acid does not precipitate gelatine, it is deficient in the most characteristic quality of the tannic acids. Cafeic acid, in the present state of our knowledge, appears to be confined to the coffee plant.

We have observed a property of cafeic acid, which facilitates the detection of that substance, and consequently of coffee in a mixture. Cafeic acid appears to be analogous to kinic acid, the acid of cinchona barks, for it yields kinone, when oxidated by means of sulphuric acid, and binoxide of manganese. To observe this property the coffee is boiled with water and a little slaked lime, the infusion filtered, and evaporated down to the consistence of a syrup. The syrupy liquid is then mixed in a retort with four times its weight of binoxide of manganese, and one part of oil of vitriol, diluted with an equal bulk of water. Sufficient heat is produced by the action of the sulphuric acid upon the other materials to bring over the greater portion of the kinone, and the lamp need not be applied till towards the close of the operation. The distillate consists of yellow crystals of kinone, which usually coat the neck and sides of the retort, and a bright yellow liquid, which is a saturated aqueous solution of kinone, with a considerable quantity of formic acid; kinone is easily discernible by its volatility and peculiarly acrid odor, which greatly resembles that of chlorine. The solution of kinone gives, with ammonia, a sepia black color, becomes reddish brown with hydrosulphuric acid. It is decolorised by sulphurous acid. The beautiful green hydrokinone is obtained by exactly neutralising the solution of the yellow kinone with sulphurous acid, great care being taken not to introduce the latter in excess.

The peculiar acid of paraguay tea agrees with cafeic acid (to

which it is no doubt related), in yielding kinone to similar oxidising agencies; so does the acid of the leaves of common holly (*ilex aquifolium*) tea, and the whole of the *cinchona* tribe.

The *prune* tribe of plants, including the sloe, cherry, laurel, &c., the seeds of which yield prussic acid, all contain amygdalin, or some similar principle. Now all of these when oxidised, in the same manner as the former class, yield oil of bitter almonds, and so can be recognised.

The *willow* and *poplar* tribe, on the other hand, yield oil of spirea ulmaria (salicylic acid), a very characteristic substance.

The tests for kinone can be applied in a few minutes, and they are sufficient to indicate the presence of 10 or 12 per cent. of coffee in a mixture.

12. The root of *chicory* presents no feature of a marked nature, beyond its large proportion of sugar, and the composition of its ash, which have both been sufficiently adverted to. The proportion of fat naturally in the root is quite insignificant. In an infusion of the fresh undried root, neutral acetate of lead appears to throw down the whole acids of chicory, and the sub-acetate of lead produces no further precipitate in the liquid. But the root appears to undergo a considerable modification, by being dried at a temperature not exceeding 212°. Its infusion now gives a second precipitate, with sub-acetate of lead following the neutral acetate. Both of these precipitates can be well enough washed, but when the attempt was made to decompose either of them by means of hydrosulphuric acid, a mucilaginous liquid was obtained from which the sulphide of lead does not fall, unless with a considerable addition of alcohol. The acid precipitates appear most indeterminate, and afford nothing crystalline. A great deal of pectin-looking substance is present. Chicory also appears to possess about one-fourth of the quantity of inulin that is contained in the dahlia root, and starch in no other form; the infusion of chicory giving only a brown with iodine, and no blue. Chicory appears to contain no oxalic, malic, citric, nor any other crystallisable organic acid. The other sweet roots, beet, turnip, &c., also like chicory, present little that is tangible in their chemical properties. But the high color of the infusions of all these roots when roasted, the great density of their solutions, and their fermentability, afford sufficient means for distinguishing them from coffee, and discovering their admixture with that substance.

The properties of a great variety of other vegetable substances, which might possibly be employed in the adulteration of coffee, are exhibited in the early Tables of this Report.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On GUANO, and How to TEST it. By JOHN HORSLEY, F.C.S.

HAVING been lately engaged as chemical referee, and contributor to a new work on agriculture, shortly to be published, I have much pleasure in furnishing the farmers, landowners, and agriculturists in general, with some valuable information respecting guano, that will enable them to *protect* themselves against the host of *pseudo*-chemists and manure dealers who too often impose on them vile compounds for *genuine* guano, backed up, too, with *well-dressed* certificates and analyses of the *samples* submitted to their inspection, which, in many cases, are widely different from that which the farmer actually receives.

In nothing, perhaps, are there greater frauds committed than with guano. Therefore it is immaterial to the writer how his motives are scanned, or whether his statements are called in question—" *qui capit ille facit* "—his only object being to benefit the farmer by putting him on his guard, and initiating him into a few simple and practical secrets respecting guano, which he believes are not to be found in such a form in any work on chemistry or agriculture.

The fertilising powers of natural guano being now so well attested, it is needless for that question to be raised; suffice it to say that guano is the excrement of the Penguin and other sea birds, which congregate about the rocks and reefs of certain continents and islands. The richest guano is that known as Peruvian, and is of a light, friable nature, and yellowish brown color, with a gross, disagreeable odor, and is not pungent until it is acted on by a certain chemical agent.

The principle of natural guano consists in the existence of a little free uric acid, with uric, oxalic, and phosphoric acids in combination with ammonia and a certain portion of lime, forming urate, oxalate, and phosphate of ammonia and phosphate of lime. Perhaps one of the best modes of ascertaining this by analysis would be:—

1. To reduce to powder a small portion, say 100 grains, of the guano to be tested.

2. Put the powder on a paper filter, supported by a funnel, in a glass vessel, or in the ring of a retort stand, and pour over it a hot solution of caustic potash: then observe whether any smell of ammonia (like hartshorn) is given off,—this may be further tested by holding over the fumes, a piece of moistened turmeric paper, which will immediately become reddish-brown: allow as much of the solution of caustic potash to pass through the filter as will extract all

soluble matter, which it will to the extent of from eighty to eighty-five per cent. if the sample be good, until it becomes colorless; leaving behind, a mixture of carbonate and phosphate of lime, with a little sand and organic matter. We shall then have a dark red colored liquid of a strong pungent odor: this liquid must now be *heated* until all smell of ammonia disappears. If to this solution you add muriatic acid, or spirit of sea-salt in excess, an abundant crystalline precipitate of a reddish-brown hue will be obtained, not unlike the red gravel in man. This constitutes one of the principles of urine, and is called uric acid.

3. Next, this brown crystalline deposit may be rendered perfectly colorless by re-dissolving it in a fresh solution of caustic potash, with a small proportion of animal charcoal, gently heating it for a few minutes, filtering, and then acidifying the liquor with muriatic acid, and collecting the precipitate on a filter, and evaporating it to a perfect dryness by a gentle heat, such as we obtain from a steam bath.

4. If this *decolorised* precipitate be treated with strong nitric acid, it will be dissolved with an effervescence. This solution should now be evaporated to dryness by the heat of the water bath, when the subsequent addition of a few drops of strong ammonia water to the hot residuum, will develop a splendid purple-red color,—indicating the existence of animal matter, and forming the well known “murexide” of Liebig. A greater proof of the genuineness of guano could not be obtained by any other process: for had a strong acid, such as nitric or muriatic acid, been used in the first instance, any earthy matter which may have been mixed with the guano would have been likewise dissolved: but by using the solution of caustic potash instead, the guano only will be dissolved to the extent stated; and thus any adulterating material is sure to be detected by its *insolubility* in that menstruum.

Setting aside the question of the adulteration of guano, which is generally done with brick-dust, &c., we shall find that even the genuine sort will sometimes vary in its relative value, which shows the necessity for inquiry through the medium of a practical chemist, into the composition of any sample before purchasing, as the following instances prove.

A party obtained two samples for analysis, one from Gloucester and another from Worcester; each sample was offered as the genuine Peruvian guano, that from Gloucester being quoted at £12 10s. per ton, whilst the Worcester sample was £15 per ton.

The Gloucester sample, besides yielding the usual quantity of ammonia, &c. dissolved in *boiling water* to the extent of 73 per cent., but the Worcester kind only 55 per cent.—which goes to prove that high prices are not always a criterion of the quality. “Try before you buy,” is a most valuable motto to be observed in the purchase of guanos, whether natural or artificial. The analysis of a well known scientific chemist, will prevent much disappointment as well as loss of money in buying a comparatively worthless article in mistake for the genuine.

Analysis of a Sample of Peruvian Guano.

	per cent.
Urate of ammonia	26
Oxalate of ammonia	40
Phosphate of ammonia	12
Phosphate of lime	15
Sand and organic matter	7
	100

ANALYSIS of a SAMPLE of JAMAICA GUANO.

By JOHN HORSLEY.

	per cent.
Organic matter, humus, and vegetable mould	35
Inorganic matter, carbonate of lime with traces of phosphate of lime and sulphuret of iron	65
	100

This sample was perfectly destitute of those principles usually associated with guano, such as uric acid and ammonia, and on which their efficacy mainly depends.

The Laboratory, Cheltenham, Feb. 2, 1857.

TRANSLATIONS AND ABSTRACTS.

BIOGRAPHY.

ROBERT BOYLE, a BIOGRAPHICAL STUDY.

By M. CAP.

(Concluded from page 285.)

LET us hasten to close this long enumeration of the details of labor, to try, and take a view of the whole, and to examine the influence which they might exercise on the progress of the sciences, which were their chief object.

When first Robert Boyle commenced his scientific career, physics and chemistry were non-existent as bodies of doctrine. The scientific reform of which Galileo gave the first signal, although it had been pursued by Bacon and Descartes, and more practically by Otto de Guericke, Torricelli and Pascal, had not yet penetrated the schools. Isolated researches, without any means of general publicity, and hitherto confined in design to laboratories, as yet threw but a faint light on public instruction, and the practice of the arts. Alchemy had accustomed men of science to live and labor in solitude, and scholastic habits, to be satisfied with empty words. Their labors were performed in silence, and surrounded with mystery, while teaching was encumbered with puerile disputes. But, from the moment when

learned men resolved to commence investigation in earnest, and to render it profitable to every one, they saw the necessity of uniting their efforts, and working in open day. Such are the principal object and more elevated thoughts which presided at the foundation of academies.

Therefore, it is to the science of the sixteenth century, that we owe that great principle of association, which in following ages, and especially our own, has led to such magnificent results. But it is from the seventeenth century, and from the foundation of academies, that arose the definitive separation between the knowledge of the middle ages and modern philosophy; the first, characterised by the isolation of studious men, and the obscurity of scientific languages; the second, depending on the association of the learned, the division of the labor of research and the publicity of discoveries. Now it was Robert Boyle who made the most persevering and efficacious efforts towards the foundation of learned societies, and this is certainly one of his best titles to glory, and one of the most signal benefits for which science is indebted to him.

The Academy of the *Lincei*, founded at Rome, in the commencement of the seventeenth century by Prince Cesi, was given up towards 1630, after the death of this zealous protector of science. The Academy *del Cimento* had been just established in Florence by the pupils of Galileo (1651) when Boyle had the idea of assembling the scientific men of England at Oxford, in an analogous conference, with the object of advancing human knowledge, with the aid of the experimental method. This idea may have been inspired by what he had seen in Italy, and even in France, where the conferences of Mersenne de Monmor were in full activity when he traversed our country.* However, this may be, it was a most happy and fruitful idea which determined him to put in practice, in his *Philosophical College*, the idea which Bacon had proposed in his *New Atlantide*, under the title of *Solomon's Institute*, and we cannot too highly praise the zeal with which he gave the impulse to this Society, by the example of his own investigations.

Robert Boyle was indeed the first of the learned men of his time, who resolutely entered into the path of conscientious research, without prejudice, without the influence of any school, guided solely by obser-

* Perhaps these assemblies in Paris gave occasion to the rise of many academies over the rest of Europe. It is at any rate certain that the English gentlemen, who first laid the foundation of the Royal Society of London, had travelled in France, and had been with de Monmor and Thevenot. When they returned to England, they assembled at Oxford, and continued the consultations they had been accustomed to in France. The government of Cromwell even contributed to this establishment. These Englishmen, secretly attached to the legitimate king, and resolved not to take any part in public affairs, were glad of an occupation which gave them an opportunity of leaving London without rousing the suspicions of the Protector. The Society continued in this state until Charles II. having remounted the throne called it to London, confirmed it by Royal authority, gave it great privileges, thus recompensing those sciences to which had served as a pretext for the fidelity which had been shown him.—Fontenelle, preface to the history of the *Academie des Sciences*.

vation and reasoning. If Van Helmont is the last representative of the *à priori* method, Boyle is evidently the chief of the new school, scorning hypotheses, resolved to admit nothing but facts ascertained by experiment, and taking no account of preconceived opinions. Between these two men is the transition between the reign of words and authority, and that of things and experiment. There nevertheless exist between these two men, who occupy so important a place in scientific history, very singular analogies, which deserve to be ascertained, and which, in certain points of view, appear to us really interesting.

Robert Boyle and Van Helmont, both scions of great families, and possessors of ample fortunes, both animated by an ardent piety, benevolent, modest, and generous, renounced the advantages of their social position, and devoted themselves to retirement, to follow exclusively the career of science, into which they both entered as medical students. Although belonging to different nations, their characters, their ideas of morality, and their scientific views approximate, and may even sometimes be confounded with each other. They were both remarkable for their private virtues, and by great liberality towards science and its votaries. Let us now look at the differences, Boyle although of a lively imagination, impressionable, and inclined to the marvellous, gave himself with the greatest perseverance to the study of the exact sciences, freeing himself at once from the doctrines of former times, from the spirit of his age, and even to a certain point from his religious preconceptions. Van Helmont, on the contrary, sincerely devoted to mysticism, sought in science chiefly a basis for his private convictions. He studied ancient languages, and even Hebrew, to find in the fathers of the Church lights and arguments applicable to his scientific ideas, and *vice versa*. Boyle devoted himself to the same studies, and even to mathematics, but it was for the purpose of controlling his imagination, to give to his labors more exactness and fixedness. With an interval between them of half a century, Van Helmont still held by more than one point to the doctrines of the middle age and spagyric school; whereas, Boyle was completely separated from them; he combats their principles, he refuses their aim, he rejects their language, and admits no other guide than experiment and induction, that is to say, the experimental method.

Entirely devoted to direct research, Boyle would not at first even read the writings of Descartes, because he feared to find in them more theories than facts, more hypotheses than realities. And yet, but with a different view, Boyle and Descartes often followed a similar path. It is well known how the latter endeavored to disembarass philosophy from occult qualities, mysterious entities, astrological predictions, and the other debris of expiring scholasticism. Boyle likewise finished by admitting many principles of cartesianism, for instance, those relating to porosity, the movements of particles of fluids, and the doctrine of elements, which Descartes attacked as resolutely as himself. It required no little courage to combat thus openly the opinions so generally admitted and taught. To remove themselves from this

influence, F. Bacon, Van Helmont, and Boyle found it necessary to withdraw into complete solitude. Descartes, with the same view, quitted his country and died in a foreign land.

Boyle was one of the greatest experimentalists of the 17th century. Boerhaave considers him as the father of experimental philosophy. He adds, that he was the ornament of his age and country, and succeeded to the talent and genius of Bacon. He is one of those men, says Fourcroy, who are the most prominent in the history of science, one of the creators of experimental philosophy, and the precursor of pneumatic chemistry. To him, indeed, belong, in physics, the principal improvements in the pneumatic machine, of the barometer and thermometer, the best works on the vacuum, heat, the magnet, coloration, porosity, sonorous waves and crystallography. In chemistry, he was the first to collect gases, made the first analyses of mineral waters, sea water and urinary calculi, he discovered the action of acids and alkalies on colored vegetable juices, he suspected that water is decomposable, and that the air contains a vital portion which serves for combustion. In physiology, we owe to him the first experiments on blood, on the transfusion of that liquid, he showed the part performed by the air in the act of respiration, he made the first toxicological experiments on animals; in one word to him are due the most beautiful experiments on which are founded our present teaching, and most of the materials which have served to raise modern theories. Perhaps, no one person ever destroyed more errors, threw more light on obscure points, or prepared with more zeal for the approach of those truths which now form the basis of our scientific doctrines.

Boyle possessed all the qualities necessary for the exercise of great influence; a solid judgement, varied knowledge, a genius for experimentation, an upright, judicious, patient, and methodical mind. Being a great enemy to conjectures and hypotheses, he scarcely ventured to give the natural explanation of phenomena, and never raised his views to the height of a theory. He said, that to construct the edifice of science, two instruments would suffice: observation and intelligence. He acted as if he had felt the observation of M. Dumas: "Theories are like crutches; to prove that they are good, we must make use of them and walk."

Boyle is to our mind the model of the true scientific man, the type of the investigator, attaching no value to the mere self-satisfaction of his labors, thinking neither of cupidity nor ambition; offering, on the contrary, as a tribute to science, and with the most perfect self abnegation, his time, his fortune, his talents, and even his fame.

Every year of his life was marked, if not by a great discovery, at least by a multitude of researches, which all aided to elevate the standard of general knowledge. From 1650 until his death, that is to say, during a period of 32 years, he never passed one year without publishing either a work or several memoirs on various subjects. The *Philosophical Transactions* of London was the general repository of his works. Sometimes as many as ten of his memoirs will be found

in one volume. Bibliographers mention no less than 46 works of which he was the author.* We think, however, that he has not written all that has been attributed to him. As he kept up an active correspondence with all the learned men in Europe, he was very rapidly informed of all scientific news; he immediately communicated everything to the Royal Society, he repeated the experiments, he added his own remarks, and the whole frequently spread under his name and authority.

Robert Boyle was frequently the object of attacks, to which he replied with calmness and dignity. When, in 1665, he published his *Occasional Reflections*,† it produced numerous criticisms. Swift wrote a pamphlet which he called *Pious meditations on a broom handle, in the manner of the Honorable Robert Boyle*. Swift had well seized the weak side of this work; he appears, moreover, to have drawn from this work of Boyle's, the idea of Gulliver's travels, as Montesquieu afterwards did that of his Persian letters. In 1669 Dr. Henry Stubbe became a violent opponent of the Royal Society, Robert Boyle suffered much vexation from it, but he maintained silence, and thus saved his own dignity, and that of that learned body.

Regarded as a philosopher, Robert Boyle might present a new and grand object to the student of biography, but we have determined to consider him only as a scientific man. Still, to complete the idea which we wish to give of this extraordinary man, we must observe that his submission to the dogmas of religion was deep and sincere, that he considered them as above the reach of human reason, notwithstanding that he professed a religion that has always made human reason a test of its belief. The work which he published under the title of *The Christian Virtuoso*, had for its object to point out the difference existing between those things which are above reason, and those which are opposed to reason. These matters had occupied his mind very much during his youth, and it was for the purpose of rendering them clear to his mind, that he devoted himself to the study of the Eastern languages and the fathers of the Church. They had caused in his mind so much terror as to disturb his heart, and sometimes even his intellect. "It is a complaint which," he says "resembles toothache, without being fatal, it causes horrible sufferings." Once firm in his convictions, he used the same ardor, zeal, and perseverance in defending and propagating them as he did in the service of natural philosophy. He contributed to the establishment of foreign missionary societies. He paid for the translation and publishing of the Gospels and Acts

* A very nearly complete edition of Boyle's works was published by T. Birsch, in 5 vols. folio, London 1744. Schaw published another in 1772, 6 vols. folio. Two Latin Editions appeared at Geneva in 1680, 6 vols. quarto, and in 1714, 5 vols. quarto.

The general history of the air did not appear in London until 1692, a year after his death, as did his *Medical experiments, or a collection of choice remedies, for the most part simple and easily prepared*. This last work passed through nine editions, two of them in German.

† *Occasional reflections upon several subjects, whereto is premised a discourse about such kind of thoughts*. London, 1665.

of the Apostles in the Malay language. He did the same for the natives of Ireland, and Highlands of Scotland who did not understand English. He founded the public lectures where Samuel Clarke gave his celebrated discourses, and when Burnett published his *History of the Reformation*, "he paid all the expenses," said the author, "in a manner proportionate to the greatness of his soul."

Boyle joined to his principles of piety, pure morals, a rare degree of modesty, an active benevolence, and extreme delicacy. He would not take orders, because he did not think himself worthy to exercise those functions. Four of his brothers were members of the peerage, but he refused it for himself. He likewise refused the direction of Eton College, as well as the presidency of the Royal Society, of which he was the principal founder, and of which office certainly no one was so worthy. He at length accepted the office of chairman of the India Company, but only for the purpose of more efficaciously assisting in the propagation of Christianity among the Hindoos. Honored by the esteem of Charles II., James II., and William III., he never used his influence except for the good of science. His laboratory was the *rendezvous* of all the learned men of his time, and his house was always open to the unfortunate. His modest charity distributed favors without giving a suspicion of their source. The remainder of his fortune was employed in the construction of instruments and apparatus, trying experiments, founding institutions, and giving scientific men pecuniary assistance. He built an observatory at Oxford, as well as a laboratory, to which he invited engineers and mechanicians to aid him in his investigations. He bequeathed his collections and library to the Royal Society of London.

Boyle was tall, his constitution was delicate, his pale, grave face showed a serious, calm mind, far above vanity and ambition. His sobriety was exemplary; he ate little, and of very plain dishes. Everything around him was arranged in an orderly and methodical manner. He dressed according to the state of the thermometer. He spoke slowly and with hesitation, because he stammered a little. He was not fond of discussion, listened patiently to objections, replied to them without bitterness, never dictatorially, but he liked to propose doubts and raise questions, thus furnishing material to others for scientific debate. His temper was sweet and equable; his conversation lively and cheerful. He never mentioned the name of the Deity without pausing a moment as in respect. Nothing was more odious to him than charlatanism and falsehood. His father often said: "I am quite certain that Robert never told a lie."

Boyle died in London on the 30th December, 1691. His friend, Dr. Burnett, Bishop of Salisbury, preached his funeral sermon. His tomb is in Westminster Abbey. Thomas Birch who published a complete edition of his works, prefaced them by an excellent life of Boyle, from which we have quoted the greater part of the details respecting his person and disposition.

Is there an existence more worthy of our admiration, of our respect, than that of such a man, eminent at once for his virtues, his character

and his learning, so passionately fond of study as to sacrifice to it all that makes the object of ambitious men? Is there a nobler use of the gifts of fortune than to remove the material obstacles that impede the path of science, to encourage labor, to form public institutions? Is there a happier fate than that of a learned man become, by the authority of his name and example, the central point to which may be referred all scientific labors, all the discoveries, in fact, the whole future of the sciences which he cultivates; we might almost say the whole final destiny of civilisation?

Journal de Pharmacie et de Chimie, December, 1856.

CHEMICAL PHILOSOPHY.

*On some SPECIAL AFFINITIES. On some NEW FACTS RESPECTING
IODIDE of SILVER and the METALLIC FLUORIDES.*

By M. H. SAINTE-CLAIRE DEVILLE.

IN chemical phenomena in which cohesion does not intervene, in which the law of Berthollet has no application, we are able to fix certain rules which the reactions appear always to obey. Thus, in the natural family of chlorine, bromine, iodine, and cyanogen, we say positively, and in a general manner, that each of these bodies will drive from its combinations those which follow it in the series which I have just given. In this memoir, my intention is to show that there are some curious exceptions to this rule, not only as regards the chlorides and other halogenous bodies, but also as regards even the fluorides, whose properties are often far from corresponding to their probable composition. In one word, there are special affinities: some are well known, for example, the affinity of cyanogen for mercury, which, in the presence of water, removes potassium and sodium from the alkaline cyanides. I wish to show that iodine and silver have such an affinity for each other as to form an exception to the rule to which I have previously alluded.

I have proved that liquid hydriodic acid dissolves pure silver, with a violent disengagement of hydrogen, when the acid is concentrated. In consequence of this reaction we obtain transparent and voluminous crystals of iodide of silver. Wishing to reproduce the natural chloro-iodide of silver, I put together silver and a mixture of hydrochloric and hydriodic acids: I only obtained iodide of silver. I then determined to treat dry precipitated chloride of silver with concentrated hydriodic acid. The chloride heated like quicklime when being slaked, and I obtained iodide of silver, easily recognised by its color, and its insolubility in ammonia. The hydrochloric acid had been driven off in the state of gas by the hydriodic acid. We could likewise ascertain that iodide of potassium dissolved in water is rendered alkaline by metallic silver, with a formation of iodide of silver. Moreover, iodide of potassium fused in a porcelain crucible, and kept in contact with silver at cherry redness, is decomposed in appreciable

quantities. We find, after the experiment, at the bottom of the crucible, a transparent pearl of silicate of potassa, and sometimes a black ring which proves the production of silicium at the expense of the substance of the crucible, and consequently the production of potassium at the expense of the iodide of potassium. Besides this, we observe in the alkaline iodide, which has remained intact, a considerable quantity of iodide of silver. It will be observed that no vessel exists which enables us to demonstrate by a more evident experiment, the separation of the potassium from its iodide by means of silver.

Before passing to another series of facts of the same kind, I will mention that anhydrous chloride of aluminium combines with the alkaline chlorides with a disengagement of heat; I must, likewise, observe that the sesquichlorides of iron and chrome present the same property, so that we can obtain with sesquichloride of iron and common salt, a matter which is fusible at about 200° C. (392° F.) and very fluid at that temperature. The corresponding compound of chrome has this peculiarity, that it is green and soluble in water, whereas the sesquichloride of chrome used to prepare it, is violet and insoluble, whence it follows that the chrome has changed its molecular state in passing into the new compound. Thus, we see that some peculiar affinities manifest themselves in these sesquichlorides, in the presence of the alkaline chlorides, thus, giving another fact in support of the general classification proposed by M. Dumas. (I have likewise prepared a double sulphuret of aluminium and potassium, which is crystallisable, decomposing water with violence, by causing sulphur to pass over a strongly heated mixture of charcoal and potassa alum). Combinations of the same kind exist among the fluorides; a remarkable example of this is found in cryolite. If we endeavor to decompose cryolite at a high temperature with hydrochloric acid, this double fluoride of aluminium and sodium is not transformed into a double chloride, notwithstanding the affinities of which I have just spoken, chloride of sodium alone is formed, the fluoride of aluminium remaining intact.

Fluor spath is readily transformed into chloride of calcium by gaseous hydrochloric acid at a high temperature, so that I have had a source of anhydrous hydrofluoric acid, which I have used in the following experiments, from which I shall draw some conclusions after having briefly described them.

A mixture of fluor spath and alumina contained in a charcoal boat, and introduced into a tube of the same material, then heated at the fusing temperature of cast iron, with gaseous hydrochloric acid, gives chloride of calcium, and fluoride of aluminium, volatilised into large cubic blocks of great beauty. It is the same with the production of sesquifluoride of chrome; only the latter is not volatilised except at a much higher temperature. Thus, hydrochloric acid decomposes the alkaline fluorides, and does not decompose the sesquifluorides, which are sublimed in the midst of the acid gas carried to an extremely high temperature.

We may likewise have a proof of what I advance, by treating a

mixture of feldspath and fluorspath by gaseous hydrochloric acid in the same circumstances. We obtain as a result, besides chloride of calcium, chloride of potassium proceeding from the feldspath, and a very complex crystallised compound which I have not been able to analyse. It contains fluorides of silicium, aluminium and calcium, the latter, doubtless, proceeding from an excess of the fluorspath. When we treat in the same manner a mixture of argil and fluorspath, we obtain as volatile products gaseous fluoride of silicium, and fluoride of aluminium, which condenses. To render these statements clear, I ought to mention how I have been able to procure anhydrous sesquifluorides of iron and chrome, which have not been met with crystallised. If calcined sesquioxide of iron is placed in a platinum crucible, and moistened with hydrofluoric acid, the mixture heats extremely, as occurs with alumina. After putting an excess of acid and drying the matter, the temperature of the crucible is raised to cherry redness. The fluoride is then fused, and partially volatilised, and we may readily distinguish in the mass the small cubic crystals of a transparent and almost colorless fluoride of iron. The sesquifluoride of chrome is obtained in the same manner, except that the hydrated oxide is to be employed. This fluoride is fusible, crystallisable, either by the cooling of the fused matter, or better by volatilisation, which takes place only at the fusing temperature of steel. These crystals are octohedra, which have the appearance of regular octohedra; but they are too small to be measured, and too dark colored to permit of studying their influence on polarised light. They are of a beautiful green color, like oxide of chrome.

Special affinities are manifested in the exceptional properties of these fluorides. The fact of their production by hydrofluoric acid, the extraordinary disengagement of heat developed on the contact of this acid, and the oxides of aluminium and iron strongly calcined to which *cooking*, as M. Chevreul terms it, has communicated a complete indifference to the ordinary temperature, the resistance of the sesquifluorides to the action of gaseous hydrochloric acid at a high temperature, finally, the curious fact mentioned by Berzelius, that alumina removes a portion of the fluorine from the alkaline fluorides, rendering them caustic, all these facts appear to me to make a fresh exception to the general rules, which serve us as guides in the classification of the halogenous bodies and their compounds.

Comptes Rendus.

ORGANIC CHEMISTRY.

On the PRODUCTION of MANNITE by MARINE PLANTS.

By Dr. T. L. PHIPSON.

It has been for some time known that certain marine algæ produce, when dried in the open air, efflorescences of mannite (sugar of manna) on their surface. Of this number are *Laminaria saccharina*, *L. digitata*, *Rhodomenia palmata*, *Fucus siliquosus*, *F. nodosus*, *F. vesiculosus*, *F.*

saccharinus, *F. serratus*, &c. In 1855, I observed this production of mannite on many of these algæ (which are very common), and especially on the *Laminaria saccharina*, the *Fucus vesiculosus*, and its numerous varieties, the *F. serratus*. I have now no doubt, that all the algæ which contain vegetable mucilage produce this substance, when in conditions favorable to its formation.

Some botanists have thought that the production of mannite, by marine plants, was in consequence of a *secretion* of the substance in question by the living plant; but I have proved that this saccharine matter is only produced after the vital activity of the plant has ceased. Moreover, my observations on this point lead me to believe, that the production of mannite is always the result of a peculiar kind of fermentation, which disoxidises the vegetable mucilage and transforms it into mannite.

We know that many of the marine algæ contain such large quantities of mucilage, that some naturalists have tried to render this production useful as a manufacture. For instance, Mr. Brown has found that prolonged boiling with dilute sulphuric acid, suffices to convert this substance into *arabins* (gum arabic).

I have obtained very large quantities of mannite after removing the algæ from the water, by placing them still moist between sheets of grey paper, so that the access of air is not prevented, and leaving them there for eight days. At the end of this time, the surface is covered with an efflorescence of mannite. It is in this case formed more in the shape of grains than in crystals, but when dissolved in water, such as is furnished by the plant, it crystallises, by the evaporation of the liquid, in long, acicular and colorless tufts.

I account for its production in the following manner:—

Supposing that the mucilage has the formula attributed to it, $C^{12} H^{16} O^{10}$, and which represents the composition of this substance dried *in vacuo* at $180^{\circ} C.$ ($266^{\circ} F.$), we find that in the presence of water and losing one equivalent of oxygen, it may be divided into two equivalents of mannite; thus—



Veg. mucilage + water = mannite + oxygen.

It is, consequently, by means of a *disoxidising influence* exerted on the mucilage, that mannite is produced. This substance is likewise produced during viscid fermentation, as is well known, in which case a viscid matter is formed of the nature of the gums (in wine, beer, and vegetable juices as they spoil), and in these circumstances the mannite produced, proceeds evidently from the disoxidising action which the fermenting matter exercises on this viscid substance.

I have often observed, that the intercellular mucilage of marine algæ, being decomposed during the production of mannite, the superficial tissue of the plant is deteriorated; this is especially the case with *Laminaria saccharina*, one of the algæ which furnishes the most mannite; with the microscope we perceive, that the pulverulent and white layer of mannite is accompanied by a great many isolated

spherical cellules, proceeding from the destruction of the plant; water put to them dissolves the mannite, and leaves these cellules at liberty. A piece on which there was no mannite, showed these same cellules in juxta-position and implanted in the mucilage, forming a continuous smooth layer, which becomes pulverulent by the formation of mannite.

The body which causes the alteration of the mucilage in the algæ, is probably the albuminous matter which all these vegetables contain, and which decomposes in the air, causing, by prolonged decomposition, those disagreeable odors which characterise the putrefaction of the algæ, as well those of fresh as of sea water.

The production of mannite: 1st, by marine algæ; 2nd, by the viscid fermentation of wine and beer; 3rd, by the decomposition of mucilaginous vegetable juices, such as those of the beet root, dog's grass, ash, &c., leads us to imagine that the origin of this saccharine matter is vegetable mucilage, rather than *sugar*, as many chemists have supposed. I think we may venture to say that wherever, in nature, there exists at once vegetable mucilage and a disoxidising action, mannite will be produced.

It appears probable, that the viscid substance produced during the decomposition of many beverages, and whose real nature has only lately been suspected, is nothing but vegetable mucilage of the formula $C^{12} H^{10} O^{10}$.

Comptes Rendus, No 22, December 1st, 1856.

On QUINIDINE. By J. E. DE VRY, Ph.D., ROTTERDAM,

In the year 1853, Pasteur found that the substance called *Quinidine* was a mixture of two crystallisable alkaloids, each of which had a different rotatory power, when observed in the polarising apparatus.

The greater part of the mixture was lævogyre, and the crystals were not altered in the air. With chlorine and *Liquor Ammonia* no green coloration was observed. To this part of the mixture, Pasteur gave the name of *Cinchonidine*.

The smaller part of the mixture was *quinidine*; it was dextrogyre and isomeric with quinine. The crystals were efflorescent in dry air and very soon became opaque. With chlorine and *Liquor ammonia* they showed the same green color as does quinine when similarly treated.

Although the mixture which Pasteur examined consisted of *cinchonidine* and *quinidine*, my own observations have shown me that the greater part of the so-called *quinidine* met with in trade has been, until recently, nothing else than *cinchonidine* containing more or less *quinine*. Thus, for instance, in the Paris Exhibition of 1855, there were several samples of so-called *quinidine*, and of its compounds, but I found them to be all mainly *cinchonidine*.

I am indebted to Mr. John Eliot Howard, for having first supplied me with genuine *quinidine*, perfectly pure: soon after I had become

acquainted with its properties, my friend M. Delondre, also sent me several samples of true quinidine, in a state of greater or less purity. The quinidine of Mr. Howard was so pure, that I found the same rotatory power as Pasteur, viz. 250° dextrogyre.

Although I am still occupied with the investigation of quinidine and the other cinchona-alkaloids, which investigation is not yet completed, I will not delay making known one property of quinidine, by which it differs widely from the other three cinchona-alkaloids, because the knowledge of this property may be useful for the successful separation of quinidine from its cognate-alkaloids.

Quinidine forms with hydriodic acid a salt which is very difficultly soluble. One part of *neutral* hydriodate of quinidine requires 1250 parts of water at 60° F. for its solution. The *acid* hydriodate is much more soluble, for one part is dissolved by 90 parts of water at 60° F. By this difficult solubility of its neutral hydriodate, the presence of quinidine can easily be detected. I was enabled by this reaction together with the observation by polarized light, to prove undoubtedly that the so-called *B Quinine* of Van Heyningen is nothing else than *quinidine*. A few grains of *B Quinine* prepared by the late M. Van Heyningen himself, enabled me to prove this fact.

Although the sulphate of quinidine is sparingly soluble in cold water, the solution is nevertheless very strongly precipitated by a solution of iodide of potassium. I have no doubt that the manufacturers of quinine will be able by using this reaction, to find quinidine in many specimens of the so-called *quinoidine* or *amorphous quinine*, or at least, in the mother liquors obtained in the preparation of sulphate of quinine.* In the hands of the scientific manufacturer, such as Mr. Howard, it may serve for the discovery of the barks which contain quinidine. If a bark contains quinidine, a tincture made with strong alcohol, will form crystals of hydriodate of quinidine upon the addition of a few drops of hydriodic acid and stirring the liquid with a glass rod. If quinidine is present, small crystals will separate, either immediately or after some hours, wherever the rod shall have rubbed against the glass.

Pharmaceutical Journal, February, 1857.

On the COMBINATIONS of ARACHIC ACID.

By MM. SCHEVEN and GÖSSMANN.

THIS work is a completion of a previous memoir; the former one contains a description of the salts formed by the arachic acid, $C^{40}H^{40}O^4$, contained in oil of arachide, and some data respecting arachamide and arachine.

Arachate of Potassa, $KO, C^{40}H^{39}O^3$.—To prepare this salt arachic acid is boiled for a long time with a concentrated solution of

* This supposition has since been proved by examination of quinoidine from the manufactory of Mr. Zimmer, from which I obtained 23 per cent. of hydriodate of quinidine.

potassa, it is evaporated to dryness, and the alkaline mass is exposed for some time to the action of carbonic acid, until the excess of potassa has been converted into carbonate. It is then exhausted with alcohol at 95° C. (213° F.) and purified by several crystallisations. This salt is crystalline, soluble in alcohol, and in fifteen times its weight of boiling water. When we add thirty or forty times its volume of water to the aqueous solution there is a separation of brilliant plates of acid arachate of potassa.

The Arachate of Soda, prepared in the same manner as the salt of potassa, possesses analogous properties.

Arachate of Ammonia, $\text{NH}^4 \text{O}$, $\text{C}^{40} \text{H}^{39} \text{O}^3$. When we supersaturate with ammonia a hot alcoholic solution of arachic acid and allow it to cool slowly, the arachate of ammonia is abundantly deposited in the form of needles. After dessiccation this salt constitutes a light crystalline powder. When exposed to the air it loses the ammonia.

Arachate of Magnesia, Mg O , $\text{C}^{40} \text{H}^{39} \text{O}^3$.—This salt is prepared by mixing an alcoholic solution of arachate of ammonia with an alcoholic solution saturated without heat, of acetate of magnesia, and carried to ebullition to dissolve the precipitate first formed. By cooling, this salt is precipitated in small prisms grouped in stars. Purified by a second crystallisation, it is found under the form of a light crystalline powder, sparingly soluble in alcohol, and insoluble in water.

The salts of lime, baryta, and strontia, may be prepared in the same manner as that of magnesia. They form crystalline powders, less soluble than the latter salt.

Arachate of Copper, Cu O , $\text{C}^{40} \text{H}^{39} \text{O}^3$.—Prepared by double decomposition with alcoholic solutions of acetate of copper and arachate of ammonia, is a greenish blue precipitate, becoming by degrees crystalline.

Arachate of Silver, Ag O , $\text{C}^{40} \text{H}^{39} \text{O}^3$.—Prepared like the preceding salt, dissolves completely in boiling alcohol, and deposits by cooling, brilliant prisms unalterable in the air.

Arachic Ether, $\text{C}^4 \text{H}^5 \text{O}$, $\text{C}^{40} \text{H}^{39} \text{O}^3$.—This is prepared by passing a current of hydrochloric gas into a solution of arachic acid in absolute alcohol. It separates towards the close of the operation, under the form of oleaginous drops.

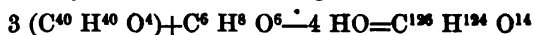
Arachamide, $\text{C}^{40} \text{H}^{40} \text{NO}^3$.—This was prepared by the direct action of ammonia on oil of arachide, this oil was mixed with alcohol saturated with ammonia, and the mixture was left for several weeks, frequently agitating it. After this time it was again saturated with ammonia, and when the reaction appeared concluded, the ammonia and alcohol were driven off at a moderate heat, the cold and solidified residue was pressed, and it was crystallised several times from alcohol.

This amide dissolves very readily in alcohol at 95° C. (203° F.), and is deposited on cooling, in prisms grouped in stars, fusible at 98° or 99° C. (208° to 210° F.)

Arachine.—By heating to 210° C. (410° F.) equal parts of syrupy glycerine and arachic acid, we obtain a neutral compound, artificial

arachine. In a pure state this matter forms a colorless brilliant mass, sparingly soluble in absolute alcohol, and especially in ether. When separating slowly from its alcoholic or ethereal solution it is crystalline. It fuses at 70° C.) 158° F.) When cooled rapidly it forms an amorphous mass; when cooled slowly it has a crystalline texture. Its composition is expressed by the formula $C^{126} H^{124} O^{14}$.

It is formed by virtue of the following reaction:—



Annalen de Chimie und Pharmacie.

On the ARACHINES. By M. MARCELLIN BERTHELOT.

M. GÖSSMANN has found a new fatty acid in the oil of the *Arachis hypogæa*. The formula of this acid has been given as $C^{40} H^{40} O^4$, and details of the salts have been given, as well as those of its ether, amide and one glyceric combination (arachine).

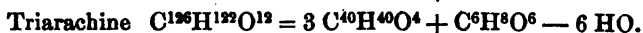
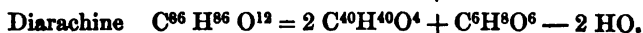
Without going into the first three points, which appear to be very carefully treated, I shall proceed to give some reflections on the preparation of arachine. This point is very important, for the preparation and purification of the neutral fatty bodies are much more delicate than those of the ethers, and forgetting any precaution might cause the greatest confusion in the interpretation of the results.

The substance which MM. Scheven and Gössmann obtained, is not formed of one single principle; it is a mixture of neutral diarachine and *free arachic acid*, which those gentlemen have not endeavored to separate. There is no doubt on this point; for in the preparation of the neutral fatty bodies, by the direct union of glycerine with the fat acid, one portion alone forms a neutral compound, whereas a great part of the fatty acid on the one hand, and of glycerine on the other, remains free. This fact results especially from the decomposing action of the water, which is a necessary product of the reaction. It is likewise necessary to free the neutral fatty body, not only from the excess of glycerine, which is very easy, but likewise from the excess of fatty acid. This latter point, in the case of fixed fatty acids, can only be certainly attained by the following process: the crude fatty body is put into a flask and melted on the sand bath, adding a known quantity of slaked lime, then a little ether, and the whole is digested on the sand bath for a quarter of an hour. That done, the mixture is treated with boiling ether, the fatty acid remains combined with the lime, and the neutral fatty body alone is dissolved in the ether. The ether is evaporated; the product of this evaporation *dissolved* in boiling alcohol, ought to have no action on tincture of litmus *dissolved* in the same vehicle; should it redden it, which frequently occurs, it will be necessary to repeat the treatment with slaked lime.

To give these criticisms an experimental verification, I have prepared the combinations of arachic acid and glycerine.

Arachic acid behaves with glycerine in the same manner as stearic

acid, and forms in analogous conditions three neutral combinations similar to the stearines.



The analyses of these compounds confirm the formula $C^{40} H^{40} O^4$ attributed to arachic acid.

Monarachine is obtained by heating arachic acid with glycerine for eight hours, at $180^\circ C.$ ($356^\circ F.$) This temperature should not be exceeded. It is isolated and purified with lime and ether. It is a white, neutral substance, almost insoluble in cold ether, sparingly soluble in boiling ether, from which it is precipitated in fine granules. When fused, it looks like wax.

Monarachine contains $C = 71.7$ $H = 12.0$, the formula $C^{46} H^{46} O^8$, requires $C = 71.5$ $H = 11.9$.

Diarachine is obtained, 1st, by heating glycerine and arachic acid for six hours, at 200° to $230^\circ C.$ (392° to $446^\circ F.$); 2nd, by heating monarachine and arachic acid with a trace of water, between 200° and $230^\circ C.$ for eight hours. It is extracted with lime and ether.

This is a white, neutral substance, almost insoluble in cold ether, very sparingly in hot ether; it separates from it in very small grains, whose crystallised nature is not evident, even when much magnified. Diarachine is more soluble in sulphuret of carbon than in ether; but those solutions, when abandoned to slow spontaneous evaporation, do not give it in appreciable crystals.

Diarachine contains:—

First Process.

C	:	:	:	:	:	:	:	:	:	74.1
H	:	:	:	:	:	:	:	:	:	12.2

Second Process.—First Preparation.

C	:	:	:	:	:	:	:	:	:	73.9
H	:	:	:	:	:	:	:	:	:	12.7

Second Preparation.

C	:	:	:	:	:	:	:	:	:	73.8
H	:	:	:	:	:	:	:	:	:	12.7

The formula $C^{86} H^{86} O^{12}$, requires $C = 73.9$ $H = 12.3$.

Diarachine fuses at about $75^\circ C.$ ($167^\circ F.$) When heated on platinum, diarachine volatilises almost entirely. It burns with a white flame and leaves no ash.

Heated to $100^\circ C.$ ($212^\circ F.$) for 70 hours with slaked lime, diarachine saponifies completely, and regenerates glycerine and arachic acid.

The conditions in which diarachine (first process) is formed, being evidently the same as those of MM. Scheven and Gössmann, I deter-

mined to ascertain how much free acid was contained in the crude product of the reaction of arachic acid on glycerine.

100 parts of the crude fatty body contained:—

Free arachic acid	42.6
Diarachine	57.4

The free acid was determined directly, in the following manner; I first ascertained, that arachic acid dissolved in boiling alcohol, and reddened tincture of litmus, dissolved in alcohol; if we add water to the liquor, the fatty acid is precipitated and the litmus turns blue; this phenomenon is a characteristic of the fatty acids. This fact proved, I dissolved 0.200 gr. of the crude fatty body in boiling alcohol, I added litmus, and I dropped in an estimated solution of lime water, until there was permanent alkaline reaction. The quantity of lime used indicates the proportion of the free fatty acid.

Triarachine is obtained by fusing diarachine with 15 to 20 times its weight of arachic acid; the mixture, *perfectly dry*, is put into a tube, and heated to between 200° and 220° C. (392° to 428° F.) for eight or ten hours. The triarachine is extracted and purified with lime and ether. It is a neutral matter, analogous to the preceding two, and in the same manner sparingly soluble in ether.

Triarachine contains C = 77.1 H = 12.6. The formula $C^{126}H^{122}O^{12}$, requires C = 76.6 H = 12.5.

Annales de Chimie et de Physique.

ANALYTICAL CHEMISTRY.

ESTIMATION of the SUGAR of MILK by the VOLUMETRICAL METHOD, or by MEANS of M. SOLEIL'S SACCHARIMETER, and a DETERMINATION of the RICHNESS of MILK. By M. POGGIALE.

MILK is a compound of a nitrogenous matter, fit for the production of our tissues, caseine, and two calorific substances, fatty matter and sugar, and is one of the most complete and most important aliments of man. It is often fraudulently adulterated, and amongst the adulterations of which it is the object, the commonest is removing the cream and adding water to the skimmed milk.

None of the processes hitherto known, have given in a rapid and vigorous manner, the indication of the richness of the milk. The galactometer is almost always an uncertain instrument, because generally ill constructed; or, because a multitude of circumstances may modify the density of the milk. It will be sufficient to observe, that the ordinary density of cow's milk is 1.032, that it may rise to 1.037 when the milk has been skimmed, and will sink to about 1.026 when the milk is rich in cream. Thus, by adding to the skimmed milk a certain quantity of water, we should obtain 1.032, the ordinary density of pure milk. The density of this liquid may likewise be augmented by the addition of some foreign substances.

The lactoscope of M. Donné gives useful indications, where the milk

is examined soon after having been drawn; but it is unfaithful when the examination is not made until 12, 24, or 36 hours after having been drawn. Moreover, several opaque liquids, such as almond emulsion, give the same degrees by the lactoscope as pure milk.

The determination of the volume of cream in a graduated vessel is, perhaps, the surest means of ascertaining the richness of the milk. Still this process has several inconveniences. It requires, at least, 12 hours for its verification, and the volume of cream separated depends on the temperature, and even the form of the vessel.

Chemical analysis is the only accurate way of ascertaining the proportions of the constituent principles of milk, but it is rarely employed in practice, in consequence of the time which it requires.

It struck me that if we could estimate quickly, without weighing, by the method of volumes, one of the constituent elements of the milk, the problem would be solved. I have obtained this result, by determining the proportion of sugar in milk by the cupreo-potassic liquid.

I have analysed many samples of pure milk from farmers, and have obtained the following results:—

Time since parturition.	Solid matters per cent.	Caseum.	Butter.	Sugar of Milk.	Salts.	Water.
2 months	13·6	3·9	4·2	5·2	0·3	86·4
3 "	13·3	3·6	4·3	5·1	0·3	86·7
4 "	13·7	3·5	4·6	5·4	0·2	86·3
5 "	12·9	3·9	3·8	4·9	0·3	87·1
6 "	13·1	3·3	4·3	5·3	0·2	86·9
7 "	14·2	4·1	4·8	5·0	0·3	85·8
8 "	13·6	3·6	4·3	5·5	0·2	86·4
9 "	14·3	4·1	4·9	5·0	0·3	85·7
10 "	14·5	4·8	4·8	5·6	0·3	85·5
15 "	13·0	3·7	4·3	5·7	0·3	86·4

The mean of these ten analyses, gives the following proportions in 1000 parts of milk:—

Water	.	.	.	.	.	.	862·8
Butter	.	.	.	.	.	.	43·8
Sugar of milk	.	.	.	.	.	.	52·7
Caseum	.	.	.	.	.	.	38·0
Salts	.	.	.	.	.	.	2·7

1000·0

The evaporation of the milk was performed in a dry vacuum. The fatty matters were isolated by means of ether. The sugar was estimated by the volumetrical method, the caseum and salts by the ordinary processes.

Thus, according to my experiments, 1000 grammes of milk contain 52 grammes, 7 decigrammes of sugar. M. Boussingault found a mean

of 50 grammes, a difference due, no doubt, to the processes which we followed for the determination.

The quantity of sugar contained in milk is, as we see, considerable, and presents, moreover, but slight variations. These two circumstances give more importance and precision to the method which I propose for ascertaining the richness of milk.

This process is the application of that of M. Barreswil to the estimation of the sugar in milk. We know that according to the observations of M. Frommertz, glucose reduces the salts of copper and causes the formation of a red precipitate of protoxide of copper, whereas cane sugar does not possess that property.

Sugar of milk, which reduces binocide of copper, like glucose, may readily be estimated by means of this reaction. The proportion of cupreous salt decomposed, will give the quantity of sugar of milk.

This estimation may be made in an accurate manner by using the method which Gay-Lussac employed, for testing commercial potassa, and in chlorometry. This process is composed—1st, of the preparation of a test liquor; 2nd, of the preparation of whey; 3rd, of testing the whey, or decoloration of the test liquor.

I shall describe each of these operations, which present some interest in a practical point of view.

PREPARATION OF THE TEST LIQUOR.

The test liquor is prepared by adding bitartrate of potassa to a solution of sulphate of copper, and dissolving the precipitate formed, by means of caustic potassa. On being filtered, it is limpid and of a dark blue.

The alkaline solution of oxide of copper being prepared, the value is very carefully determined. For this purpose, we use a solution made with distilled water, and a known quantity of pure, dry sugar of milk. To prepare this solution well, we must have a graduated test glass with a foot; into this we put a known quantity of sugar of milk, say five grammes, and add a known volume of distilled water, say 100 cubic centimetres. This liquor is then agitated to aid the dissolution of the salt.

The test liquor is reckoned by the quantity of sugar required to decolor a known volume of this liquor. Thus, using the above proportions, I will suppose that 3.7 c.c. of a saccharine solution have been taken, containing 5 per cent. of sugar of milk, to decolor 20 c.c. of cupreous liquor, the following calculation will show the quantity of sugar employed:—

$$100 : 5 :: 3.7 : x. \quad x = 0.185 \text{ milligrammes.}$$

Twenty cubic centimetres of test liquor are consequently reduced by 185 milligrammes of sugar of milk. This operation will be described further on.

It is important to observe, that the value of the normal test liquor should be determined with sugar of milk. A serious error will be made by the employment of cane sugar.

I have tried to avoid the determination of the solution of binoxide of copper, which is undoubtedly a very delicate operation. The following proportions have always given a liquor, 20 cubic centimetres of which correspond to 0.185 milligrammes of sugar of milk :—

	Gr.
Crystallised sulphate of copper	10
Crystallised bitartrate of potassa	10
Caustic potassa	80
Distilled water , ...	200

It is understood that the sulphate of copper is to be pure.

I have for two years used Fehling's liquor, which keeps longer than the above. It is prepared as follows :—

	Gr.
(A.)—Pure crystallised sulphate of copper	40
Distilled water	160
(B.)—Caustic soda	130
Distilled water	600
Neutral tartrate of potassa	160

These are mixed, and to them is added—

Distilled water q. s. to form 1154.4 c.c. of liquor at 15°.

Twenty cubic centimetres of this liquor are completely decolored by 136 milligrammes of sugar of milk.

PREPARATION OF THE WHEY.

The estimation of the sugar of milk cannot be executed with precision by acting directly on the milk. The opacity of this liquid does not enable us to know the exact moment of the reduction of the bin-oxide of copper. It is therefore indispensable to separate the fatty matter and the caseum by coagulation. This is easily done by putting 50 or 60 grammes of milk in a small flask, adding a few drops of acetic acid or sulphuric acid, and raising the temperature to 40° or 50° C. (104° or 122° F.) It is then filtered; the first portions of whey are turbid, these are replaced on the filter, and a transparent liquid is then obtained. The whey thus prepared is composed of water, sugar of milk, salts, and a small quantity of caseine and butter; 1000 grs. of milk give, according to my experiments, 923 grs. of whey. This gives about 57 grammes of sugar in 1000 grammes of whey.

I have ascertained that the butter, caseine, and the salts in milk exercise no sensible action on the cupreous solution.

TESTING THE WHEY.

The instruments for testing the whey are :—

1st. A small flask with a round bottom, holding from 70 to 80 grammes of water.

2nd. A burette, each division of which is equal to a tenth of a cubic centimetre.

3rd. A pipette, holding from 10 to 20 cubic centimetres of water.

These three small instruments being prepared, about 20 c.c. of test liquor are taken with the pipette and introduced into the flask, which is preferable to a porcelain capsule, because we are able to observe the liquid from the bottom to the top, and to know more accurately when the decoloration is complete. We add 20 or 25 grammes of water, and two or three grammes of caustic potassa. The temperature of the liquid is then raised to the boiling point, either by means of a lamp, or of burning charcoal. The burette is filled with whey, which is dropped into the liquid. In the same manner as with glucose, this forms at first a yellow precipitate of hydrated protoxide of copper, which soon becomes red and falls to the bottom of the flask. The whey is carefully poured in until the blue color has entirely disappeared. It is necessary to heat the flask after each addition of whey. When the operation is concluded, we read off from the burette the quantity of whey employed in the decoloration of the test liquor, and by means of the proportion we determine the weight of sugar. Thus, I suppose that 20 c.c. of Fehling's liquor are decolorized by 2.4 c.c. of whey, the following proportion shows the quantity of sugar contained in a quart of whey:—

$$2.4 : 0.186 :: 1000 : x. \quad x = 56.6.$$

100 grammes of whey, consequently, contain 56.6 of sugar of milk. More exact results will be obtained by adding to the whey an equal volume of distilled water, and multiplying by 2 the number expressing the proportion of sugar.

I have thought it best to give the amount of sugar in the whey on which we operate, instead of in the milk. It is easy to calculate from that, the proportion of sugar in a quart of milk.

This method appears perfectly successful. It is very easy to use, the results are perfectly exact, and the operations may be executed in a few minutes.

I have observed above, that 1000 grammes of whey contain 57 grammes of sugar; but I think that an allowance of a few grammes is necessary.

The result of my observations is, that the milk commonly sold contains only 35 to 45 grammes of sugar in 1000 grammes of whey. The sellers usually add water, after having removed a portion of the cream. This fraud is immediately discovered by the process, and I think that it will be found especially useful for this purpose.

If the milk is adulterated by albuminous substances, gum arabic, dextrine, fecula, emulsions of feculant seeds, the fraud will be ascertained by estimating the sugar, because this can only be done by adding water to the milk. It might still happen, that cream may have been removed without the addition of water; or sugar of milk, or glucose may have been added. The quantity of sugar remaining the same, we might be induced to consider it as good milk, from which the fatty matter has been removed. In this case, I determine the quantity of fatty matter by a simple and expeditious process, by adding acetic acid to the boiling milk and agitating it, after cooling,

with ether, which removes the butter. Two layers are quickly formed. The upper one, which contains the fatty matter, is decanted; the butter is obtained as a residue by evaporating the ethereal liquid. The process proposed by M. Marchand may then be used, but this second test is rarely necessary, as we never find the normal quantity of sugar in commercial milk.

SUMMARY OF THE PRECEDING OPERATIONS.

Twenty cubic centimetres of the above described liquor are introduced into a small flask, to which are added water and some pieces of caustic potassa, and the temperature of the liquid is raised to ebullition. On the other hand, a graduated burette is filled with whey which is dropped into the cupreous solution, agitating it continually, and heating after each addition of whey. This is continued until the binoxide of copper is entirely reduced. The operation is concluded when the liquor is completely decolorized, when examined from the bottom upwards. The quantity of whey used is then read from the burette, and the quantity of sugar contained in 1000 grammes of whey is calculated. It must be remembered, that 1000 grammes of whey contain 5.7 grammes of sugar, and that an allowance of a few grammes should be made.

It is often advisable to repeat these experiments as verifications. Everything being ready and the quantity being nearly known, one or two minutes will suffice for the second testing.

ESTIMATION OF THE SUGAR OF MILK, AND THE DETERMINATION OF THE RICHNESS OF THE MILK BY MEANS OF THE SACCHARIMETER OF M. SOLEIL.

In 1850, I executed with M. Soleil's saccharimeter, some researches which have led me to very accurate results in the estimation of sugar of milk. This new method is based on the optical properties of the well clarified serum of the milk. For this purpose, I used the ingenious apparatus for polarisation invented by M. Soleil. It is known that the light which penetrates this instrument produces in traversing the prisms found in it, two discs separated by a medial line. The two halves of each disc have the same tint in the normal state of the instrument; but when a saccharine liquid, whey for instance, is placed on the passage of the luminous rays, the two sides of the disc have no longer the same coloration. If the colors are then rendered identical by means of two prisms of quartz, which are moved at will, and the numbers of the degree marked on a table fixed on the prisms of quartz is observed, we can determine very accurately the quantity of sugar contained in the whey, and thus ascertain the richness of the milk.

To make this experiment the milk is first coagulated by means of acetic or sulphuric acid, at the temperature of 40° or 50° C. (104° to 122° F.); it is then filtered, and a few drops of acetate of lead are added to the liquid, which cause a very abundant precipitate. A fresh filtration gives us a perfectly transparent liquid, very fit for this species of investigation.

The serum being thus prepared, it is introduced into an observation tube, 20 centimetres in length, which must be closed, and then placed on the instrument to obtain the number of degrees, indicating the deviation of the polarised light produced by traversing the saccharine liquid. If 28°, for instance, be obtained, it will only be necessary to examine a table, which I have arranged, to ascertain the weight of the sugar contained in a quart of whey. The same result may be obtained by this calculation.

$$100 : 201.90 :: 28 : x. \quad x = 56.53,$$

that is to say, that in this supposition 1000 c.c. of whey contain 56 grammes 53 centigrammes of sugar of milk.

201.90 is the quantity of sugar of milk, which, dissolved in distilled water to 1000 c.c. produces a deviation of 100°.

The following are the most useful numbers for this calculation :—

Degrees found.	Quantity of sugar per litre of whey.	Degrees found.	Quantity of sugar per litre of whey.	Degrees found.	Quantity of sugar per litre of whey.
	Grs. Cent.		Grs. Cent.		Grs. Cent.
15	30.28	24	48.45	33	66.62
16	32.30	25	50.47	34	68.64
17	34.32	26	52.49	35	70.66
18	36.34	27	54.51	36	72.68
19	38.36	28	56.53	37	74.70
20	40.38	29	58.55	38	76.72
21	42.39	30	60.57	39	78.74
22	44.41	31	62.58	40	80.76
23	46.43	32	64.60	100	201.90

I should mention that, according to my experiments, 1000 grs. of milk contain 52.7 grs. of sugar, and give 923 grs. of whey. Consequently, 1000 grs. of whey contain about 57 grs. of sugar. This proportion corresponds with about 28° of the table; but it is necessary to make an allowance of one or two degrees.

I have found that milk, as commonly sold, marks only about 19° to 23°. This is because the vendors generally add water to the milk, after having removed a portion of the cream.

Journal de Pharmacie et de Chimie, Nov. 1856.

ESTIMATION of COMMERCIAL ACIDS. By M. VIOLETTE.

GAY-LUSSAC has given the estimation of commercial potashes; his alkalimeter is in the hands of every manufacturer. I propose using the same instrument for the estimation of commercial acids; it is only necessary to add a solution of saccharate of lime to the reagents of the alkalimeter, and to operate with the same utensils, and in the same manner as in the estimation of potash. The principle of the process

is as follows:—If we saturate with the quantities b and b' of one base the quantities a and a' of two different acids, whose equivalents are e and e' , we have

$$\frac{a}{a'} = \frac{e}{e'} \times \frac{b}{b'}$$

If we take for a 1 gramme of the acid to be estimated, for a' 1 gramme of *normal* sulphuric acid, whose density is $e=612.50$, which is that indicated by Gay-Lussac in the estimation of potassa, and if we call T the centesimal value of the acid a , or the real quantity contained in 100 parts, we have the general formula—

$$(1.) T = 100 \times \frac{e}{612.50} \times \frac{b}{b'}$$

The values b and b' will be determined by the quantities of saccharate of lime necessary to saturate the acids, and are given by the experiment; as for the value of e , it will be found in the chemical tables. By substituting this latter in the formula (1) for each acid, we have the following formulæ, giving for each acid its centesimal value.

Sulphuric Acid.

$$(2.) T = 100 \times \frac{612.50}{612.50} \times \frac{b}{b'} = 100 \times \frac{b}{b'}$$

Hydrochloric Acid.

$$(3.) T = 100 \times \frac{455.70}{612.50} \times \frac{b}{b'} = 74.40 \times \frac{b}{b'}$$

Nitric Acid.

$$(4.) T = 100 \times \frac{787.50}{612.50} \times \frac{b}{b'} = 128.57 \times \frac{b}{b'}$$

Acetic Acid.

$$(5.) T = 100 \times \frac{750}{612.50} \times \frac{b}{b'} = 122.44 \times \frac{b}{b'}$$

And thus for the other acids.

As an example of the process, suppose we have to test a sample of common commercial hydrochloric acid, the density of which is 1.16.

First Operation.—Weigh 50 grammes of the hydrochloric acid to be tested, dilute it with water to a litre; take with a pipette 50 cubic centimetres of this solution, (representing 1 gramme of acid) redden them with a few drops of tincture of litmus, and drop in a solution of saccharate of lime by means of the alkalimetric burette, until the change of color restored to blue shows that the saturation is complete; the number b of divisions of the burette showing that the saccharate used was 24.

Second Operation.—Take with the pipette 50 cubic centimetres of *normal* sulphuric acid (containing 1 gramme of hydrated sulphuric acid) redden it with litmus, and saturate with saccharate of lime as above; the number b' of the divisions of the burette shows that the saccharate employed was 53.

By substituting the values of b and b' in formula (3) we have—

$$T = 74.40 \times \frac{24}{53} = 33.83 \text{ per cent.}$$

We find in the tables compiled by Davy, that hydrochloric acid, whose density is 1.17; contains 34.34 per cent. of real acid; this approximation inspires us with confidence in the process.

The preparation of the reagents is described by Gay-Lussac in his estimations of potassa; as for the saccharate of lime, it is prepared by digesting without heat, 50 grammes of caustic lime, slaked in 1 litre of water containing 100 grammes of sugar.

In my memoir I have tested successively all the commercial acids, and have given some modifications for the estimation of weak acetic acid or comestible vinegar. The alkalimeter will consequently serve in future for testing acids as well as alkalies; we can thus estimate the free acids contained either in residues of manufactories, starch water, pulps, &c. &c., or in any acid fruits; it will also serve to guide the manufacturer in the production and concentration of acids.

Comptes Rendus.

ESTIMATION of COMMERCIAL SALTS of TIN.

By M. SCHLAGDENHAUFFEN.

MR. CRACE-CALVERT, of Manchester, uses the protochloride of tin, as a reagent for the chromates, and renders it useful in the volumetrical analysis of the ores of chrome; the equation on which this reaction is based is the following:—



The author does not generalise, and is satisfied with giving his method of estimating.

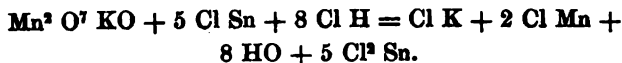
It is well known, that this reducing action of chloride of tin is general, and extends to all easily reducible compounds, especially to the manganates and hypermanganates. In the same way, when we wish to analyse quantitatively a liquor containing a stannous salt, it is made to pass into the state of a stannic salt by means of an oxidising agent, such as chromic or hypermanganic acid.

Whence it follows, that Mr. Calvert's method serves for the volumetrical estimation of manganese ores transformed into hypermanganates, exactly in the same manner as chrome ores are converted into chromates; in the second place we can, on the same principle, analyse compounds of tin by means of an estimated liquor of chromate, or hypermanganate of potassa.

There are many methods of estimating commercial manganese. I should not venture to propose a fresh one, did it not present itself naturally in the course of the study which I have proposed to make.

To estimate commercial manganese, a known quantity of the sample must be converted into hypermanganate, and the amount of stannous chloride acidified with hydrochloric acid ascertained, which is neces-

sary to decolor the hypermanganate, and consequently to transform it into a manganous salt. The weight of chloride of tin used, shows proportionately that of the hypermanganate. Indeed, we have—



The proportion between the weights and the equivalents is then—

$$5 \text{ eq. Cl Sn} : \text{eq. Mn}^2 \text{ O}^7 \text{ KO} :: p : x,$$

p indicates the weight of chloride of tin used, and the fourth term of the proportion expresses the weight of hypermanganate required. It is easy to deduce from this the weight of real peroxide contained in the sample of manganese analysed.

Operating in an inverse manner, namely, by taking the hypermanganate of potassa as the reagent for chloride of tin, we obtain the true weight of this compound contained in a solution. For this purpose, we pour into the liquor to be analysed, the hypermanganate rendered acid by hydrochloric acid, until this salt is no longer decolorized; we then read the divisions of test liquor used; this number of divisions corresponds to a certain weight of hypermanganate. Then knowing the weight of hypermanganate used, we have the weight of the chloride of tin by a proportion analogous to the preceding.

This method enables us to ascertain the quantities of stannous and stannic salts, which may be found mixed in one solution. For this purpose, two distinct operations are necessary.

1st. To transform the stannous chloride into stannic chloride, by means of the hypermanganate test liquor; the weight of this hypermanganate will give, as has been just seen, that of the stannous chloride contained in the mixture.

2nd. To reduce the stannic chloride with another quantity of the liquor into stannous chloride, and to pour into this new solution, the estimated liquor of the hypermanganate. The number of divisions of the burette will, in this second case, give the weight of the stannous chloride proceeding from the previous transformation of the stannic into a stannous chloride.

For these proportions we therefore require—

$$\text{Mn}^2 \text{ O}^7 \text{ KO} : \text{Cl Sn} :: p : x,$$

in which the value of x indicates the weight of the quantity of Cl K of tin, free in the solution.

The second proportion is—

$$\text{Mn}^2 \text{ O}^7 \text{ KO} : \text{Cl Sn} :: p' : x';$$

x' designates the total weight of the stannous chloride obtained by the transformation. That arranged, let us represent $x' - x$ by p'' we shall have—

$$\text{eq. Cl Sn} : \text{eq. Cl}^2 \text{ Sn} :: p'' : y,$$

for the weight of the stannic chloride of the original liquor; designa-

ting in this proportion by p the weight of stannous salt found directly, and by y , that of the stannic salt proposed to be calculated.

The best method of transforming the stannic chloride into stannous chloride, consists in driving off the excess of acid from the solution, and boiling the mixture with granulated tin in this manner; one equivalent of stannic chloride gives two equivalents of stannous chloride, and consequently, to regenerate stannic chloride with the latter, it is necessary to take twice the quantity of hypermanganate, which is necessary for transforming one single equivalent of stannous chloride.

Accordingly, it is only necessary to take for the second operation, half the weight of hypermanganate indicated by the burette. This number is in proportion to the whole of the stannic chloride found in the solution analysed.

When we wish to estimate similar mixtures of stannous and stannic salts, we must not neglect to evaporate the liquor, so as to eliminate the greater part of the acid.

Various compounds of tin are found in trade, amongst others the protochloride, deutochloride, and the ammonio-chloride of tin. These compounds all contain very variable proportions of protosalt and double salt, according to the manner in which they have been prepared.

Dyers and printers consume large quantities of these salts, sometimes to produce white patterns on colored grounds, sometimes in the preparation of the fabrics intended for printing steamed colors, and sometimes to obtain applied colors. It is consequently not unimportant to ascertain exactly the real quantities of stannous and stannic chloride contained in these compounds of tin, so to obtain the exact shade of color which is desired.

Journal de Pharmacie et de Chimie, February, 1857.

NEW METHOD of ESTIMATING CHLORIDES and SULPHATES.

By M. A. LEVOL.

THE usual way of estimating different bodies is to weigh them after having eliminated them, or to weigh certain definite compounds which we form with them.

The analysis of gaseous mixtures and alkalimetry, led by imitation for many years past, to the methods of estimating by volumes; this has been much extended; but those methods are unfortunately very limited when they depend on precipitation, because they then depend on the greater or less facility with which we can clear liquors by simple agitation, and so as to avoid the filter, whose use entails delays which we are anxious to avoid, when we have recourse to the method of volumetry.

This difficulty is, however, not insurmountable; and in some peculiar cases chemists have found means to obviate them by depending on phenomena of coloration, or on the insolubility of certain compounds. Still there has not appeared any improvement in this

direction in the case of an estimation which has very frequently to be performed by the chemist, I mean that of the alkaline chlorides and sulphates. This circumstance leads me to think that it may be useful for me to publish the methods which I have used for the rapid determination of the amount of hydrochloric and sulphuric acid contained in various commercial products.

ESTIMATION OF CHLORIDES.

Gay-Lussac's process, which is so valuable for the determination of mixtures of silver, and which may evidently be applied inversely for the estimation of the chlorine in chlorides, unfortunately loses one of its principal advantages, that of rapidity, when the proportion of the element which we wish to estimate is not previously known, at least approximately; moreover, we find great difficulty in clearing the liquors by simple agitation when they contain the alkaline chloride in excess, and it is then necessary to filter, and thus fall back into the delays inherent to the method of estimation by the weight of the chloride of silver.

These practical difficulties have led me to use a method based on the decomposition of phosphate of silver by the alkaline chlorides, in a similar manner the silver salts which are more soluble than chloride of silver. If, for example, we pour on to a precipitate of carbonate or phosphate of silver, a solution of chloride of potassium or sodium, there will be instantly formed a chloride of silver and an alkaline carbonate or phosphate; so that starting from the facts which rest upon the difference of solubility of the two salts, if we pour nitrate of silver into a mixture of phosphate and of alkaline chloride, the precipitation of the phosphate is only manifested after the entire precipitation of the chlorine. Whence it results that if we add *common* phosphate of soda to the solution of a chloride, the *yellow* precipitate of phosphate of silver which a soluble salt of silver forms in it does not appear, or at least does not continue after a slight agitation, until the whole of the alkaline chloride has been decomposed.

According to this, chlorides may be estimated in the following manner :—

Dissolve in about 50 parts of distilled water a known quantity, say 1 gramme of the salt, the chlorides in which we desire to estimate; add about a tenth of this volume of a solution, saturated without heat, of common phosphate of soda, free from chlorides; if the liquor is acid it is to be saturated, or even slightly supersaturated, by means of pure carbonate of soda, then pour from a burette, graduated in tenths of a cubic centimetre, an estimated aqueous solution of neutral nitrate of silver, until the precipitate retains a *slight yellow color*, which occurs only after the precipitation of the chlorine, for although the addition of the first drops of nitrate of silver produce a yellow precipitate, which is explained by the effect of the reagent on the point it touches, we find that agitation instantly causes its disappearance until after the complete precipitation of the chlorine.

It must be remembered, that common commercial phosphate of soda is seldom free from chlorides; to render it fit for the above operation nitrate of silver must be added, until the yellow precipitate remains permanent; it is then to be filtered.

I have ascertained that chromate of potassa, which would have the advantage of producing a deeper colored precipitate than phosphate of soda, cannot be substituted for it; in the first place, in consequence of its own coloration, but especially because of the cohesion of the chromate of silver, which has the inconvenience of offering too great a resistance to the action of the alkaline chlorides; moreover, its density, which is very superior to that of chloride of silver, causes its precipitation to the bottom of the vessel, so that the reaction of the chlorides on this salt, already very difficult, as we have said, becomes almost impossible to be performed rapidly and completely.

ESTIMATION OF THE SULPHATES.

I establish the amount of sulphuric acid in the sulphates by means of nitrate of lead; if the liquor contains a free acid, I neutralise it with an excess of white magnesia (removing the excess by filtration), then I add, by degrees, sufficient solution of iodide of potassium, of about 10 per cent. of iodide, for the first drops of nitrate of lead poured from the burette to produce at the point of contact a yellow precipitate, disappearing on slight agitation. We know we have finished the operation, as in the estimation of chlorine, by the permanence of the yellow coloration in the mass of precipitate after agitation. The manipulations are, therefore, exactly the same in both cases; they are very simple. I operate in a test glass, and agitate with a glass rod; here, as in the estimation of chlorine, the burette should indicate the tenths of a cubic centimetre.

To estimate chlorine, I use a liquor containing, per cubic centimetre, 0.03054 gr. of silver, representing 0.010 gr. of chlorine = 1 gr. per 100 cubic centimetres. That which I use in the estimation of sulphuric acid, contains 0.04133 gr. of nitrate of lead per cubic centimetre = 0.010 gr. SO_2 , or 1 gr. per 100 cubic centimetres, so that we obtain our results at once in hundredths.

These determinations, which may be depended on, within a half or even a quarter per cent., require only a few minutes.

It is evident, that to avoid confusion we should previously seek and eliminate, if necessary, those substances, fortunately but few, which are precipitated by these test liquors, as well as hydrochloric and sulphuric acids.

I must observe, in conclusion, that the principle on which these processes are based does not belong to me; I have only applied them in the manner that appeared best to me, to the particular cases of estimating chlorides and sulphates.

Journal de Pharmacie et de Chimie, February, 1857.

On the ACTION of IODIDE of STARCH on VARIOUS SALTS.

By M. F. PISANI.

IODIDE of starch poured into solutions of various salts, is decolorized in certain cases, whereas in others its blue color is unaffected. Thus, the salts of silver immediately decolor iodide of starch, but those of lead and copper have no action on it. The metals, besides silver, which produce this decoloration are—mercury, tin (stannous salts), antimony (the chlorides), arsenic (arsenious acid and chloride, As Cl^3), gold (perchloride), iron (ferruginous salts), and manganese (manganic salts.)

Iodide of starch enables us at once to distinguish the mercurous from the mercuric salts; for with the mercurous salts, there is decoloration, at the same time that the yellow iodide is precipitated, whereas with the mercuric salts there is decoloration without the formation of any precipitate.

Stannous and stannic salts, likewise behave quite differently with this reagent. Thus, the first alone have the property of causing it to lose its color. Arsenious acid and its corresponding chloride, likewise decolor iodide of starch. With arsenic acid there is no decoloration.

If we bring to the maximum of oxidation by nitric acid the metals which decolor iodide of starch, silver and mercury are the only two which retain that property. This decoloring action of these two metals on iodide of starch is easily explained in another manner, their great affinity for iodine. Vauquelin observed, that in triturating nitrate of mercury with iodine, iodide of mercury was formed, and probably also iodate. It is only necessary to place a crystal of iodine in a solution of mercuric nitrate to see the red iodide form on its surface, and the same time a still lighter white powder appears, which is the iodate. I have observed the same reaction with iodine and nitrate of silver; when put in contact with this salt, the iodine is quickly surrounded with a yellow aureola of iodide, the quantity of which gradually increases.

M. Deville has shown how strong an affinity exists between silver and iodine, since under the action of hydriodic acid the chloride of silver itself is decomposed. But the most curious thing is, that iodide of starch agitated with chloride of silver, very readily yields its iodine to it, and transforms it into iodide. Chlorine is found in the filtered liquor, and the blue color has disappeared.

Iodide of starch is the most sensitive reagent for ascertaining the presence of silver, should no mercury be present. Thus, in 100 c. c. of liquor containing $\frac{1}{10}$ th of a milligramme of silver, a cubic demi-centimetre of iodide of starch produces decoloration, whereas the same quantity produces perceptible coloration in 100 c. c. of pure water. In a smaller volume of liquor, we can recognise the presence of a $\frac{2}{100}$ of a milligramme of silver with iodide of starch.

We can instantly discover the presence of silver in commercial lead, by dissolving it in nitric acid, letting the liquor cool and then pouring

in a little iodide of starch. The excess of acid must be previously saturated with carbonate of lime, so that it should have no action on the iodine. A similar quantity of lead, containing no silver, becomes perceptibly colored by the addition of a few drops of this solution.

In these experiments I have used the soluble iodide of starch, prepared by Dr. Quesneville.

Comptes Rendus.

INORGANIC CHEMISTRY.

RESEARCHES on the SILICATES. By M. E. FREMY.

THE various combinations which silica forms with the different bases, and the important part which these compounds perform in the phenomena of nature, give them an interest which other salts are far from possessing; nevertheless, we are obliged to admit, that there are no saline compounds whose chemical history is more incomplete than that of the silicates.

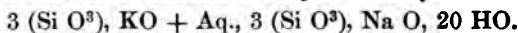
We have hitherto been content with making the analysis of the natural silicates; whose composition, in its infinite variety, appears to demonstrate that these bodies form an exception to the laws which regulate the composition of salts: and if the analysis of some double silicates proves, that the relation of the oxygen of the acid to the oxygen of the bases is the same as that found in the neutral sulphates, it has hitherto been impossible to confirm this result by synthetical experiments, and to reproduce artificially a series of silicates which can be considered as neutral.

The fundamental properties of the silicates have scarcely been examined; where is the chemist who can say, for instance, why certain silicates are attacked by acids, whereas others resist the action of the most powerful acids?

I have therefore thought that the study of the silicates should be recommenced, and that first we must determine the causes of the various capability of saturation of silicic acid; this is the object of the work, the first part of which I now present to the Academy.

My previous studies on the metallic acids have led me to the opinion, that the two isomeric states of silica which occasion such a great difference between quartz and silica prepared chemically, would also have an influence on the capacity of saturation of that acid, that is to say, on its power of retaining more or less of base; the following facts prove this.

When hydrated silica is combined with alkaline bases at a rather high temperature, under the influence of an excess of acid, we obtain a first series of silicates with the following characters:—These salts are soluble in water and crystallisable. We can however obtain them in an isolated state, by precipitating them with alcohol, or by evaporating the solution; which gives, by cooling, a gelatinous mass, which is easily pressed; these silicates are represented by the formula—



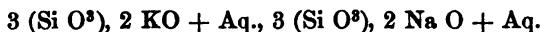
Under the influence of heat they undergo a very remarkable modification. When taken up again by water after a slight calcination, we perceive that the saline grouping which constituted them has been destroyed, for they no longer dissolve; they leave an insoluble siliceous deposit, and yield the alkali nearly pure to the water.

The state of the silica produced by this decomposition, appears to depend on the temperature at which the modification of the salt is produced; for when the calcination has been carried to bright redness, the silica obtained scratches glass, is unattackable by acids, dissolves only in alkalies in fusion, and presents the evident characters of quartz. If the calcination has been made at a lower temperature, on the contrary, the silica retains its solubility in dilute alkaline solutions; moreover, numerous experiments have shown me that the silica thus removed from a silicate simply by calcination, no longer retains traces of alkali, and is, in fact, chemically pure. This curious decomposition of the silicates may be attributed to a phenomenon of dishydration, or to an isomeric modification of the silica, which, in approaching the state of quartz, gradually loses its affinity for bases; in all these cases, it presents an interest which I will now render evident by some general considerations.

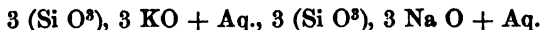
We are in possession of only very vague notions of the conditions which cause potassa to pass from feldspathic rocks, and to penetrate into vegetables. The preceding observations appear to me to throw some light on this important natural phenomenon; they demonstrate that there exists a class of alkaline silicates which may be compared to those furnished by the feldspaths, having so little stability as to deposit the silica under the most feeble influences, and thus set at liberty the alkali at first engaged in an alkaline combination.

A practice well known to agriculturists, *baking the earth*, proves, moreover, that argil often contains silicates decomposable by heat. The influence of *burnt clay* should be attributed, according to Liebig, to a decomposition of the silicates which sets the potassa at liberty, permits its assimilation by plants, and augments the fertility of certain kinds of soils.

The silicates of which I have spoken, may, even at the ordinary temperature, be modified under the influence of an excess of base, and be transformed into a new series represented by the following formulæ:—



These salts are more stable than the first; but may, nevertheless, abandon a portion of their base when subjected to the action of heat. When the preceding salts are boiled with an excess of alkali, they form a new series represented by the formulæ—



These silicates are uncrystallisable like the above, but much more stable; they are no longer decomposed by calcination, and preserve their solubility in water, even when heated to redness.

Finally, by calcining the preceding salts in a silver crucible with an excess of alkali, or by attacking quartz directly with potassa or soda in fusion, we form a last series of very stable silicates, which crystallise readily. Silicate of soda, which belongs to this series, and which may be obtained in voluminous crystals, is represented by the formula—



It is remarkable, that silica takes, under various influences, quantities of base which increase rapidly. Silica belongs then to that already numerous class of acids which I have endeavored to describe in other memoirs, whose capacity of saturation is variable, and depends in some sort on the quantity of base put in contact with it. These acids are truly *polybasic*, and, to have an exact idea of their capacity of saturation, they must be traced through the saline development which a powerful base induces in them.

The new facts which I have described in this extract, may be summed up in the two following propositions, of which I shall show all the consequences on another occasion. 1st. Silicic acid is polybasic, and forms various series of salts, which differ from each other in their general properties, and the quantities of base which they contain. 2nd. The saline groupings which form the first series, may be destroyed by the action of heat only, and give pure silica under both isomeric states; the last series, on the contrary, is very stable.

Comptes Rendus.

INDUSTRIAL CHEMISTRY.

THEORETICAL and PRACTICAL STUDIES in FIXING COLORS in DYEING. By M. F. KUHLMANN.

(Concluded from page 296.)

AFTER having ascertained the ease with which nitrogenous principles dispose thread and fabrics to absorb colors energetically, and having shown that other agents which do not form a chemical combination with the fabric may produce analogous effects, I wished to verify experimentally the value of the opinions broached by some authors who have endeavored to explain the reactions which take place during the dyeing of cotton with Andrinople red, in relation to the influence on this dye possessed by cow and sheep dung, which are frequently used.

With this dye the processes are very complicated, and the fixing of the color and its solidity depend on various circumstances; on the existence of an animal matter, on the combination of this matter with aluminous mordants, on the combination of the aluminous mordants with tannin; finally, on the employment of rancid olive oil; so that it becomes necessary for the purpose of showing the influence of nitrogenous matters, to study the isolated influence of each of these matters on the fixing of colors.

One particular fact fixed my attention.

When eggs are dyed to give the various colors of the *Easter eggs*, they are boiled in decoctions of various dye stuffs,—Brazil wood, Campeachy wood, onion peel, litmus, orchil, &c. All the colors are perfectly fixed, without the aid of any mordant, with this difference only, that some eggs take the color better than others. I imagined that in this case the fixing of the colors must be determined, not by the calcareous salt of which the eggshell is formed, but by a nitrogenous layer on its surface. This idea became a certainty by means of the following experiment.

I soaked the eggs for a few moments in diluted hydrochloric acid, taking the precaution to moisten only half the surface of each egg. By this contact the parts of the egg subjected to the action of the acid were covered by a white emulsive matter, which subsequent washing in water detached. The eggs thus treated, when dyed, only took the colors in those parts which had not been touched by the acid, when the carbonate of lime was still covered by its natural layer, which is analogous to coagulated albumen. Those parts of the egg which had been in contact with the acid remained perfectly white.

The powerful action of albumen, in causing the absorption of colors was proved to me by dyeing albumen, coagulated by heat, in baths of Brazil wood, litmus, orchil, &c. These curious results led me to try to augment directly the property of tissues of absorbing colors by the use of various animal matters. I made many experiments by preparing fabrics of cotton, wool, and silk, by immersion in a solution of albumen, and coagulating this albumen in the fabrics by the action of heat, or of an acid, previous to dyeing them.

I obtained very favorable results in dyeing cotton, rather less marked results in dyeing silks, and scarcely any appreciable difference with wool. My experiments were made with madder, and Brazil and Campeachy woods.

After trying albumen, I tried with equal success the action of milk and caseum, which may be coagulated on the surface of the fabrics by means of an acid. Milk especially, whether alone or mixed with mordants, gave me very rich colors.

Finally, I tried gelatine; but in this last case I produced coagulation with tannin. I thus obtained good, although not marked results, with the aid of mordants. I proved in these last experiments that gelatine, which fixes tannin abundantly on the fabric, may be used with great benefit in dyeing grey or black by means of the salts of iron. The colors which I thus obtained had a very good body.

I completed these investigations by submitting to an attentive examination some coagulable nitrogenous matters, as a means of perfectly fixing on fabrics the metallic oxides, even those which decompose but sparingly in contact with the fabrics.

Numerous comparative experiments were made with acetate of alumina, chloride of manganese, sulphate of zinc, sulphate of copper, sulphate of protoxide of iron, perchloride of mercury, and chloride of platinum.

By using Brazil wood we obtained the following results:—

The natural cotton, without a mordant, took a pale violet red color, and the albuminised cotton a deep violet red color.

The intervention of metallic salts manifested themselves in the following manner, in the same dyeing bath.

The natural cotton, after immersion in a solution of metallic salts, washed immediately and dried, took with acetate of alumina a reddish brown. The albuminised cotton, treated in the same manner, took a reddish violet of a deeper tone.

With chloride of manganese the natural cotton took a carnation color,—the albuminised cotton an almost black carnation.

With sulphate of zinc, the natural cotton was of a pale reddish violet—the albuminised cotton a dark violet.

Sulphate of copper gave nearly the same results.

Sulphate of protoxide of iron gave a red violet on the natural cotton, and a violet black on the albuminised.

With perchloride of mercury the natural cotton was dyed crimson—the albuminised black, with a red tinge.

With chloride of platinum the natural cotton was a dirty reddish brown—the albuminised, the same color darker.

The same experiments were repeated with madder as the dye, analogous results were obtained, but the differences were less marked; in fact the colors were the same, only of a deeper tint on the albuminised cotton.

All these experiments prove that albumen when applied uniformly to the surface of cotton fabrics will aid in fixing the colors of madder and Brazil wood on them, in the same manner as a mordant, but in a less powerful manner, and that it will also aid in precipitating on the fabric various metallic oxides, with which it forms insoluble compounds.

In dyeing, the fabrics impregnated with these combinations absorb colors more readily than if the latter were prepared either with albumen, or with the metallic salts only.

Analogous results are produced when tannin is fixed by means of gelatine. This latter body is well applied to dyeing black, by producing a combination of tannin and oxide of iron. Tannin alone is very useful to fix acetate of alumina, which it decomposes, which produces very rich tones.

As the result of all these experiments on fixing colors in dyeing, I consider that the following propositions are rendered indubitable.

1. Cotton or linen transformed into pyroxyline are not more susceptible to dyeing.

2. When pyroxyline, by means of spontaneous decomposition, has lost a portion of its nitrogenous principles, it not only has lost all resistance to dyeing, but it absorbs colors with greater readiness than ordinary textile fabrics.

3. By the combined action of nitric and sulphuric acids, we are enabled to give to cotton a disposition to absorb colors in dyeing as powerfully as that possessed by spontaneously decomposed pyroxyline.

4. Caustic potassa and soda, sulphuric and phosphoric acids, also augment the readiness with which cotton absorbs colors.

5. Other alterations or modifications of cotton, by ammonia, chlorine, hydrochloric acid, and hydrofluoric acid, with or without the aid of heat, do not give it analogous properties.

6. The neutral animal matters may be usefully employed as intermediaries in fixing colors on threads or fabrics, and to vary the nature of mordants.

This property is peculiar to them—the presence of nitrogen in the number of their constituents would not explain their readiness to receive dyes; for there are nitrogenous matters, such as uric acid and the urates, in which the property of absorbing colors in dyeing does not exist.

7. Dyeing depends essentially on a chemical combination—the natural or variously combined and modified fabric; the physical state of this matter has no influence on the phenomenon but that of an accessory.

It is moreover difficult to distinguish what belongs to chemical affinity—properly so called—from that which is the result of cohesion; in the dyeing of charcoal, for instance, what proceeds from the chemical properties of this body, and what is the result of its porosity?

In most cases these two actions combine to give one result, and are in some sort confounded.

Comptes Rendus.

On DECOLORISING CHARCOALS, and their Power of Absorbing some of the GASES.

By JOHN STENHOUSE, LL.D., F.R.S.

THE singular property which vegetable charcoal possesses of removing odors and coloring matters from solutions, was first observed by Löwitz towards the close of the last century. In 1811, Professor Figuier, of Montpellier, ascertained that animal charcoal, or bone-black, as it is called, was immensely more efficacious as a decoloriser than any of the ordinary kinds of vegetable charcoal. M. Figuier's discovery was speedily confirmed by numerous other experimenters, and soon afterwards applied to the refining of sugar, for which bone-black has ever since formed one of the most important reagents.

The way, however, in which charcoal acts as a decoloriser, remained involved in considerable obscurity till 1822, when three prize essays on the subject were published by MM. Bussy, Payen, and Desfosses. M. Bussy's memoir was particularly valuable, and threw much light on the subject. Those of Payen and Desfosses, though less systematic and practical than M. Bussy's memoir, agreed with it in its general conclusions. These were chiefly that the decolorising power of charcoal, though inherent in the charcoal itself, is entirely dependent on its physical state, especially on its extreme porosity and minute state of division. Thus, charcoal which has been strongly heated so as to become hard and lustrous by undergoing a species of fusion, scarcely exhibits any decolorising power. On the other hand,

charcoal, such as bone-black, which contains much saline and earthy matter, such as phosphate of lime, &c., as its particles are prevented from agglutinating, yields a dull non-lustrous material, possessing very considerable decolorising power. Bussy found also that the distinction which had previously been drawn between vegetable and animal charcoals as decolorisers, was erroneous, and that charcoals, whether of animal or vegetable origin, should be divided into two classes, compact and lustrous, and porous and non-lustrous.

Till of late only two species of decolorising charcoal have been employed in the arts. First. Bone or ivory black, made by calcining bones in close cylinders till the organic matter they contain is entirely carbonised. This is by far the most common of all the decolorising charcoals, and is the only one employed in sugar-refining, and for decolorising similar neutral solutions. It usually contains little more than 10 per cent. of carbon, and about 90 per cent. of phosphate and carbonate of lime. The second species of decolorising charcoal is the so-called purified animal charcoal, which is made by digesting bone-black in hydrochloric acid, and edulcoration with water till all the lime-salts are removed. Purified animal charcoal, when carefully prepared, may be regarded as nearly pure charcoal, which has a dull lustrous appearance, and is extremely porous, provided that, after purification, it has been dried at a temperature little exceeding 212° F. It decolorises neutral and acid solutions exceedingly well; but after being heated to redness, its texture becomes compact, and its decolorising power is almost entirely destroyed. A second process for purified animal charcoal consists in intimately mixing blood or the fleshy parts of animals with pearlash, and then calcining them in close vessels. The alkaline salt is first washed out with pure water, and then the last traces of the potash and lime-salts are removed by digestion with hydrochloric acid. The purified animal charcoal obtained in this way is even more efficacious than that produced from bone-black. Such, then, was the state of our knowledge in regard to decolorising charcoals till about eighteen months ago, when I first directed my attention to the matter.

As the cost of purified animal charcoal—about two shillings per pound—is very considerable, I was induced to procure an economical substitute for it, which could also be employed in acid solutions. This I effected by combining alumina with ordinary vegetable charcoal. The following is the process employed. Fifty-four parts of the sulphate of alumina of commerce, which is made by digesting the purest kinds of pipe-clay in pretty concentrated sulphuric acid, and which usually contains about 14 per cent. of alumina, were dissolved in water and digested with $92\frac{1}{2}$ parts of finely-powdered ordinary wood charcoal. When the charcoal had been thoroughly saturated with the solution of sulphate of alumina, the mass was evaporated to dryness. It was then introduced into covered Hessian crucibles, or large muffles, and was heated to redness till all the water and acid had been dissipated. In this way a decolorising charcoal was obtained, which, though it still appeared perfectly black, was thoroughly impregnated

with anhydrous alumina. When sulphate of alumina and charcoal were employed in these proportions, a decolorising charcoal was obtained, containing almost exactly seven and a half per cent. of alumina, which is the quantity of alumina which I have found, after various trials, to be the most effective, as it appears to be exactly the amount requisite thoroughly to coat all the cells of the charcoal; for, on increasing the quantity of alumina above seven and a half per cent., the charcoal did not gain any additional decolorising power; and on diminishing the amount of alumina below seven and a half per cent., the decolorising power of the charcoal was also diminished. After calcination in the way described, on being again carefully pounded, the aluminised charcoal was ready for use. A more economical method of preparing aluminised charcoal is, instead of employing dried sulphate of alumina to prepare a solution of sulphate of alumina of known strength, by simply digesting calcined pipe-clay in pretty concentrated sulphuric acid. Such an amount of the solution is to be employed as shall impregnate the charcoal with seven and a half per cent. of alumina. In preparing sulphate of alumina, the chief point to be attended to is to employ a clay as free from iron and lime as possible, though the presence of a trace of iron is quite immaterial. A clay containing a considerable quantity of lime may likewise be made available, by previously removing the lime by digestion with hydrochloric acid. The charcoal, which is now produced in large quantity by the destructive distillation of sawdust for pyroligneous acid by Halliday's patent process, being almost a waste product, is exceedingly well adapted for aluminising.

The aluminised charcoal may be employed to decolorise all acid solutions except those containing much free sulphuric acid; for, after being heated to redness, the alumina becomes so compact, that it is not soluble in any but pretty concentrated sulphuric acid. Aluminised charcoal decolorises tartaric and citric acids quite as efficiently as either bone-black or washed animal charcoal, i. e., bone-black digested for some time with hydrochloric acid, while it is much cheaper than washed animal charcoal, and does not introduce nearly so much inorganic matter into the tartaric acid mother liquors. The price at which aluminised charcoal can be produced, does not exceed that of bone-black. The only two points required to be attended to, with regard to aluminised charcoal, are to employ it in very fine powder, and to boil it for a few minutes with the solution it is wished to decolorise.

I have repeatedly decolorised solutions of crude tartaric and citric acids with aluminised charcoal, and have invariably found that the aluminised charcoal possessed an equal decolorising power with washed animal charcoal—the material usually employed by tartaric acid manufacturers. I likewise found that the aluminised charcoal introduced a very small quantity indeed of inorganic matter into the mother liquors, compared with that which always results from the employment of either washed animal charcoal or bone-black, as may be seen by the subjoined experiments. Equal quantities of crude tartaric acid

were decolorised with the same amounts of bone-black, washed animal charcoal, and aluminised charcoal. When the resulting solutions were evaporated to dryness, they yielded, on incineration, the following quantities of fixed residue:—

Aluminised charcoal	0.32 per cent.
Ivory black	7.58 “ “
Washed ivory black	3.84 “ “

It is evident, therefore, that aluminised charcoal is especially well adapted for the decolorising crude tartaric and citric acids, as it introduces such a very small quantity of inorganic matter into the mother liquors. And, doubtless, it is owing to their employing washed animal charcoal, and sometimes even bone-black, that the tartaric acid manufacturers find their mother liquors so intractable from the large quantity of inorganic salts they contain.

An artificial bone-black may likewise be produced by impregnating powdered wood charcoal with basic phosphate of lime, dissolved in an excess of hydrochloric acid, so as to impregnate the charcoal with seven and a half per cent. of bone earth, instead of eighty per cent. as in ordinary animal charcoal. It is manufactured by a precisely similar process to that already described, the acid and water being driven off by ignition in close vessels. It decolorises very well, but can only be employed in neutral solutions. It is evident that both aluminised and artificial phosphate of lime charcoals, just described, are merely mordanted charcoals, which decolorise solely by the bases or mordants which they contain.

Besides the aluminised charcoal, I have also prepared an additional substitute for purified animal charcoal, by means of hydrate of lime and a mixture of coal-tar pitch and coal-tar. The following is the process employed for this purpose. I take one pound by weight of coal-tar pitch, and gently heat it in an iron pot till it melts. I then add two pounds of fluid coal-tar, and mix the liquids. Seven pounds of hydrate of lime in very fine powder are next gradually stirred into the mass, which soon becomes thick and pasty. It is then gently roasted, the stirring being continued during the whole time, till it is reduced to the state of a very fine powder. The great object of the stirring and roasting being to incorporate the lime with the vegetable matter as intimately as possible. The dark brown powder obtained in this way, is then introduced into ordinary covered crucibles, or iron retorts, and is ignited, until the whole of the vegetable matter is entirely carbonised. The mixture when cold is then digested with dilute hydrochloric acid, thrown upon a filter, and finally washed with distilled water till everything soluble is removed. In this way an extremely porous, very light, nearly pure charcoal is obtained, which for some purposes, such, for instance, as decolorising logwood and similar solutions, is four times as effective as the most carefully prepared purified animal charcoal. It likewise decolorises impure gallic acid solutions admirably. This charcoal from coal-tar may likewise be prepared with much weaker hydrochloric acid than is

required to remove the phosphate of lime from bone-black. Quick-lime in fine powder may likewise be employed instead of hydrate of lime, and also calcined magnesia and the light sub-carbonate of magnesia of the shops, but chalk, even when finely powdered, answers very badly indeed. Instead of coal-tar and coal-tar pitch, some other vegetable substances, such as maize, wheat, and the various kinds of flour, common resin or colophonium, pitch, wood-tar, asphalt, or bitumen, may be employed. When flour, resin, &c., are intimately mixed with carbonate of potash and calcined in close vessels, very good decolorising charcoals are obtained, but I was surprised to find, that carbonate of soda, either hydrated or dried, did not produce a similar result.

As was first observed by Messrs. Bussy and Payen, I find that every species of decolorising charcoal acts with varying efficiency on each colored solution to which it is applied. Thus, for instance, one charcoal decolorises a solution of indigo better than it does one of logwood, syrup, or tartaric acid, while another charcoal will decolorise a solution of logwood more readily than one of molasses; and so of others.

As the result of this investigation, I think that decolorising charcoals may be very properly divided into three classes: First, into those which, like the purified and the coal-tar charcoals, above described, may be regarded as merely pure charcoals in an extremely minute state of division, and which decolorise by their porosity alone. Second, into those mordanted charcoals which, like aluminised charcoal, and artificial phosphate of lime charcoal, decolorise solely by the mordants or bases which they contain. Third, into bone-black, which is a composite charcoal, and decolorises, partly, by the large quantity of phosphate of lime, and partly by the ten per cent. of minutely divided charcoal it contains.

The propriety of this classification may be made very plain by a simple experiment. If equal quantities of purified animal or coal-tar charcoal, aluminised charcoal, and bone-black, are boiled in decoctions of logwood till they are saturated with coloring matter, and if they are then thrown upon filters, which are washed with dilute solutions of aqua-ammonia, the ammoniacal solution from the aluminised charcoal will be of a straw, yellow color; that from the bone-black will be somewhat deeper; while those from the purified animal and coal-tar charcoals, will be nearly as deep as ink. This evidently shows, that in the case of the aluminised charcoal, the coloring matter was in true chemical combination with the alumina. In the bone-black, this was also partially the case, while in the purified animal charcoal, no true chemical combination had been formed, but the coloring matter was merely retained by the porosity of the charcoal.

I shall conclude this paper by shortly noticing some experiments on the power which the various decolorising charcoals possess of absorbing three of the gases, viz., ammonia, carbonic, and hydrochloric acids. Their results, which are somewhat anomalous, I have thrown into the subjoined tabular form. A gramme of each charcoal was employed.

All the charcoals were decolorisers, except the platinised charcoal, which, as it consists of common wood charcoal, impregnated with a small quantity of metallic platinum, of course possesses no affinity for coloring matters.

Cubic Centimetres of Gases absorbed by One Gramme of various kinds of Charcoal.

NAME OF CHARCOAL.	NAME OF GAS.		
	Ammonia.	Carbonic Acid.	Hydrochloric Acid.
Common Wood . . .	145	15	158.7
Common Animal . . .	105	2.5	Not tried.
Purified Animal . . .	210	No absorption.	185
7½ per cent. Aluminised . .	212.5	12.5	177.5
7½ per cent. Phosphate of Lime .	152.5	10	Not tried.
Pure Coal-tar Charcoal . .	386	No absorption.	180
5 per cent. Platinised . .	142.5	No absorption.	135

In connexion with the absorbant power of charcoal for gases, I may mention that the charcoal air-filters, respirators, and bandages for putrid wounds, described by me in a former paper nearly three years ago, are now coming into very general use.

Saint Bartholomew's Hospital, Dec. 6th, 1856.

Pharmaceutical Journal, January, 1857.

METEOROLOGICAL CHEMISTRY.

Of the QUANTITY of NITRIC ACID and AMMONIA in RAIN-WATER.

By J. T. WAY, 15, Welbeck Street, Cavendish Square.

It will be in the recollection of the readers of this Journal, that, in the Midsummer number for 1856, I published a paper "On the Composition of the Waters of Land-drainage and of Rain," in which I gave analyses of the rain-water of each month in the year 1855, collected at Rothamsted, and kindly supplied to me for the purpose by Mr. Lawes.

In order to save the necessity of reference, I will very briefly state the results of that examination. It should be premised that up to the time from which the investigation in question dates, the methods in existence for the determination of the very minute quantities of nitric acid and ammonia present in rain-water were of so faulty and incomplete a character, that no reliance could be, or by competent judges was, placed in the few isolated results which had been published on this head; and opinions on the amount of influence which the nitrogenous elements of rain-water might exercise on vegetation, were loose and speculative in the extreme. By the aid of entirely new and

highly refined methods of analysis, I was enabled to ascertain with certainty what quantity of nitric acid and ammonia was brought down to the soil by the rain-water of each month in the year 1855. It was found generally :—

1. That the total quantity of nitrogen brought down in the form of rain was much smaller than had been previously supposed, and that it was altogether insufficient to account for the amount of produce obtained naturally in uncultivated and unmanured soils.

2. That of the nitrogen so brought down by rain, very much the larger proportion existed in the state of ammonia.

3. That, as a rule, both the ammonia and nitric acid falling in rain in any one month were in direct proportion to the quantity of such rain, modified only to a certain extent by the number of occasions on which rain had fallen.

4. That nitric acid was found in the rain of each month of the year, and consequently, if it be the result of electrical action, such action must be continuously in exercise and not confined to special seasons.

Such were the general conclusions which seemed justified by the analysis of the rain-water of different months in the year 1855. It seemed desirable that this investigation should be extended over a second year, and accordingly, by the help of Mr. Lawes, who has a second time placed samples at my disposal, I am in the position to publish analyses of the waters of each month of the present year (1856), together with those of a few selected samples of the rain of thunderstorms, &c., which seemed likely to throw light upon the formation of these compounds in the air.

The following Table shows the quantity of nitric acid and ammonia per gallon of rain-water during each month of the year 1856, and, for the sake of comparison, the figures before published for the year 1855 are given in another column.

TABLE I.—Nitric Acid and Ammonia in Rain-Water.
(Grains in the Imperial Gallon).

	Ammonia.		Nitric Acid.	
	1855.	1856.	1855.	1856.
January	0·092	0·079	0·017	0·025
February	0·104	0·136	0·042	0·018
March	0·086	0·098	0·021	0·035
April	0·123	0·146	0·035	0·018
May	0·080	0·127	0·035	0·028
June	0·135	0·113	0·080	0·047
July	0·061	0·085	0·017	0·085
August	0·080	0·070	0·060	0·036
September	0·095	0·121	0·021	0·035
October	0·061	0·060	0·036	0·032
November	0·054	0·080	0·018	0·043
December	0·067	0·080	0·017	0·040

The differences in the quantities of ammonia and nitric acid of the two years are only such as would be anticipated from the circumstan-

ces of the case, whilst the general resemblance of the figures is a sufficient proof of their correctness. But little information, however, is to be obtained from these results, except in connection with the quantity of rain falling. The following Table exhibits the rainfall per acre, with the quantity of nitric acid and ammonia contained in it. The fourth column shows the quantity of nitrogen in both these compounds.

The reader who will take the trouble to compare this Table with the corresponding one for 1855 in the last number of the Journal, will find that, although there are some differences in the two, the present figures substantially bear out my previous results. The total amount of nitrogen present in both its compounds is somewhat greater in the rain-water of the year 1856 than in that of last year, but not to such an extent as in any way to modify the practical conclusions to which we were led when detailing the former experiments. It would be a waste of time therefore to go over these arguments again. It is quite obvious that the nitrogen of *rain* is not adequate to account for the influence of the atmosphere as a source of nitrogenous manure to the soil and plants.

TABLE II.—Nitric Acid and Ammonia in Rain-Water per acre, 1856.

	Gallons of Rain.	Nitric Acid in Grains.	Ammonia in Grains.	Total Nitrogen in Grains.
January	62,952	1561	5005	4526
February	30,586	544	4175	3579
March	22,722	806	2108	1945
April	59,083	1063	8614	7869
May	106,474	3024	18313	15863
June	48,253	2046	4870	4540
July	83,561	1191	2369	2670
August	59,859	2125	4214	4021
September	49,477	1756	5972	5373
October	65,033	2075	3921	3767
November	82,181	1371	2591	2489
December	50,870	2035	4070	3352
Total in lbs. the whole year		2·80	9·53	8·31

In addition to the samples for each month of the year, Mr. Lawes has kindly supplied me with a few specimens, which might be supposed to give interesting results—they are as follows:—

June 20.—Heavy thunder-rain after about a week of showery and apparently thundery weather, but without actual sound of thunder. This was the largest fall in any day of the year (0·9676 of an inch). It contained—

Nitric acid 0·035 grains per gallon
 Ammonia 0·091 „

This is if anything rather below the average quantity of both substances in the rain of the year.

August 9, A.M.—Thunder-rain after very hot dry weather for 8 or 9 days (0·1782 of an inch in the day)—

Nitric acid 0·089 grains per gallon

The quantity of water at my disposal did not allow in this case, and others that follow, of an estimation of ammonia.

August 9, P.M.—Thunder-rain, moderate fall—

Nitric acid 0·0355

August 11.—Heavy thunder, small fall of rain (0·0710 of an inch)—

Nitric acid 0·1243

These last examples will illustrate the fact that the percentage of nitric acid is in inverse relation to the fall of rain: when the latter is large, we have a small proportion per gallon of nitric acid; on the other hand, when the rain is small, we find a higher *relative*, though not a larger *actual*, quantity of nitric acid.

The nitric acid of the thunder-rain is certainly very high, but, as will be seen shortly, it is not very much greater than the water of mist and fog, and we are hardly justified in considering these results as corroborating the theoretical explanation of the production of nitric acid by electrical action.

September 2.—Thunder-rain—

Nitric acid 0·035

October 22.—Frost, mist, fog, &c.—

Nitric acid 0·071

October 27.—Fog, mist, &c.—

Nitric acid 0·071

October 28.—Fog and mist—

Nitric acid 0·089

October 30.—Fog, mist, and little rain—

Nitric acid 0·088

November 27.—Snow which fell on the 26th, melted, and collected with rain on the 27th, large total fall—

Nitric acid 0·053

Ammonia 0·654

The quantity of ammonia in snow is here seen to be comparatively very large, a circumstance which has been repeatedly observed, and which was confirmed by the late experiments of M. Boussingault.

December 5, A.M.—Snow which fell on the 2nd and 3rd, melted, collected with rain on the 5th, moderate fall—

Nitric acid 0·0461

December 5, P.M.—Little rain, very misty all day—

Nitric acid 0·053

It appears to me that all that we can legitimately conclude from these results is, that neither in the case of nitric acid nor ammonia is the quantity present in the air sensibly increased by such influences as electricity, heat, &c.; for so far as the water of rain is concerned, these compounds of nitrogen are evidently inversely proportionate to the quantity of water falling. In the cases of thunder-rain, the fall being slight, we have a large proportion of nitric acid, although even then not much greater than is found in the water of fogs and mists where the influence of heat or electricity is not supposed. Where, however, a fair quantity of rain falls accompanied with thunder, we find only the average proportion of nitric acid. The refreshing influence on vegetation of a thunder-shower is due to the much-needed water as water, and not to its being a vehicle of nitrogenous manure.

Journal of the Royal Agricultural Society.

PHYSICS.

On the ELECTRICITY of TOURMALINES; RELATION between the QUANTITY of ELECTRICITY DEVELOPED and the RAPIDITY of the COOLING.

By M. S. M. GAUGAIN.

THE quantity of electricity which a tourmaline can produce, when cooled between two given limits of temperature is far from invariable, it depends essentially on the rapidity with which the cooling is effected. To ascertain this fact, it is only necessary to count the number of charges which a tourmaline can communicate to the electroscope with gold leaves, first letting it cool gently in the open air, and then hastening the cooling by means of a current of air from a bellows, the initial temperature of the tourmaline being supposed the same in both cases, the number of charges obtained is perceptibly more considerable when the cooling is hastened by a current of air.

From this, it appears natural to suppose, that the quantity of electricity produced by a tourmaline in cooling, is a direct action of the rapidity of the cooling, and as this rapidity constantly diminishes in proportion as the temperature of the tourmaline lowers (at least, if the cooling takes place in a tranquil atmosphere), it results that, in this case, it is the origin of the cooling which should produce the maximum of electricity; it appeared to me interesting to ascertain whether this was the fact. This question, I should observe, has been already treated by M. Becquerel, and he has been led to admit, that "the electric intensity of each pole is not in the ratio of the rapidity of the cooling;" but the method which he adopted did not appear to me likely to show the relations which, perhaps, may exist between the quantity of electricity developed and the rapidity of the cooling. This method consists in causing the tourmaline being cooled, to oscillate between the poles of a dry battery; now, it is clear that the tension deduced from the number of oscillations, will by no means represent the quantity of electricity produced at the moment of the

observation. After heating a tourmaline to 115°C . (219°F .) it is allowed to cool to 60°C . (140°F .), the tension which it possesses at 60°C . does not proceed solely from the quantity of electricity produced at temperatures near 60°C ., it results from all the quantities of electricity developed since the commencement of the cooling; and if we could remove all causes of loss, it is evident *a priori* that the tension would continue to increase as the cooling continued, whatever might be the relations between the development of electricity and the rapidity of the cooling. Consequently, I think that the method of oscillations is useless in studying these relations. The process of experimenting which I have adopted, is indicated in my previous note (see p. 309), it consists in putting the tourmaline on which we operate on the one hand in connection with the earth, and on the other with an electroscope of gold leaf, and to note the intervals of time which separate the successive discharges of the gold leaves. It is clear, that the development of electricity is so much the more rapid as the discharges are multiplied, and that the quantity of electricity produced at a given time, during the cooling, is in inverse ratio to the duration of the charge which corresponds at the moment considered.

I have tried this method in a great many experiments, and the general results may be formulised as follows:—

1st. When the temperature of the tourmaline does not surpass the limit beyond which this stone ceases to be isolating, about 150°C . (302°F .), electricity is manifested from the commencement of the cooling, and the duration of the charges of the electroscope at once reaches its minimum value; this value increases more or less rapidly, according to the size of the crystals. When we use very large tourmalines, the development of electricity remains nearly constant during the first moments of the cooling.

2nd. When the tourmaline used in the experiment has been brought to a high temperature, 300° or 400°C . (572° to 752°F .), and allowed to cool, a certain time elapses without the production of any sign of electricity; afterwards, when the electricity begins to manifest itself, we see that during a shorter or longer time, the duration of the charges of the electroscope diminishes. This duration, after attaining its minimum, then augments as in the former case.

3rd. Finally, when the tourmaline on which we operate is voluminous, and when it has not remained long enough in the stove for its whole mass to be of one temperature, we see, as in the preceding case, that the duration of the charges diminishes during the first moments of the cooling, even when the stove has not been heated beyond 150°C . (302°F .)

These various results are easily explained, when we admit the principle that the electricity developed is a direct function of the rapidity of cooling, and when we take into account the variations of conductivity which are produced at a high temperature.

In the first case, the conductivity being regarded as *nil* during the whole experiment, the quantity of electricity produced depends only on the rapidity of cooling, and should diminish as the rapidity of the cooling diminishes; such is really the result observed.

In the second case, two opposite effects are at first produced; in proportion as the temperature lowers, the rapidity of the cooling diminishes, and according to the principle admitted, the quantity of electricity should diminish; but, at the same time, the conductivity of the tourmaline, which is very considerable towards 400°C . (752°F .), decreases as we approach the limit at which this stone can be considered perfectly isolating, and consequently the loss becoming greater, the production of electricity evidenced by the electroscope should augment. It is easy to comprehend that, under the influence of these two antagonistic causes, the duration of the charges of the electroscope should first diminish and afterwards increase.

In the third case, the conductivity may be regarded as *nil* during the whole of the cooling, and consequently, the increase at first manifested in the production of electricity, cannot in any way be attributed to the cause which I have just indicated; it depends exclusively on the inequality of the temperature. The surface of the stone being supposed to be originally hotter than the interior, in the first moments of cooling the layers situated at a certain thickness continue to heat while the surface cools. Whence it results, that the electricity transmitted to the electroscope, is only the difference between the electricity proceeding from the cooling of the surface, and the electricity of the contrary kind produced by the heating of the interior; later, when the temperature is decreasing throughout, the electricity collected results from the cooling of the entire mass, and although the superficial layers, taken separately, furnish less electricity than at first, it is easy to comprehend that the quantity transmitted to the electroscope may be definitively more considerable, the electricity of the interior having changed its pole.

These considerations may likewise assist in accounting for the almost perfect constancy which the development of electricity presents during a certain time, when we operate on a large tourmaline whose temperature is uniform and not above 150°C . (302°F .); in this case, the whole mass cools at once, as soon as cooling commences; but the layers, situated at different depths, do not attain their maximum speed of cooling at the same moment; in the superficial layers this maximum is found from the commencement of the cooling; the internal layers on the contrary, do not at first, cool at all, and they reach their maximum rapidity all the later as they are deeper in. Whence it results, that if the electricity of the external layers is constantly diminishing, the electricity furnished by the internal layers, on the contrary, augments for a certain time, and it may be imagined that these variations in opposite directions may compensate for each other, so as to produce the regular development of electricity which we have observed. To appreciate how far this regularity goes, I will quote the numbers obtained in the first portion of an experiment executed with a very large tourmaline; the numbers express the seconds which elapsed between the successive discharges of the electroscope:—23, 29, 29, 29, 29, 29, 29, 29, 29, 29, 30, 30, 31, 32, 34, 34, 33, 35, 36, 42, &c.

Comptes Rendus.

On the ELECTRICITY of the AIR and the EARTH, and on the CHEMICAL EFFECTS PRODUCED by VIRTUE of SLOW ACTIONS, with or without the CONCURRENCE of ELECTRIC FORCES.

By M. BECQUEREL.

THIS work, which embraces the sources of terrestrial electricity and the physical effects, as well as slow actions, is composed of five distinct parts, forming as many separate Memoirs, and which treat, the four first—

- 1.—Of the electrical state of gases and vapors.
- 2.—Of the electrical effects produced by the contact of earth and waters.
- 3.—Of terrestrial pairs, with constant currents.
- 4.—Of storms.

The fifth part treats of crystallised compounds, formed in slow actions, with or without the aid of electrical force.

First Part.—The fundamental principle which ought to guide us in investigations as to the natural sources of electricity, as well as other sources, is the following:—All mechanical molecular work, physical or chemical, disengages electricity; in other terms, the equilibrium of the molecules cannot be disturbed without that of the electricity being so likewise. In the same manner, electricity cannot be put in motion without the manifestation of molecular actions, proved by an elevation of temperature, a chemical decomposition or magnetic action.

All the combustible conductors of electricity, disengage electricity during their combustion; the combustible body frees negative electricity, the body necessary to combustion, positive electricity; this fact, has been put beyond a doubt by M. Pouillet with regard to charcoal. I demonstrate in this work that the effects are the same, whether the bodies are conductors or non-conductors of electricity.

The change of state of bodies, and evaporation, properly so called, do not give electricity, as has been long observed. This negative result appears to imply a contradiction to the above cited principle, namely, that there is a disturbance of the equilibrium of electric forces, whenever there is productive molecular work. Why are there no electric effects in these cases? Does there not take place a something similar to what occurs when two bodies are rubbed uniformly against each other? There is no electricity, because there is no reason why one molecule should take electricity more than another. But if no electricity is disengaged, there is a production of heat. On the other hand, we know that the friction between two similar bodies is more powerful than between dissimilar bodies. We likewise know, that changes of state are accompanied by powerful calorific phenomena; moreover, there appears to exist such a dependence between the production of heat and that of electricity, that when one augments, the

other diminishes, and *vice versé*; so that one may disappear at the expense of the other. Some examples will prove this:—When electricity circulates in a metallic wire, the more heat is disengaged the less electricity passes through, and *vice versé*; this occurs as though the electricity were transformed into heat. When we collect the heat disengaged in a pair of copper and amalgamated zinc, the circuit being formed, we find that the sum of the heat produced in the chemical reaction of acidulated water on the zinc and conducting wire is constant, so that the quantity of heat disengaged in the reaction is greatest when the least electricity passes through the wire, and *vice versé*.

I have shown that, in the friction of two similar bodies, so performed that one of them heats more than the other, there is a disengagement of electricity, and the body which heats the most takes negative electricity. We have even thence inferred, that in the friction of two dissimilar bodies, generally the one which disengages most heat has a negative tendency. All these facts show an intimate relation between the production of heat and that of electricity, productions which I have always considered as concomitant, and which may be such, as one is annulled at the expense of the other. It is therefore nothing astonishing that in the changes of state, the electric effects disappear in the presence of calorific effects.

These considerations support the chemical theory of the disengagement of electricity in the battery, the theory which I have always maintained for more than 30 years, namely, that contact alone will not disengage electricity, and that for this purpose some molecular work is necessary.

But, if the evaporation of water, or of a solution in a platinum capsule, at the temperature of ebullition and below that, does not produce electricity, it is not the same when we project, with a pipette, some drops of the liquid into a capsule sufficiently heated to produce immediate evaporation, there is then a production of electricity; the vessel becomes negative to such a degree that it can be observed without a condenser; this effect is due to the friction of the water, at the instant when it becomes vapor, against the inner surface of the capsule, and is of the same kind as that observed in Armstrong's experiment.

Gases are electrical when disengaged abundantly from a liquid, hydrogen and ammonia take an excess of negative electricity, carbonic acid and oxygen an excess of positive electricity; these effects are due, not to a chemical decomposition, for the results would be inverse, but to a friction of the gases against the liquid.

Should we not infer from this, that by an effect of the same nature the oxygen and carbonic acid exhaled from the leaves of plants, take positive electricity with them into the atmosphere.

Second Part.—The electrical effects produced on the contact of earth and fresh or salt water, have been observed successively with gold and platinum leaf, and with these same leaves covered with powdered sugar candy charcoal, to retard as much as possible, the polarisation

produced when the circuit is closed, so as to measure the effects; the charcoal then acts on the deposited gases by its absorbant power. In the circuit was placed a needle.

Numerous experiments made in the Jura, in Loiraine, in the Nivernais, and on the sea coasts, in Belgium and in France, have proved that vegetable earth is always positive in its contact, whether with fresh or sea water; that the electrical effects are very weak between the waters and the adjacent earths, when they are permeable by the water; thus, during the inundations of the plains of the Saone, we obtained scarcely perceptible currents by establishing a metallic communication between the Saone and the land which had been inundated.

In the salt marshes, as might be expected, the earth is negative to fresh water.

I have found that electromotive force, in contact with sea water and vegetable earth, is about 2.4 greater than that produced by contact with the same earth and fresh water, all other things being equal.

These experiments were followed up last winter at the *Jardin des Plantes*, night and day for three months, by putting the water of one of the wells in the establishment in connection with the surrounding earth. We have observed variations and inversions in the way of the currents, due to the infiltrations of the water, containing substances which are not always of the same nature, in the wells of large cities.

Third Part.—Experiment having demonstrated that we can only depend on the results when the electrodes, prepared with charcoal, had been placed for several days in contact with the earth and water, I found myself obliged, to prepare an observatory during last summer, in a part of the department of the Loiret, where I collected the greater part of the materials mentioned in the work which I now present to the Academy.

On comparing the terrestrial pair composed of water, salted to 3 areometrical degrees, of good vegetable earth, and of two electrodes of retort charcoal, with a pair of sulphate of copper, we found the electromotive force obtained with sea-water, or salted water is 0.45 of that of this pair; this latter was formed of a glass vessel containing a saturated solution of sulphate of copper, and a plate of copper, then of a diaphragm of unpolished porcelain plunged in, and containing acidulated water, containing $\frac{1}{10}$ of sulphuric acid, and a cylinder of amalgamated zinc. On using as electrodes retort charcoals covered with peroxide of manganese, this force becomes five times greater; the peroxide of manganese acts by removing the hydrogen, which is deposited on the negative electrode, whence results a current in the same direction as the terrestrial current. The electromotive force produced by the contact of water and vegetable earth, is one-tenth of that produced by a pair formed of amalgamated zinc plunged in soft water, and of charcoal in contact with vegetable earth.

The terrestrial compound pair formed of salt water at 3°, in which is placed a plate of amalgamated zinc, and of vegetable earth in

contact with a plate of manganese charcoal, has an electromotive force represented by 0.52, that of the pair of sulphate of copper being represented by 0.579, and that of the nitric acid pair by 1. This pair was a Grove's, formed of a diaphragm of 4 centimetres long and 2 diameter, and an electrode of platinum 4 centimetres wide, and the same in length. The diaphragm contained water acidulated to $\frac{1}{10}$, in which was a small cylinder of amalgamated zinc.

The constancy of the current of this compound terrestrial pair when the circuit is closed, is more remarkable. In the twenty-four hours the electromotive force only loses 0.02 of its initial intensity, then the pair becomes perceptibly constant for several days.

With iron and lead we likewise obtain terrestrial pairs with constant currents.

The compound terrestrial pair formed of a plate of amalgamated zinc of 1 square decimetre of surface placed in good vegetable earth at 1 decimetre in depth, and a plate of manganese retort charcoal, plunged in a cylinder open at both ends, buried in the earth at one end, and surrounded with argil to the height of 1 decimetre, then filled with salt water, marking 3 degrees. The zinc was 2 metres from the carbon.

The resistance of this terrestrial pair is 5.34 more considerable than that of the nitric acid pair; but this resistance varies, and depends on the nature of the earth, its hygroscopic state, and the distance between the elements.

Fourth Part.—Among the natural sources of electricity which are continually furnished to the air, we must mention the following:

1. The exhalation of oxygen and that of carbonic acid by the leaves of vegetables, which gives positive electricity.

2. The contact of earth with fresh or salt water; the first takes an excess of positive electricity,—the second, an excess of negative electricity.

Living vegetables behave in the same manner with respect to earth—that is to say, the parenchymic tissues are negative and the earth positive; both electricities are conveyed into the air by vapors.

3. The decomposition of animal and vegetable matters on the surface of the earth and in water, which causes complex effects.

4. The contact of the cold and hot waters circulating on the surface of seas; cold water is positive and hot water is negative. The tension of the electricity, in consequence of the extent of surfaces, should be very weak; for the disengagement of electricity only takes place on contact, when there is a mixing of the cold and hot waters.

It has been asked, whether the evaporation in these and similar cases—that is to say, in a voltaic pair whose circuit is not closed—it has been asked, I say, whether each element would furnish electricity to the vapor which escapes from it. We know well that when we put in communication with the earth one of the elements of a pair, we can give a charge to a condenser by putting one of the plates in

communication with the other element; would it be the same in this pair when vapor escapes from each element? Nothing is opposed to this, provided that the vapor removes continually the excess of electricity proper to each element, as would be done by a conducting body which would close the circuit. This is precisely what occurs when we put into communication the two plates of a condenser, with the two elements of a pair.

We see, then, that numerous facts tend to prove that there is disengaged in the air, from various natural sources, sometimes positive electricity, sometimes negative electricity; there must consequently be continual recompositions of the two electricities. The result is an excess of positive electricity, when the sky is serene; for this it is necessary that the sources which furnish this electricity should be the most numerous; these sources are probably vegetation and the contact of earth and water.

After having recalled all that we know as to the formation of storm clouds, I have remarked that the storm seldom bursts, unless when there is a presence of two clouds charged with the different electricities, or when a cloud highly charged with electricity is within the reach of the attraction of the earth. In these two cases, there is an action of influence, and the re-establishment of equilibrium. Thus, in places where there exist at certain distances natural sources of contrary electricity, this locality has one of the principal conditions for the formation of storms.

In the polar regions the rarity of storms is probably due as much to the absence of evaporation as to the small number of natural sources of electricity; in the same manner that, in the tropics, the frequency of storms is connected with the abundance of evaporation, and sources of electricity.

The diminution of storms as we advance into land on continents, on the one hand, and as we go from the coast out to the open sea, where it seldom thunders, appears to indicate that it is near the sea coasts that there are the most sources of contrary electricity; that is, consequently, where we must seek them.

Fifth Part.—The examination of the natural sources of electricity which intervene in the phenomena of the decomposition and recomposition of mineral substances have led me anew to the study of the slow actions which take place with or without the apparent aid of electricity—a question of the greatest interest to geology.

I show, in the first place, the improvements which I have introduced into the processes which I have hitherto employed to obtain crystallisations of many metallic oxides, and double or simple basic salts; then I have shown how we may, in the course of some years, obtain crystallisations on some mineral substances placed in suitable conditions, the following products:—

1. The double carbonate of lead and soda on galena, in small acicular crystals found sometimes in lead ores into which have infiltrated water charged with carbonate of soda.

2. Carbonate and sulphate of lead on galena, in crystals similar to those of nature.

3. Arseniate of lime on sulphate of the same base, in every respect similar to that formed in some veins.

4. Bibasic phosphate of lime, crystallised with phosphate of copper on calcareous stone.

5. Some double compounds described in the work.

6. Pyritic acid which has undergone such a decomposition that its surface is covered with the beautiful colors presented by the iridescent pyritic copper.

When I commenced, thirty years ago, to investigate the reproduction of mineral substances by causing the action of electric forces to concur with that of affinities, the objection was made to me that nature did not employ voltaic pairs; this objection did not appear to me a serious one: I shewed that I sometimes attained the same end without the intervention of a pair, by putting insoluble mineral substances in contact with various solutions during several years. The result has been this principle, new to chemistry, that the insolubility of bodies is not absolute, and that with time, double decompositions will take place, in consequence of their contact with certain solutions, causing crystallised compounds, similar to those found in metallic ores.

In this work we have mentioned all the sources of natural electricity hitherto known, and their influence on the formation of storms, as well as the chemical effects produced by virtue of slow actions, with or without the aid of electric forces.

M. ELIE DE BEAUMONT mentioned with relation to this communication that geological observations enable us to conceive the existence in nature, of weak electric currents, analogous to those by means of which M. Becquerel has been able to produce some mineral substances. These species and their analogues are found particularly in veins: now the fissures in which the veins commenced passed successively through layers of different natures which by the effect of contact only become in different electric states; this has been proved by direct experiment. Moreover, these veins pass through mineral masses whose temperatures differ in some extreme cases as much as 1000° and 1200° C. and even more, and these differences of temperature are also sources of electricity. It is therefore natural to suppose that these veins have in all times, and still do, serve as conductors of electric currents.

Comptes Rendus.

TOXICOLOGY.

SLOW POISONING by ANTIMONY. By Dr. NEVINS.

DESCRIPTION OF EXPERIMENTS.—The animals experimented upon were rabbits, ten of which were healthy and vigorous, whilst one was puny and feeble. Dry tartar emetic was administered to each four times a day, in doses of half a grain, a grain, and two grains respectively, in order to ascertain the effects of different quantities; and they were killed from day to day by a blow, until they began to die from the effects of the poison, in order to observe the morbid appearances produced day by day. They had food in abundance, and were kept in a large and airy hayloft.

Amount of Antimony given, and period of Administration.—The smallest amount which produced death was twelve grains, which were given in four days to the delicate rabbit above mentioned, and the next fatal case was caused by twenty-one grains in five days. After this, no rabbit died for a week, the next fatal case being after forty grains, given in twelve days. The largest amount given was seventy-three grains, and this caused death in seventeen days. The fatal quantity, therefore, ranged from twelve to seventy-three grains, and the period requisite for causing death, from four to seventeen days.

General outline of Symptoms during Life.—For the first few days no striking symptoms were evident, but after this the animals lost their spirit in a great degree, and gradually became emaciated. In some of the later cases the emaciation was extreme. They continued to take food until almost the hour of death, but the amount eaten in the day, fell at last to less than half the quantity consumed before the commencement of the poisoning. None of them vomited, for rabbits are incapable of this action; and in the earlier animals killed by the poison, there was an absence of diarrhoea, whilst this symptom was present in all those which lived beyond the tenth day. Cramps were not present in a single instance, but several of them died in violent convulsions, which lasted from a quarter to half an hour, before death put a period to them. The mouth was severely ulcerated in several of them, from the local action of the tartar emetic, whilst being introduced into it. One of the rabbits proved to be with young, and continued to increase in size and weight for about a fortnight, after which all motion ceased in the belly. She lost weight and flesh, and died whilst giving birth to the sixth of a litter of seven dead immature fœtuses.

General Pathological Appearances.—In many of the rabbits killed, the morbid appearances were insufficient to account for death; and although several organs were evidently affected by the poison, the amount of diseased action was trifling. In the latter cases, however, the pathological appearances were generally strongly marked. Their general outline was as follows:—*Emaciation* sometimes extreme, so

that not a trace of fat remained in the body. *Mouth* ulcerated. *Stomach* frequently inflamed in patches, but not throughout; sometimes, but rarely, ulcerated; always more than half full of food. *Pylorus* frequently so thickened and indurated, as to resemble cartilage under the knife. *Small intestines* frequently inflamed in patches, but very rarely ulcerated. Intestinal glands sometimes excessively enlarged, and deeply colored by sulphuret of antimony. *Large intestines* frequently empty, but seldom presenting any diseased appearance. *Liver* generally congested in parts; occasionally inflamed; hard, or brittle. *Kidneys* generally more or less congested, but never alike in this respect; sometimes one only; and sometimes the upper portion of one, and the under portion of the other. *Bladder* generally distended with urine, and more vascular than usual. *Brain* seldom examined; healthy. *Lungs* and *Trachea* frequently congested; sometimes *highly* inflamed; the two lungs seldom alike. *Heart* healthy; generally full of black, coagulated blood. In several cases there was *extensive extravasation of blood* upon the surface of the lungs, and of the liver and stomach, and beneath the mucous coat of the cœcum.

Distribution of Antimony in the Tissues.—It was always present in the *liver* after five grains had been given, and it appeared in this organ before it was clearly proved elsewhere, except in the coats of the stomach, about which some doubt existed, as to whether its presence there might not be owing simply to adhering antimony. It appeared next in the *kidneys*, and after the fifteenth day it was present in the *bones*. Its presence was also easily proved in the *blood* and *lungs*, the *urine*, and the *fæces*, both hard and soft. In the *brain* it was never clearly present, and its evidence in the *muscles* was very slight.

Elimination from the System.—The antimony was constantly passing off by the urine and the fœces, and it was discovered in both these excretions abundantly in some rabbits which survived twenty-one days after the last dose of antimony had been given; and in the fœces it was slightly present thirty days after the last dose. It disappeared from the liver before the fifteenth day, and from the kidneys somewhat later than this; whilst it was found abundantly in the bones thirty-one days after the last dose had been given.

Effects upon the Fœtus in Utero.—The whole litter, consisting of seven young ones, was killed by the poison a day or two previous to the death of the mother, and antimony was found in abundance in the placentas and livers examined separately; in the remaining contents of the abdomen, examined in a mass; in the contents of the chest, also examined in a mass; and, lastly, in the remaining flesh and bones of several fetuses, which were well washed after all the viscera had been removed.

Chemical process adopted.—The solid organs were reduced to a semi-fluid state, by pounding them in a mortar when requisite, and then boiled for five or ten minutes in pure fuming muriatic acid, or until they were dissolved, along with bright slips of copper. The materials and copper slips, were then set aside for twenty-four hours, and the

comparative amount of antimony was estimated by the comparative effect produced in this period of time. The antimony is often very slowly deposited, and several hours are frequently necessary before any decided effect is produced when the metal is present in small proportion.

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MATERIA MEDICA, PHARMACY, AND THERAPEUTICS.

AMYLENE; a NEW ANÆSTHETIC AGENT.

By DR. SNOW.

DR. SNOW, in a paper read before the Medical Society of London, on the 10th of January, and since published in the *Medical Times and Gazette*, has directed attention to amylene as an anæsthetic agent, and numerous trials of this substance for producing insensibility are now being made. In the paper alluded to, Dr. Snow briefly traces the history of the introduction of anæsthetics. He gives the following quotation from Sir Humphrey Davy's *Researches concerning Nitrous Oxide*, published in the first year of this century: "As nitrous oxide in its extensive operation seems capable of destroying physical pain, it may probably be used with advantage during surgical operations in which no great effusion of blood takes place." About forty years after the publication of this sentence, nitrous oxide was used with doubtful success by Mr. Wells, a dentist, of Hartford, Connecticut, in some cases of tooth extraction; but its use was speedily relinquished, the effect being considered uncertain. In September, 1846, Dr. Morton, who had previously been in partnership with Mr. Wells, and had witnessed the trials with nitrous oxide, administered the vapor of sulphuric ether with success in a case of tooth drawing, and afterwards in surgical operations. This led to the general use of ether vapor. In 1847, Mr. Jacob Bell, substituted the substance known as chloric ether, for sulphuric ether, and this, although not sufficiently active, was found to produce insensibility. In the autumn of 1847, Mr. Waldie, of Liverpool, being in Edinburgh, explained to Dr. Simpson that the so-called chloric ether was a solution of chloroform in spirit, whereupon the latter obtained some pure chloroform and administered it with great success. Dr. Snow states, "Ever since the introduction of chloroform, I have been of opinion that other agents would be met with, more eligible for causing anæsthesia by inhalation. It seemed improbable that this one, which happened to be standing on the shelf of the Pharmaceutical Chemist for another purpose, should be better than all the very numerous volatile compounds which organic chemistry is daily bringing to light; and the continued use of chloroform is probably due to the circumstance, that hardly any one has made

anæsthesia by inhalation a subject of constant and protracted investigation. I have from time to time made experiments on animals with a variety of substances, and I find that the agents which might be inhaled for the prevention of pain, in the absence of others which are more eligible, are extremely numerous. They include, among other things, carbonic acid and carbonic oxide gases, olefiant gas, the vapor of hydrocyanic acid, and cyanogen gas; which last is contained, as I found, in the fumes of the puff-ball, which Dr. Richardson brought before the notice of this Society. The agents which I have exhibited as anæsthetics to the human subject, in addition to ether and chloroform, have as yet been but few. They are nitric ether, Dutch liquid, benzin or benzole, a bichloride of carbon, made by decomposing chloroform with chlorine gas, the monochlorinetted chloride of ethyle, and amylene, the subject of this paper. Nitric ether was exhibited also by Dr. Simpson, and Dutch liquid by him and Dr. Nunneley. These substances possess no advantage over chloroform, unless it be in their slower action, while in other respects they are scarcely so agreeable. With regard to benzin, I discontinued the use of it on account of convulsive tremors which it occasioned in a case of amputation in St. George's Hospital. I found that these tremors are a constant result when its effects reach a certain stage. I administered the chlorinetted muriatic ether, in twenty surgical operations in the summer of 1851, chiefly in King's College Hospital. Its sensible and physical properties and its effects, are nearly the same as those of chloroform, but I thought that it might possess some advantage in the circumstance that, being less volatile, while its other properties are the same, it would be less liable to cause accident, even if incautiously used. I was, however, prevented from using it further, owing to the great difficulty of procuring it in a state of purity." He further states: "I was not aware of the existence of amylene till a few months ago, or I should have tried it sooner; for I made inquiry in 1848, for a substance named eupion, by Reichenbach, its discoverer, but was unable to obtain it. Eupion is a carbo-hydrogen, described as having all the physical characters which belong to amylene, though obtained in a different way, and I believe it is the same substance. Reichenbach obtained it from coal-tar, but other chemists have not been able to make it."

Dr. Snow describes several cases in which he has administered amylene, and says:—

"The relative advantages and disadvantages of amylene may, as nearly as I can judge, be summed up as follows:—In regard to its odor, it is more objectionable than chloroform, but much less so than sulphuric ether. The odor of any volatile substance is, however, no longer perceived after a patient begins to inhale. In respect to its pungency, it has a great advantage over both ether and chloroform, being much less pungent than either of them. Thus, whilst the patient, especially if a female, often complains of a choking feeling and want of breath, in commencing to inhale chloroform, and two or

three minutes are lost before the vapor can be inhaled in any useful quantity, she can begin to inhale the amylene of full strength, within half a minute from commencing, and the operation may generally be begun within three minutes. In the amount which suffices to produce insensibility, it is intermediate between chloroform and ether, chloroform having the advantage. Amylene has an advantage, in preventing pain with a less profound stupor than that occasioned by the other agents, and in the ready waking and recovery of the patient it has an advantage over chloroform, and a still greater advantage over ether. Its probable safety I have spoken of;* and the greatest advantage of all, if it should continue to be met with in all cases, is the absence of sickness from its use. The almost entire absence of struggling and rigidity may also be mentioned as an advantage of amylene over ether and chloroform."

Preparation and Properties of Amylene.—Amylene was discovered and described in 1844 by M. Balard, of Paris. It is made by distilling fusel oil, or amylic alcohol (the oily liquid which is separated from spirit of wine in the process of rectification), with chloride of zinc. On adding the fusel oil to a concentrated solution of chloride of zinc while they are cold, solution or admixture does not take place, but on applying heat they mix and form a homogeneous liquid, which begins to distil at a temperature of about 266° F. On re-distilling the product thus obtained, the ebullition which commences at 140° F., rises during the process to about 570° F. The most volatile parts of this distillation are to be separated, and agitated with concentrated sulphuric acid, when the amylene in a pure state will rise to the surface. It is colorless, very mobile, and has a low specific gravity (stated by Dr. Snow to be 0.659 at 65° F.) Its boiling point is 102° F., and the density of its vapor is 2.45. It has a peculiar and disagreeable smell. Its composition is represented by the formula $C^{10}H^{10}$. It is soluble in alcohol and ether in all proportions, but is very sparingly soluble in water. Specimens prepared by different manufacturers have been found to present some difference in their sensible characters, which probably arises from the circumstances that the fusel oil from which it is made is not a definite body of uniform composition, and that the amylic alcohol, the principal constituent of the crude fusel oil, is not easily isolated in a state of purity. Should it be found that amylene cannot be always obtained in an uniform state, this will be a serious obstacle to its successful employment as an anæsthetic agent.

Pharmaceutical Journal, Feb., 1857

* The author states, "While I cannot venture to predict for it the absolute safety which seems to attend sulphuric ether under all circumstances, I confidently trust that it will be perfectly safe with careful management."

EXPERIMENTAL NOTES *on* GLYCERINE.

By Dr. W. LAUDER LINDSAY, Perth.

IN order to test the nutrient properties of glycerine, Dr. Lindsay tried its effects upon himself, by taking two or three spoonfuls daily for several weeks. He says:—

For the space of a month I took two teaspoonfuls every morning in coffee, which I found to be the most palatable mode of using it. My diet and daily occupations were the same as before I began to take it, my exercise, in consequence of protracted bad weather, rather less than usual. The result was a gain of weight to the extent of $1\frac{1}{4}$ lbs. at the end of the first fortnight, and of an additional $\frac{1}{4}$ lb. at the termination of the second—in all, an increase in weight of 2 lbs. On discontinuing the glycerine, my weight gradually fell: and after an interval of six weeks, during which I have not used it, I find myself 1 lb. lighter than before I began to take it daily. It produced no other appreciable effect. I have tried it as a dietetic remedy along with various articles of food, and in divers combinations. It is so readily miscible with fluids of all kinds, that there can be no difficulty in its administration. The pure concentrated glycerine of Price's Company is too pungently sweet to be used alone or undiluted; and I may here remark that, as a general rule, whether for internal administration, or for external application, it ought to be diluted, the degree of dilution being regulated by the practitioner, according to the purpose for which he prescribes it. I have sometimes sweetened coffee with it instead of sugar. Coffee, so made, however, has a somewhat peculiar taste, which might prove objectionable to some fastidious stomachs; but when it is sweetened only partially by glycerine, or when glycerine is superadded to ordinary sugar, the beverage is exceedingly pleasant, and quite free from any peculiar flavor or taste. It might be used daily to a considerable extent, when thus mixed with coffee, or chocolate; it is perfectly miscible, and does not betray its presence by floating oil globules or otherwise. In such circumstances, it behaves precisely like a syrup. Tea, to which glycerine has been added in a similar way, is much more apt to be flavored by it, but the taste is not likely to be regarded generally as unpleasant, unless it is added to the extent of two or three teaspoonfuls per cup. It also sweetens milk or cream very pleasantly. A mixture with water is very palatable, and is, undoubtedly, its most ready and cheap mode of administration.

I carefully observed its effects, as a nutrient and alterative, in eight patients—four males and four females—to whom it was given in doses of two or three tea or table-spoonfuls daily, for the space of a month.

They were weighed, at intervals, to ascertain their gain or loss of flesh; for I found that apparent physical improvement was not a reliable criterion of real physical growth—that a most marked ameliora-

tion in the general health did not always coincide with increased weight of body. All the patients, before taking it, were more or less anæmic, emaciated, and feeble: in all, the diet, exercise, and occupations, were otherwise the same. At the end of the month, all of them *appeared* greatly improved in their general condition; they seemed plumper, and stronger, and the countenance, in some, was even ruddy.

The great advantages of glycerine over cod-liver oil, consist in its pleasant sweetness and its freedom from all disagreeable odor; in its ready solubility in, or miscibility with, ordinary fluids; in the absence of the principles, which in animal and vegetable oils so frequently nauseate and purge; and in its solvent and other properties, which render it so useful as a vehicle or basis for pharmaceutical preparations. Its great disadvantage, on the other hand, as contrasted with cod-liver oil, we shall immediately see, is its present comparatively high price. Cod-liver oil frequently produces an acrid burning sensation in the throat; it is extremely difficult of digestion by many stomachs; by others, it cannot be borne at all, without generating disagreeable and even serious gastric symptoms; and, in general, nausea and purging are very frequent results of its use. These effects would appear more liable to be produced by the dark-brown, than by the pale or colorless cod-liver oils. Many of the vegetable oils have a similar tendency to nauseate or purge, such as linseed, olive, and almond oils. None of these disagreeable symptoms are likely to be produced by the use of glycerine, and have not been produced in any cases which have come under my own observation. Hence it is reasonable to anticipate, that glycerine ought to become a valuable substitute for the ordinary animal or vegetable oils used in medicine as nutrients and alteratives, in the numerous cases in which they are contra-indicated—always, of course, granting or assuming what has yet, however, to be fully proved, that it possesses true fattening properties—that it acts as a food. A substance, which may be considered intermediate in chemical composition and properties between ordinary fats and oils and glycerine, has recently been used to a considerable extent in phthisis as a nutrient and alterative,—as a substitute for cod-liver oil. I allude to the substance called “coco-oleïne,” or cocoa-nut oleïne, which is obtained by pressure from cocoa-nut fat or oil, and subsequently purified. It has been prepared by Price’s Patent Candle Company, for use chiefly at the Consumption Hospital at Brompton, where it was introduced by Dr. Thompson. It has likewise been tried throughout the country; but it has been found very liable to produce nausea and gastric derangements. For this reason, its use cannot be long persevered in; and, moreover, it is expensive compared with cod-liver oil. Thompson found, however, that cocoa-nut oil increased the solids of the blood in the same way as cod-liver oil. It has been objected that glycerine, from its composition, is not likely to be of the same service in medicine, or to have the same physiological effect, as cod-liver oil and other oils. This objection, however, pro-

bably arises from mistaken views regarding the active ingredients or principles of cod-liver oil. Dr. Garrod has recently suggested that the last-named oil acts simply in virtue of its oleine, he having found that the margarine, or solid portion of the oil, produced no marked result when administered internally, and that its use could not be persevered in. Chemically speaking, oleine, as we have already seen, is the oléate of glycerine; and, *à priori*, there are more numerous and stronger grounds for believing that the active principle is the glycerine rather than the oleic acid. Glycerine appears to have been tried somewhat extensively in phthisis as a substitute for cod-liver oil, but the results described are very contradictory. Dr. Crawcour, of New Orleans, used it in cases where the nauseous taste of cod-liver oil rendered it most obnoxious to the patient, and its administration therefore impossible—with such success, that in the course of a year it had entirely superseded cod-liver oil in his practice. (See CHEMIST.) On the other hand, in "Thomson's London Dispensatory" (1852), it is stated—"The editor some years since tried its powers as an internal agent, giving it chiefly to patients in whom the use of cod-liver oil was indicated, and in about the same doses, viz. from one to four drachms. The results of his trials were such as to induce him to put but little reliance upon its efficacy. In some cases it appeared to allay the cough of phthisical patients, but no very marked improvement could be found to follow its use." "All reason and analogy," says Dr. Garrod in the review before quoted, "and the little clinical observation yet obtained, are against the idea of its acting similarly to cod-liver oil." I have had no opportunity of trying its effects in phthisis, either when administered internally, or when applied by the endermic method. But experiments on its use in this protean disease, would undoubtedly serve to dispel the contrariety of opinion which at present exists. It has been remarked to me by those who have tried it, that it does not allay the phthisical cough; nor do we expect that it should, at least immediately or directly. But it appears to have proved serviceable in other strumous cases. Dr. Stirling gave it in doses of three teaspoonfuls daily, along with one half-grain of quinine, for a period of three weeks, to a delicate, emaciated, cachetic, strumous boy of eight years; the improvement in general health was most marked and rapid, Dr. Deighton of Clapham [Lancaster] reports marked good effects from its use in chronic gastritis; and Mr. Wilson mentions a case of affection of the mucous membrane of the stomach occurring in the practice of an Edinburgh physician, in which it "appears to have worked wonders," in the dose of three tablespoonfuls per day. Dr. Browne of the Crichton Royal Institution for the Insane, Dumfries, has kindly favoured me with the particulars of several cases in which he has used it internally—including phthisis, strumous abscess, pulmonary abscess, chronic bronchitis, and general cachexia. In one case a strumous abscess cicatrised rapidly and satisfactorily under its use. In a case of phthisis, where ordinary aliments and remedies were refused or swallowed in too small quantity

to support life or health, it appeared for a time to take their place. In other cases, in which it was used internally, the results were inappreciable or nugatory.

Edinburgh Medical Journal, September 1856, p. 207.

On the Use of IODINE in FARCY.

By J. B. HALL, M.R.C.V.S., V.S. Royal Artillery.

ON the 19th of August last, a black troop-horse, stationed at Northampton, was reported to me by the farrier as being lame and having a swelled leg. I examined it, and more than suspected it to be farcy. The animal was therefore placed in an infirmary box, away from the other horses, and allowed to remain there until some more decided symptoms presented themselves. I watched him carefully every day, and about the fifth day from the above date I perceived the lymphatic vessels on the inner side of the near hind leg to become swollen, after which the glands in their course rapidly enlarged, and pustules made their appearance about the eighth day from the date of admission. Many of these very soon burst, discharging a dirty yellow matter; those which did not were opened, their capsules being thin and almost ready to burst. Nitrate of silver was freely applied to all. Two days after its use, they assumed a healthier aspect, and presented the appearance of small ulcers, varying in size from that of a sixpence to a shilling. They were washed twice or thrice during the day with a solution of sulphate of copper, and the same agent, in doses of a drachm, was given twice a day with the corn, of which the horse had from the first his full allowance. The appetite was good, and the animal in fair condition, considering he had lately returned from the Crimea. About the 5th of September, the inguinal glands began to enlarge to about the size of a hen's egg, resembling buboes. In a short time they burst, and discharged healthy-looking matter, it being white in color, and thick in consistence. Iodine was now given in conjunction with the sulphate of copper, in the form of the iodide of potassium, in drachm doses, twice a day. After this the buboes very soon healed up, and the leg rapidly decreased in size; but there still remained something like twenty ulcers all the way up the leg, both inside and out. These were sprinkled with the sulphate of copper in powder daily, and they very soon began to heal, and in one week from the administration of the combined agents, I had not more than half the number to deal with; and these also showed a disposition to granulate and to heal; which they all did about the middle of the month of September, excepting one just above the coronet, which remained for a week or ten days afterwards, and proved rather troublesome.

I find, generally speaking, that the inguinal glands suppurate, discharge their contents, and heal much quicker than the rest of the

ulcers; and the lower down the ulcers are situated, the more difficult they are to deal with. This undoubtedly is accounted for by their being further from the centre of circulation.

The cure of farcy is common enough undoubtedly, and also easy, provided the animal's constitution be good, and proper attention paid. I send this case merely from recollecting that, during the time I was a student, you advocated the use of the bin-iodide of copper in these cases; and from the above case improving so rapidly after the exhibition of the two agents in conjunction, I am compelled to consider iodine a valuable therapeutic in the treatment of this disease. The horse is doing duty with the battery at the present time, November 13th, having been discharged from the sick-stables about a month. I have examined him carefully since, but find not the slightest disposition in the leg to swell, or any other symptoms of the original disease present. During his probationary or convalescent stay in the infirmary, he was regularly exercised, and well groomed; both of which I consider essential in this affection, if the horse is not lame.

The deduction I draw from the cases of farcy that have come under my notice, and the success attending their treatment is, that every case should be treated with a prospect of cure, provided the lungs are healthy and the horse will feed. I have a horse under treatment at the present time, but he is a foreigner, which very much diminishes my prospect of success; and his constitution is also much shaken by a campaign in the Crimea; but as long as his appetite remains good, and there is no indication of internal organic disease, I entertain a hope that eventually the case will turn out favorable. At any rate, I will send to you the particulars of it whichever way it may terminate. I may here be permitted to remark, that the accommodation in the out-stations of the army, as far as I can judge, is very good for the treatment of infectious diseases. The sick animal is immediately removed from the rest to a box at some distance, and which is white-washed, repainted, and repaired (if necessary) before it is occupied by another, so that the next occupant finds it as clean and sweet as if newly built.

Veterinarian.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

CHEMISTRY *as a Science*. By H. C. JENNINGS, ESQ., M. D.

(Continued from page 278.)

CHEMISTRY has more than realised the wildest dreams and the most extravagant promises of the Alchemist; although it may not have formed the elixir which should prolong life to a fabulous extent, it has extorted some of the most mysterious secrets of nature, so as to mitigate the fatality of disease and the severity of pain, and has thus contributed to prolong the life of man beyond its former average, and aided in increasing the population by giving fresh means of preserving health and of augmenting comfort and innocent pleasure; although it may not have effected the transmutation of the baser into the nobler metals, it is every year developing new mines of riches, and expediting the operations of the manufacturer, which, as money is nothing but the representative of labor, is the real philosopher's stone.

We are progressing towards the period when the laboratory will succeed in producing antidotes for some of the most severe mental and physical perturbations that assail humanity, will enable the physiologist to correct the deformities and irregularities which degrade the form and rob the human face divine of its regularity, expression and beauty, control the influences which degenerate the osseous structure, which deprive the muscular development of its power and flexibility, and which render the nervous tissue and brain less acute, energetic and stable; when it will enable him to deprive the circulating fluid of those deleterious particles which render it the source and vehicle of the most loathsome disease. These happy results may be slow in arriving, but they are certain. Let us pass in review a few of these benefits.

Perhaps no discovery of chemistry stands out more prominently as worthy of our admiration than that of Chloroform: this wonderful fluid, inhaled in moderate doses (except where certain diseased states of the brain, lungs or heart render its use impossible), safely relieves mankind from the excruciating agony of the surgical operation, the extraction of polypus, the application of caustic to the living flesh or carious bone, and, by leaving the mind of the operator free from the perturbation which the consciousness of producing terrible suffering must occasion, renders the success of the operation far more certain; but, above all, it mitigates the terrific throes of nature, when the delicate and suffering mother would almost expire in giving life to her offspring.

Then let us reflect on the effect of the insertion of a minute particle of virus, which has already passed through the organisation of another

animal. Vaccination, for the prevention of a most formidable and hideous contagious disease, has, after many years of pertinacious opposition, gained great triumphs, and is guaranteed and sanctioned by the Legislature of the two most powerful Empires on the earth. It is likewise reported, upon what appears tolerably sound authority, that the venom of a reptile found in Southern Africa has been applied successfully in 250 cases, by inoculation, as a specific against yellow fever. We may safely infer that these discoveries are but an instalment of what is yet to be realised in chemical physiology.

Photography is based on the combination of some of the most delicate and complicated chemical agents; by its means alone can that rapidity be obtained necessary to give results, which are frequently scarcely less perfect and beautiful than the original from which they are taken.

The action which the gastric juice of graminivorous animals possesses of converting grass and other vegetable substances into albumen, stearine, and elaine is most wonderful, and is only equalled by the processes which are constantly and regularly being performed throughout the vegetable kingdom.

It is impossible to assign any limit to chemical progress. Every year accumulates new facts and augments our store of phenomena; chemistry is pregnant with immense discoveries, which are destined to decorate the world with beauty, power, and perfection. Chemistry professes nothing occult or mysterious; she presents only simple truths to the eye of reason, and challenges the severest scrutiny for her discoveries in the mighty laws of nature; she knows that they may be received with confidence, for they are merely the interpretation of the handwriting of the Deity.

We as yet know absolutely nothing of the ultimate constitution or composition of the metals: we recognise them as belonging to one great family: we are acquainted with their relative specific gravities, many of the alloys which they form, their affinities for acids and alkalies, their salts and oxides, and the colors these produce, which probably are nothing more than the visual effect arising from the different velocity with which they reflect the solar rays. Many of them are very combustible, and have a great affinity for oxygen and chlorine. If we may be permitted to take an excursion beyond the domain of fact, and soar into the region of theory, we might say that there appears some reason for supposing the metals to be some form of, or combination with, hydrogen. It appears to me reasonable to infer that, as this is the lightest body we are acquainted with, if its molecules could be made to approach nearer to each other, and if it could be liberated from its equivalent of caloric and electricity, it would then be one of the heaviest substances; it has a great affinity for oxygen and chlorine, &c.; so that all the variations of specific gravity, elasticity, hardness, and brilliancy, might result from atomic arrangement and proximity.

It has been proved that mercury will combine with ammonia, and produce an amalgam which has all the splendor of quicksilver, but has much less specific gravity. Ammonia is a compound of nitrogen

and hydrogen. There are many other chemical phenomena of a similar character. But we must quit chemical metaphysics: the idea is only started to give an impulse to research and inquiry in this direction, which cannot fail to elicit new facts and phenomena.

We must not omit pure carbon, or the diamond, (out of respect to its useful relative, coal) this substance exceeds every other in hardness, and in its property of reflecting the rays of light; but coal far surpasses it in utility; about 150,000 tons are annually converted into carbonic acid gas, thus surrendering its store of latent heat, impelling the locomotive over the surface of the globe, &c. &c.; if we calculate the value of the fuel consumed for all purposes during ten years, the amount will be found to exceed the value of all the diamonds that have ever fed the vanity of man, or decorated the form of woman.

As human beings multiplied on the earth coals were found; as these will, however, inevitably diminish in time, the Almighty has stored up for future necessity thousands of square miles of peat-bog, having a depth of from 30 to 60 feet, which will supply fuel for many ages; should these be exhausted, the ocean will furnish an inexhaustible source of caloric, for the time must and will come, when the genius of man shall discover some cheap and rapid means of decomposing water; its hydrogen will replace wood, coals and peat, and as soon as the liberated combustible gas has performed its destined service, it will be again condensed, by combination with oxygen, into water, and then used again in endless perpetuity.

Let us now glance at chlorine, which forms in combination with sodium above one-fortieth part of the ocean, and which preserves this vast receptacle of the earth's ordure from putrefaction and change. Cast your eyes over that waggon laden with enormous sacks; what do the sacks contain? Rags, pieces of textile fabric, which after performing some useful service to our race, are now saturated with every description of filth that can disgust at least two of our delicate and acute senses. They are placed in contact with chlorine gas; a short time suffices to remove every foul matter they contain, and to fit them for conversion into pure, white paper.

There are three substances which possess the property of deodorising and decoloring; animal charcoal, composed of carbon and phosphate of lime; chlorine, which appears to act through or from its affinity for water; and oxygenated water; this last substance is not a mere saturation of water with oxygen, but is a true chemical combination and is the substance which contains the greatest proportion of oxygen hitherto discovered; for its centesimal proportions are, according to Thénard, 93.98 oxygen, and 6.02 hydrogen in 100 parts; it is the most important product chemistry has produced, it has opened a new field of research and of phenomena, which the science cannot probably ever hope to exhaust, as it demonstrates the existence of a body which can produce chemical reactions by its presence only, *without giving to or taking anything from, the substance which it decomposes*; this fact gives birth to numerous difficult questions of exceeding interest and importance.

Water is, perhaps, the most extraordinary and important substance with which the chemist is acquainted; its eminent necessity and constant action in the fluids and solids of all animal and vegetable organisations, its properties or powers of modifying the various forms of animal and vegetable matter, are almost infinite; the circulation of the blood in animals and of the sap in trees, is wholly dependent on this fluid. The vapor of water, at 212° F., has an increase of volume amounting to 1700 times that which it has in its liquid form; but *in vacuo*, at a temperature of 60° F. it expands to 7500 volumes; and at a lower temperature it is much greater still. It is probable, that the expansion and elasticity it possesses when circulating through the veins and arteries of animals, at a temperature of 92° F., at which degree it has an expansive force equal to counterpoise atmospheric pressure, enables men and animals to resist the pressure of the atmosphere, or 15 pounds upon every square inch of surface of their bodies. This is supposed to be effected by certain gases within the bodies of animals; but we have no proof of the existence of such gases; water is quite adequate to perform this office when mixed with atmospheric air; it is, therefore, unphilosophical to attribute it to any cause, of the existence of which no proof can be given. Water, also plays an extraordinary part in most of the geometrical or crystalline forms of vegetable and mineral matter. The elastic power of steam is not strictly a chemical effect or action, and its almost unlimited expansive power, far exceeding that of gunpowder, is too familiar to require any comment, nor is its electrical development unknown.

Animal charcoal has the peculiar property of decoloring and odorising certain substances, particularly such as have affinity for water; the cause of these effects is (in the present state of chemistry) unknown; but it is probably connected with some property of electricity, and also with the infinite division of the carbon, by the admixture of phosphate of lime. Chlorine possesses similar properties; but has a very different mode of action, and is attended with destructive effects and an odor, which the animal charcoal is free from.

It may be interesting to many readers to learn, that some experiments are upon the tapis which give considerable warrant for believing, that we are upon the eve of an important discovery, no less than the artificial production of floss silk; the specimens already produced are so nearly perfect, that little doubt exists that ultimate success will crown the labors of these chemists; the article will probably cost about the price of raw cotton; this invention will considerably contribute to the wealth and comfort of the people, and will render us wholly independent of the United States and India for the supply of cotton, and of China and Italy for that of silk. The secretion of the spider and the silkworm are nearly identical in their composition; this fluid after a few minutes contact with the atmosphere, becomes an insoluble elastic fibre, exceeding in strength anything of animal or vegetable production, and almost equal to the metallic wires, and is one of the best non-conductors of electricity.

Pyroxyline or gun-cotton is one of the most interesting substances; it

is insoluble in water, and its detonation (when pure) yields neither residue nor smoke; it has considerably more projectile force than gunpowder, weight for weight; and is, perhaps, the substance that has the highest degree of facility to charge with electricity, and is certainly beyond every other substance that gives off electric matter, in the quantity it throws off when excited. It is perhaps to this latter effect that may be attributed its remarkable effects, in receiving the rays of light when used for photographic purposes, under the form of collodion. The commercial application of pyroxyline for blasting slates, notwithstanding its high price, is a proof that it offers superior advantages over gunpowder; and we do not despair of seeing it applied with safety and advantage for projectile purposes, and even used as a power.

The boy, throwing his globules charged with fulminating powder may, when he reaches manhood, employ the same substance to bring down his game, or for the more hateful purpose of war; for it is beyond all question the substance the most rapid for igniting gunpowder, whilst its application is far more safe and facile than the old practice afforded.

Physiological chemistry will enlarge the horizon of the physicians' view; it cannot be doubted that chemistry should be the basis of the medical or hygienic practice, as surely as that of anatomy is the fulcrum of the surgeon's ability and power.

The action of ozone upon albumen, converting it into urea, is a most important feature in animal chemistry.

Chemistry having the property of separating secondary principles or matters from vegetable substances, has added an enormous amount of energy to the simple bases, viz. strychnine, quinine, morphine, &c.

It may perhaps interest some readers, if we endeavor to illustrate some of the moral effects resulting from chemistry; gas illumination figures very prominently, as it dispels the gloom of night, and so checks burglary and midnight robbery in cities and towns. It also renders fires less frequent, and is less filthy than oil and tallow lights. The power possessed by the analytical chemist, of detecting the most minute portions of mineral or vegetable poisons, renders the perpetration of these deadly and secret crimes more dangerous to the desperate and guilty. Regard that man with stern features and restless eye, in that railway carriage; he contemplates escape from the consequence of a foul crime; he assures himself that a velocity of 60 or 70 miles per hour will convey him to a refuge where he will be safe. But ere he had left London a few miles behind, his description and an order for his apprehension had already reached the port from which he hoped to embark. He is there arrested, is soon after tried, and finally executed!! A small portion of sulphuric acid, some discs of zinc and copper, and a few small wires, prevent crime from enjoying impunity, and are the means of satisfying Divine and human justice! Chemistry again affords a formidable impediment to that insidious and most dangerous invasion of health and life, arising from the infa-

mous artifices resorted to by those who adulterate human food, &c., the laboratory offers the only protection possible in these cases. The diminution of crime, by the chemical colors applied to bank notes and postage stamps, and the easy means of testing the precious metals, is but the lighter phase of the moral attributes of this science.

Now, just see that cold, shivering wife; it is a cold, damp, winter morning; see how she strikes that morsel of flint stone against the bit of steel in her other hand,—a spark falls into the dish beneath—she blows, but in vain; the tinder is bewitched or damp; again and again she renews her efforts in vain—she runs to a distant neighbor with a bit of candle—this she covers with a paper funnel, and then shields that from the keen blast with her apron—to the imminent danger of setting herself on fire. She is anxious to get something warm and nourishing for her husband, to enable him to face the hard weather and harder day's work before him. Turn to another *Tableau*. Look at that little maid; she takes a neat paper box from the chimney-piece; from it she draws a lucifer match, with a touch that would hardly excoriate the wing of a butterfly, she, in less than a second, obtains *light and fire*. Ah! what a mine of chemistry lurks in these two words! Will you return to the tinder-box? No. I thought not. Just reflect a moment. This humble effort of chemistry employs thousands of our men, women, and children, profitably and honorably; it saves time and temper—is ready in cases of danger, alarm, or sudden sickness—and how cheap!—yet what time, expense, and talent, have been prodigalised, to bring the lucifer match to its present state of perfection!

I will conclude this article, apologising, if I have trespassed too long upon the patience and time of the reader, by saying that astronomy is more calculated to excite our admiration and wonder than it is to stimulate our gratitude or devotion, the absolute infinity of its scope is so overwhelming, and so absolutely interminable in every possible direction, that the soul is plunged into an ocean of mystery and doubt. Finite creatures cannot grasp more than a mere parenthesis of infinite space or duration. Astronomy is the luxury of the privileged few. Chemistry is the daily mental food of the many, and can be, and is participated in by all mankind; and as chemistry is a science of facts, it can be attained and exercised by all who possess a common portion of mind; though it must be confessed that to apply it to new purposes, or elicit brilliant discoveries in chemistry—genius of a very high order is required, and can alone command success.

8, Gt. Tower Street,
12th Feb. 1857.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On SQUILLS. By M. MARAIS.

This work is divided into six parts :—

The first is devoted to historical researches on the use of squills in medicine.

The second is the discussion of the analyses of this substance by MM. Vogel and Tilloy.

The third contains the monograph of the squill, and the microscopical examination of the bulbs.

In the fourth and fifth the author shows the plan which he pursued for the proximate analysis of this substance, and the processes which enabled him to isolate its active principle, scillitine, of which he then describes the characters and properties.

In the sixth chapter, he describes the toxicological experiments which he undertook on scillitine, with the assistance of Dr. Gosselin and M. Adrien Bussy.

The historical researches prove, that the priests of ancient Egypt knew of the diuretic properties of the squill and those preparations now most used, such as the wine, the vinegar, hydromel and oxymel of squills; that Pythagoras obtained this knowledge from them, and that Hippocrates recommended the squill as the best diuretic of his time.

We shall not here reproduce the various opinions held from these early times to our own on the nature, properties, modes of preparation and use of squills, which are of no real use to the advance of science. We shall merely mention that it is to Boerhaave and Athanasius, that we must attribute the first idea that the squill contained a volatile principle on which depended the piquant action of the bulb, and that this error which was partaken by Cartheuser, and still later by M. Vogel, continued until M. Tilloy opposed it by accurate experiments, in a memoir which he presented to the Academy at Dijon, in 1820.

The first chemical observations of any value, of which squills were the subject are those of Cartheuser, a doctor in Paris, who found three peculiar principles.

The first, volatile and sharp.

The second, fixed, acrid, bitter, and gummy.

The third, fixed, acrid, bitter, and resinous.

In 1809, Planche found in squills, salts of lime and tannin. In 1812, Vogel made a careful analysis, and found a saccharine matter, a vegetable mucilage, which he improperly called a gum, and an acid which communicated to its juice the property of reddening litmus paper; but the viscid bitter principle which he isolated was a very complex product, and by no means deserves the name of scillitine, since bestowed upon it by some chemists.

The memoir, published in 1820, by M. Tilloy, made a fresh step in science; this chemist discovered in the squill a fatty matter soluble in ether and insoluble in alcohol, and proved that there was no volatile principle, but that the very bitter resinoid substance which he found was a mixture of a small quantity of scillitine with the fatty coloring and tannin matters which accompany it in squills, and if he was right in attributing the sharp action of the squill to the crystals diffused through its tissues, he was wrong as to the nature of these crystals, which he supposed to be formed of citrate of lime, whereas they are composed almost exclusively of carbonate.

After these observations as to the composition hitherto attributed to the squill, M. Marais enters on his monography; but judiciously thinking that it would be quite incomplete, if he did not add the microscopical examination of the squill, he, with the valuable assistance of M. Payen, undertook this study.

His researches led him to the following results.

The epidermis of the bulbs of the squill is formed of a single layer of colorless cellules. This epidermis is pierced with numerous stomata, having the appearance of small, elongated, oval openings, bordered with two crescents, whose two touching extremities are obtuse, and having the form of mouths bordered by two thick lips. These stomata are found only on the posterior side of the bulbs, and are more numerous on the upper than on the inferior portion.

The tissue of the squill is exclusively formed of cellules joined to each other, and presenting indifferently the pentagonal and hexagonal form.

The air vessels are very numerous, especially towards that part of the parenchyma which touches the stomata. The greater part of the utricles contain a colorless, limpid liquid; about one-third are filled with a coloring matter, sometimes rose colored and sometimes grey. The coloring matter of the squill become a bright rose under the influence of acids, and an apple green in the presence of alkalies. From this observation joined to the data furnished by the microscopical examination, M. Marais considers that he ascertained, that the coloring principle of the squill is developed under the influence of the oxygen of the air and of light.

The brilliant crystalline needles perceived in the parenchyma of the squill particularly fixed the attention of the author, and in studying them he observed very numerous raphides in the layers of the bulbs.

According to Decandolle and Keiser, the raphides are crystals elongated into the form of needles, or excessively thin, long prisma, terminated at both ends by very sharp pyramidal summits. These crystals are united in considerable numbers in one utricle, pressed one against another and parallel with one another; the raphides are enveloped with a common pellicle, which covers the whole mass of those in the same cellule, and moreover each raphide is covered with a separate envelope of extreme tensity. These crystals are formed in most cases of carbonate and oxalate, or of sulphate of lime, sometimes of carbonate only in squills.

Besides the raphides of the utricle we likewise observe in the squill, bundles composed of more voluminous crystals, which, in growing, appear to have broken the cellule in which they were originally inclosed, and which however cannot be perceived without the aid of a microscope. But there are others which are still further from the primitive state, these are those brilliant white needles, observed with the naked eye in the layers of the squill, and which have always been considered as simple crystals. Now each of these needles is a group of raphides, which has reached much more considerable dimensions than those found at first in the utricle. These bundles of crystals are covered with a nitrogenous matter which enveloped them in the inside of the cellule, a substance which, by its transparency and extreme tensity, give them the homogenous aspect of one single crystal. This membrane exactly applied on the group of the raphides presses them one against the other, and thus gives them sufficient cohesion to pierce the skin.

- One of these needles, placed in the field of the microscope, presents from 18 to 25 very elongated prismatic crystals, exactly applied one to another, and terminated at each end by an acute cone. Each little needle therefore presents from 40 to 50 acute points, which are able to pierce through the skin notwithstanding the extreme thinness of the membrane which covers them.

These crystals, joined to other smaller ones, being diffused by millions in the parenchyma, produce numerous pricks when we rub the fresh squill on some portions of the bodies. Their action is confined to piercing the skin, the acrid principle of the plant then acts on the prick and produces the redness, the little pimples and itching to which we are exposed when we put fresh squills in contact with the skin.

The membrane which envelopes the raphides, gives with ioduretted water and sulphuric acid, a reddish yellow; the author hence concludes that this is not cellulose, but the nitrogenous matter of the interior of the true cellule.

After these curious observations, M. Marais gives the processes which he has used to execute the proximate analysis of the squill, and the results which he obtained.

After having ascertained that boiling water dissolves all the principles of the squill, and leaves only 28 to 30 per cent. of a residue formed of cellular tissue and raphides, he chose the aqueous extract of this substance as the basis of his analytical investigations, and he successively isolated the following principles:—

Vegetable mucilage	80
Sugar	15
Tannin	8
Acid red coloring matter	10
Yellow odorous acid coloring matter	2
Fatty matter	1
Scillitine	1
Iodine	traces.
Salts	5

The vegetable mucilage was isolated by treating 100 grs. of aqueous extract of squills with alcohol at 0-90, which eliminated this substance, under the form of a glutinous precipitate analogous to gum and of a chocolate brown color, communicated to it by a coloring matter, insoluble, like itself, in alcohol. This mixture of mucilage and coloring matter weighed 40 grammes. This coloring matter is found in the cellules of the parenchyma of the squill; as it exists in the white parts of the layers it passes to a deep rose color, in contact with acids; to apple green, in contact with weak alkalies; to yellow, and sometimes to orange yellow, with concentrated alkalies. It is this substance which, under the influence of oxygen and light, gives the rose color which characterises the layers of the bulb of the squill. It reddens litmus, and behaves like the tannins with gelatine and the salts of iron, which it usually precipitates green, but it is distinguished from them by its insolubility in alcohol. This matter being, like the mucilage of the squill, insoluble by alcohol and precipitable with acetate of lead, the estimation of these two matters offered great difficulties. To surmount them, M. Marais had recourse to an indirect process.

He put one drop of perchloride of iron into 100 grammes of distilled water, holding in solution five centigrammes of pure tannin. On the other hand, he prepared an estimated solution of the mixture of mucilage and the coloring matter which he proposed to test, and poured it gradually, by means of a graduated burette, into 100 grammes of water, to which had been added one drop of perchloride of iron; by bringing the mixture to the green tinge of the typical liquor prepared with the tannin, and observing the indications of the burette, we are able to appreciate approximatively, the proportion of coloring matter associated with the mucilage, and consequently that of the mucilage itself. He thus found that there were 30 parts of the latter and 10 of the coloring matter in the mixture of these two substances, eliminated by alcohol from the aqueous extract of the squill.

The yellow acid coloring matter always accompanies scillitine, from which it can only be separated by a base, it appears to fill the place of a vegetable acid in the squill.

To estimate the sugar and tannin, M. Marais exhausted the aqueous extract of squill with alcohol, evaporated the alcoholic tincture, and took the residue up again with ether, to which it yielded the fatty matter, and the combination of yellow coloring matter and scillitine, which we have just described. The alcoholic extract thus exhausted by ether was reduced to a mixture of sugar and tannin. This mixture, dissolved in weak alcohol, was treated by acetate of lead, which precipitated the tannin and left the sugar in solution. The liquor treated by sulphuretted hydrogen, filtered and evaporated, left as residue the sugar, the weight of which compared with that of its mixture with tannin, made known the proportion of this last substance.

M. Marais attributes to the difference of oxygenation, the characters which distinguish the coloring matter of the tannin, properly so called, and considers that they proceed from one common origin.

The sugar which the squill contains, is of the same nature as the

inverted sugar found in vegetables with acid juices, it deviates the plane of polarisation strongly to the left. The saccharimetre and the use of the cupreopotassic liquor of Fehling, rendered it easy to estimate it very precisely.

The salts existing in the bulb of the squill, are almost entirely represented by the raphides. They are composed of carbonate of lime, and a small proportion of chloride of sodium.

The proportion found by M. Chatin amounted to $\frac{1}{100,000}$ of a milligramme per kilogramme of dried squill.

M. Marais has devoted the fifth chapter of his work to the recapitulation of the numerous attempts which he made, before succeeding in isolating the active principle of the squill, to showing the best method of obtaining it, and of defining its characteristics.

Struck with the persistence of the acid reactions of the active principle of the squill, which he found in all the products of the analysis, by means of the bitter taste which characterises it, he has supposed that it exists in the plant in a state analogous to a salt, and that the intervention of an alkaline base would be necessary to its elimination. These various considerations have led him to the following process:—

Prepare a concentrated tincture of squill with alcohol, at 0.90 or at 0.56, according to whether the squill is fresh or dry. This tincture is precipitated with clear milk of lime, and on to the whole is poured a sufficient quantity of ether to form, when the mixture is agitated and suffered to stand, a magma covered with a certain quantity of alcoholic and ethereal liquor, which is immediately decanted. The magma is washed with fresh ether until it has lost all its bitterness. The united liquors are distilled, until there only remains in the retort the alcohol, with the scillitine and a little fatty matter. This solution evaporated as rapidly as possible at a gentle heat, leaves a residue, which is taken up again with alcohol at 90° to dissolve the scillitine, and eliminate the fatty matter; the bitter liquor, evaporated in its turn to complete dessication, leaves the scillitine, which is removed in little lamellæ, and immediately put into a well closed phial.

There only appears to be about one per cent. of scillitine in the squill. It is uncrystallisable, hygrometric without being deliquescent, because insoluble in water; it is very soluble in alcohol and ether without heat. It is in pale yellow semi-transparent lamellæ; if, after being dissolved in a little alcohol it is precipitated by water, it is deposited very white, but it retakes its color and semi-transparency by dessication. It has a penetrating intensely bitter taste, which is increased by the presence of water.

Concentrated sulphuric acid dissolves scillitine, causing a beautiful violet color, exactly similar to that which the same acid produces with cod-liver oil. Water instantly causes the disappearance of this color, and the deposition of a green precipitate. If, after adding a very little water more sulphuric acid is added, the original color reappears.

Concentrated nitric acid dissolves scillitine, causing a bright red color, which disappears quickly. The addition of water forms no precipitate. Whether concentrated or weak, hydrochloric acid does not

dissolve scillitine, and gives no characteristic reaction. Hydrated alkalis disengage ammonia from scillitine, and thus show that it contains nitrogen.

Concentrated solutions of ammonia and potassa do not dissolve scillitine, but they decompose it and remove its bitterness. It gives a pale yellow precipitate with tannin; with perchloride of iron, an orange yellow precipitate; with perchloride of platinum, a yellow precipitate.

Projected on a sheet of platinum heated to redness, it liquifies, turns brown, catches light, swells up, becomes black, and disappears without leaving any residue.

Scillitine resembles the alkaloids in many points, which further experiments will doubtless render evident. Its reaction is alkaline, it contains nitrogen and forms a combination with acetic acid.

Scillitine has been the subject of several toxicological studies made by Dr. Gosselin, assisted by M. Adrien Bussy. These experiments have given the following results:—

1. That scillitine has all the characters of the acro-narcotic poisons described by Orfila.

2. That it is very poisonous in the dose of five centigrammes, and causes a strong inflammation of the digestive apparatus, even in the dose of from three to four centigrammes.

3. That when injected into the œsophagus, its first effect is to produce vomiting and violent purging; narcotism then manifests itself, and death appears to ensue from paralysis of the heart.

4. That scillitine, applied by the endermic method, acts much more quickly than by way of the stomach, that its action in this case is almost exclusively narcotic, and that it killed a rabbit in 37 minutes, in the dose of 0.02 gr., and a vigorous dog in an hour and 22 minutes, in the dose of 0.04 gr.

Journal de Pharmacie et de Chimie, February, 1857.

ANALYTICAL CHEMISTRY.

PROCESSES for the PREPARATION and ANALYSIS of OXIDE of URANIUM.

By M. L. KESSLER.

THE different processes which have been given to separate uranium from the metals which accompany it in the ores, all leaves something to be wished!

That of Arvedson, which is generally employed and which is the most exact, has the fault of leaving in the oxide of uranium oxides of nickel and zinc which, precipitated at the same time as the oxide of iron which retains them, are afterwards redissolved, as well as a notable portion of this latter, in carbonate of ammonia, and are found in the acid which evaporation separates from it. The last treatment of the protoxide does not easily or accurately effect their elimination.

On the other hand, if the crystallisation of the double uranico-potassic

or uranico-sodaic sulphates enables us to obtain oxide of uranium perfectly pure, this method is not analytical.

We have, therefore, considered that it might be useful to describe a process which we have found always successful, and which we have based, in the first place, on the great affinity of the alkaline carbonates for oxide of uranium; on the other, on the little affinity existing between uranium and sulphur.

The pechblende is dissolved in nitric acid, water is added, and it is precipitated at about 30° C. (86° F.) by sulphuretted hydrogen, so as to reduce the arsenic acid and to effect its separation by filtration, under the form of sulphuret, at the same time as that of the copper and lead. The iron is again oxidised in the liquor, either by chlorine or with hot nitric acid. Tartaric acid is added, then ammonia to saturation, and the whole remains in solution. Bicarbonate of soda, well saturated with carbonic acid, is then added; then, by subjecting it anew and rapidly to the action of sulphuretted hydrogen as long as the liquor precipitates, we separate the sulphurets of zinc, iron, nickel, and sometimes of cobalt, whereas the oxide of uranium remains in solution. These precipitates are washed with a dilute solution of bicarbonate of soda saturated with carbonic acid with sulphuretted hydrogen added to it. Evaporation and roasting enables us to collect the oxide of uranium.

It is probable, that for analysis, bicarbonate of ammonia would produce the same effects as that of soda, and would enable us to obtain by calcination, roasting, and if necessary at last, by deflagration with a little nitrate of ammonia, an oxide of uranium free from alkali.

In this process, care is necessary during the passage of the sulphuretted hydrogen into the tartaric liquor, to maintain in it an excess of carbonic acid which SH^2 is disposed to displace, and which by its presence prevents the oxide of uranium from becoming sulphuretted, as well as the metallic sulphurets from forming green sulphosalts, which would pass through the filter.

This is easily managed by using for the production of the sulphuretted hydrogen an apparatus in which hydrochloric acid attacks a sulphuret of iron mixed with a few pieces of marble.

We may also economically substitute cream of tartar for tartaric acid when we are not very anxious to avoid the presence of a little alkali in the oxide of uranium.

Journal de Pharmacie et de Chimie, March, 1857.

INDUSTRIAL CHEMISTRY.

On the MANUFACTURE of SOLUBLE GLASS. By M. BUCHNER.

SINCE the last researches of M. Kuhlmann on the soluble glass called Fuchs' soluble glass, from its inventor, attention has been attracted to this silicate, whose properties are so interesting. As described by Fuchs, soluble glass was silicate of potassa; but soda is now substituted for potassa, as being less costly, and giving a product very sui-

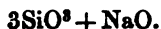
table as a substitute, under many circumstances, for the silicate originally employed.

The silicate of soda is obtained either by fusing a mixture of powdered quartz and carbonate of soda and charcoal in given proportions, or by dissolving amorphous silica in a soda ley. The latter process is the most expeditious, especially when it is executed on the large scale. At M. Kuhlmann's manufactory, the solution is effected in boilers, at a pressure of seven or eight atmospheres, and with a concentrated soda ley.

But the hydrate of soda is derived from carbonate of soda, which, in its turn, is produced from sulphate of soda. In the manufacture of soluble glass there was, therefore, a step to be gained, by applying to it the fact proved by Gehlin, and which is turned to account in glass-works—namely, that soda glass may be obtained by fusing a mixture of silica, *sulphate* of soda, and charcoal. This plan, conceived by Fuchs himself, has been carried out by Buchner, who, after careful experiments, has stopped at the following proportions:—

Silica	100 parts
Anhydrous sulphate of soda	60 „
Charcoal	20 „

The product corresponds nearly to the formula



The sulphate of soda employed in this proportion, is in slight excess, M. Buchner having thought that a small excess of alkali could only precipitate the solution of the silica; however, he obtained a very satisfactory result by using only 53 parts of sulphate of soda, instead of 60.

Neues Repertorium, fur Pharmacie.

INORGANIC CHEMISTRY.

RESEARCHES on SULPHUR. By M. BEETHELOT.

THE study of sulphur has always been a favorite one with chemists. The physical properties of this body, the varied compounds which it forms, the facility with which it unites with other substances, and is again disengaged from its compounds, finally the various states under which it presents itself, all concur in making sulphur truly a typical substance. From the time of the alchemists until this time, each result to which the examination of sulphur has led, has almost always been extended to a great number of analogous phenomena, and has thrown a fresh light on the general theories of chemistry.

The present researches are relative to the various states of free sulphur, and to the relation existing between these states and the nature of the sulphurous combinations which may be derived from them; this latter point is the essential object of this present work.

I shall first give a summary of the known facts relative to the states of sulphur; this body, indeed, notwithstanding its simple nature and chemical identity, has very various appearances, according to the conditions of its preparation, and the influences to which it is subjected. Sometimes it appears under the form of octohedral crystals, derived from the right rhomboïdal prism, sometimes under the form of oblique rhomboïdal prisms (Mitscherlich), sometimes in the state of soft sulphur, more or less liquid and elastic, often forming an emulsion with water, sometimes colored of a reddish tinge. It may likewise be obtained in the utricular form (Brame), or as an amorphous matter insoluble in sulphuret of carbon (Ch. Deville). These various states may be produced under the influence of a high temperature, followed by a more or less rapid cooling; the sulphur, disengaged by the reagents from its combinations, may affect this same diversity of properties (Fordos and Gélis and Selmi).

Among these very dissimilar states, whose variety is almost infinite, are there any fundamental and stable states to which all the others may be brought? If these states exist, have they any constant relation to the nature of the combinations from which the sulphur is disengaged? To ascertain this was the aim of my experiments.

I. States of Sulphur.—Among the states of sulphur, I have been able to distinguish two essential states, two stable limits to which all the others may be reduced, namely, octohedral or electro-negative sulphur, being the combustible body, which is amorphous and generally insoluble in solvents properly so called. The study of these two states simplifies that of the sulphurous combinations, and reduces them to a fundamental opposition. If they do not exist alone, at least all the others, whose detail may be infinitely varied, are intermediate and transitory states, and may be brought again to these two principal states in an indubitable manner.

In fact, with octohedral sulphur are connected two less stable states, prismatic sulphur and the soft sulphur of the polysulphurets, both spontaneously transformable into octohedral sulphur, under the influence of time alone. These three varieties are soluble in sulphuret of carbon.

Electro-positive sulphur may be obtained by disengaging sulphur from its oxygenous, chloruretted, and bromuretted combinations; the sulphur from the chloride and bromide forms the most stable limited state. It is amorphous and insoluble in solvents, properly so called (water, alcohol, ether, sulphuret of carbon, &c.)

With electro-positive sulphur, are connected three other less stable varieties.

1st. The soft sulphur of the hyposulphites, soluble in sulphuret of carbon, but becoming gradually insoluble by the simple fact of the evaporation of the solvent. The soft sulphur obtained under the influence of heat, and that which is produced by a mixture of sulphuret and hyposulphite (action of alkalis on sulphur), may be regarded as a mixture of these two species of soft sulphur, corresponding to the two states of the sulphur itself.

2nd. The insoluble sulphur obtained by exhausting flowers of sulphur alternately, with alcohol and sulphuret of carbon.

3rd. The insoluble sulphur obtained under the influence of heat, and exhausting soft sulphur with sulphuret of carbon. This last variety is the least stable of all; it is sufficient to cause it to boil with alcohol for a few minutes to transform it entirely, by the action of contact, into crystallisable sulphur soluble in sulphuret of carbon.

The color of these varieties is between lemon color and deep red; it depends on the circumstances of their production, and also often on some traces of foreign matters.

These varieties are distinguished from each other by the greater or less ease with which they are transformed into soluble and crystallisable sulphur, as well under the influence of a temperature of 212° F. (100° C.) as well as on contact with various bodies, such as the alkalis and their sulphurets, sulphuretted hydrogen and alcohol, acting at the usual temperature.

All these varieties of amorphous sulphur may be brought to the most stable fundamental variety, if we keep them in contact, without heat, with chloride of sulphur, bromide of sulphur, iodine, and even, up to a certain point, with fuming nitric acid.

All these varieties, on the contrary, are transformed entirely into octohedral sulphur if we expose them to reiterated fusions and sublimations; if we reprecipitate them after having dissolved them in an alkali, or an alkaline sulphuret; finally, if we keep them for several weeks in contact with a solution of potassa at the ordinary temperature; prismatic sulphur appears to be in certain cases one of the intermediary terms of this transformation.

In fact, all the forms of sulphur are reducible into two essential states, electro-positive sulphur, amorphous and insoluble; and electro-negative sulphur, or octohedral sulphur; of these two states, the latter is the most stable.

The preceding results represent the states of sulphur, independently of the circumstances in which they are produced; it remains to define the relation which these two states present to the combinations from which the sulphur may have been extracted.

II. The first point to be explained is, the existence of a constant relation between the sulphur and the compounds whence it is derived. Now, on the one side, the state of the sulphur disengaged from a combination is independent of the agent employed to disengage it, provided this agent is not alkaline, nor oxidising, and that its actions be *rapid* and without sensible disengagement of heat; on the other hand, the state of the sulphur disengaged from a combination is independent of the state of the sulphur with which it had been formed. I have combined successively, at the ordinary temperature, sulphur belonging to the various kinds with the following bodies:—potassa, alkaline sulphurets, anhydrous sulphuric acid, sulphite of soda, bisulphite of potassa, bromine, iodine; the sulphur disengaged from all the combinations presents a constant state independent of its initial state.

III. These facts established, we are able to seek for a relation be-

tween the state of the sulphur and the nature of its combinations. I have prepared sulphur in the following conditions, and it has presented itself in the following forms:—

1st.—Sulphur produced by the action of the Battery.—If we decompose with the battery an aqueous solution of sulphuretted hydrogen, the sulphur deposited at the positive pole, is entirely soluble in sulphuret of carbon, and crystallisable.

The electrolysis of sulphurous acid in aqueous solution, and monohydrated sulphuric acid furnish to the negative pole an amorphous sulphur insoluble in sulphuret of carbon.

2nd.—Sulphur produced by the decomposition of a sulphuretted compound.—The sulphur formed in the spontaneous decomposition of the polysulphuret of hydrogen (prepared with a *pure* alkaline polysulphuret) and polysulphuret of calcium, is entirely soluble in sulphuret of carbon, and crystallisable in octohedra. It is the same with the sulphur formed in the decomposition by acids of the *pure* sulphurets of sodium and ammonium. In all those compounds the sulphur plays the part of electro-negative, the element supporting combustion.

We obtain, on the contrary, amorphous and insoluble sulphur by decomposing by water or hydrochloric acid, hyposulphite of soda, trithionate of potassa, tetrathionate of soda, pentathionic acid, chloride of sulphur, chlorosulphuret of carbon, bromide of sulphur, and iodide of sulphur. In all these compounds, the sulphur plays the part of electro-positive combustible.

3rd.—Sulphur produced by the reciprocal reaction of sulphuretted hydrogen, and sulphuric and sulphurous acids.—This sulphur is similar to that of the decompositions of the thionic compounds and of chloride of sulphur. We know, moreover, that the thionic acids are produced in the reaction on sulphurous acid by sulphuretted hydrogen, and in the decomposition of chloride of sulphur.

The sulphur formed in these conditions, may be regarded as produced in consequence of an incomplete oxidation, according to the following facts:—

4th.—Sulphur produced in oxidising conditions.—If we set on fire sulphuretted hydrogen or sulphuret of carbon, so as to produce an incomplete combustion, we obtain amorphous and insoluble sulphur. Any sulphuretted compound, capable of yielding sulphur (thionic compounds, sulphuretted hydrogen, polysulphuret of hydrogen or ammonium, sulphurets of arsenic and copper), treated by fuming nitric acid, yield amorphous sulphur of the most stable variety. Amorphous sulphur is likewise produced in the reaction of sulphuretted hydrogen on sulphate of peroxide of iron, and on a mixture of bichromate of potassa and sulphuric acid.

The formation of electro-positive or combustible sulphur, in oxidising circumstances is worthy of remark; it proves that sulphur when produced, takes the state which it will possess in the oxygenous compound which it is forming. It is known that electrised oxygen or ozone may be prepared under the influence either of phosphorus, or

of some other very oxidisable substances, giving rise to a similar observation.

This fact is, moreover, conformable to the various actions of contact mentioned above, with respect to the states of sulphur. In fact, under the influence of chloride of sulphur, bromide of sulphur, iodine, and nitric acid, all bodies operating by contact, the various kinds of amorphous sulphur are all brought back to the most stable of all, namely, that which appears to exist, whether in pre-existent chloride and bromide of sulphur, or in sulphuric acid, which is disposed to form from the oxidising action of the nitric acid. On the contrary, under the influence of the alkalies, alkaline sulphurets and sulphuretted hydrogen, substances, some of which contain the sulphur which supports combustion, and of which others tend to make it play that part, the various kinds of amorphous sulphur are brought, by action of contact, to the state of octohedral sulphur, or electro-positive sulphur. The connection thus established, between the actions of contact and the electrical states of bodies will be remarked. In all the examples which I have given, it appears that the modifying body and the modified body, take opposite states of electricity, by virtue of the chemical phenomena which take place; thence would result the transformations of the sulphur and oxygen.

The interpretation of these phenomena is likewise corroborated by various observations relative to the unequal facility with which the divers varieties of sulphur unite with metals and other substances; I must particularly mention the following fact—electro-positive sulphur is dissolved easily and quickly in bisulphite of potassa, whereas octohedral or electro-negative sulphur is only dissolved with extreme slowness, and in very small proportion. Now, the compound which is disposed to be formed is the trithionate of potassa, which corresponds with electro-positive sulphur.

All these facts lead to one general conclusion—namely, that the states of free sulphur are connected with the part which it plays in its combinations; all these states, I repeat, may be brought to two fundamental varieties corresponding to the double part of sulphur; if the sulphur plays the part of electro-negative element, or supporter of combustion, analogous to chlorine and oxygen, it manifests itself under the form of crystallised octohedral sulphur, soluble in sulphuret of carbon. On the contrary, if it plays the part of electro-negative element, or combustible, analogous to hydrogen and the metals, it is manifested under the form of amorphous sulphur, insoluble in solvents properly so called.

These observations consequently furnish a fresh example of the connection existing between chemical phenomena and electrical phenomena: they establish the existence of the permanent multiple states which a simple body may assume under the influence of electrical forces acting at the same moment that the simple body is set at liberty. The formation of ozone (electro-negative oxygen?), many phenomena produced by the action of contact, and almost all the facts attributed to the *nascent state*, are doubtless due to an analogous cause.

However this may be, sulphur shows a more complete and better characterised type of this species of phenomena.

The analogy existing between the permanent states of sulphur developed by the action of heat and those which it takes when forming under the influence of electricity is not less remarkable; by this there is established between these two orders of phenomena a new link of so much the more importance as we find it reproduced in the study of another simple body—selenium; and perhaps even in phosphorus, as I shall now endeavor to show.

IV. The characters which approximate selenium and sulphur are well known: these two simple bodies form very analogous and sometimes isomorphous compounds. These resemblances are found even in the modifications which selenium undergoes under the influence of heat, and in the existence of several varieties of selenium, whether crystallised or amorphous—whether soluble or insoluble in sulphuret of carbon. It is likewise well known that selenium disengaged from its combinations does not always present the same properties: for example, the selenium obtained from the alkaline seleniurets is crystallisable, whereas the selenium reduced from selenious acid is amorphous and vitreous. Without entering into the details of these as yet little known states, I confined myself to decomposing, with the battery, hydroselenic and selenious acids, so as to obtain the selenium by turns at the positive and the negative pole. Now, the selenium disengaged at the positive pole during the electrolysis of the hydroselenic acid is completely or sensibly soluble in sulphuret of carbon. On the contrary, the selenium disengaged at the negative pole during the electrolysis of selenious acid is in great part insoluble in sulphuret of carbon; and the portion at first dissolved becomes almost entirely insoluble in it by evaporation alone, nearly in the same manner as the sulphur of the hyposulphites.

These facts are similar to those observed during the electrolysis of the acids of sulphur; they establish in the same manner the existence of two varieties of selenium, one electro-negative, the other electro-positive.

The various states taken by phosphorus under the influence of heat, namely: red phosphorus, amorphous, and insoluble in sulphuret of carbon, and white phosphorus, crystallisable and soluble in sulphuret of carbon, are likewise analogous to the states which sulphur takes under the same influence. Unfortunately, by reason of accessory circumstances, we can scarcely disengage phosphorus by electrolysis, from the combinations in which it plays an antagonistic part; but we must remark that red phosphorus may be formed under the influence of iodine, bromine, and chlorine, that it is produced as well by the incomplete combustion of phosphorus and phosphuretted hydrogen as by the reaction of chloride of phosphorus on phosphuretted hydrogen. Now these conditions are all similar to some of those in which electro-positive amorphous sulphur is produced. We must likewise observe that the heat of combustion of red phosphorus, and that of amorphous sulphur are both less than those of white phosphorus and octohedral

sulphur (Fane). From these various characters we are led to assimilate these two substances, and to regard with some probability red, amorphous, insoluble phosphorus as the analogue of electro-positive amorphous, and insoluble sulphur, and white soluble crystallisable phosphorus as the analogue of electro-negative, soluble and crystallisable sulphur.

I cannot without hazarding premative hypotheses, enter into the question of how far these analogies in the states of selenium, sulphur, phosphorus, and even of oxygen extend to the various states observed in the study of most of the metalloid bodies, especially carbon, boron and silicium. I will merely remark that crystallised carbon becomes amorphous under the influence of electrical heat, and that the carbon which separates from carburet of iron is crystallised; phenomena which are to a certain point analogous to those observed with sulphur.

These questions are all the more delicate, inasmuch, as the greater part of the simple bodies are incapable of being disengaged like sulphur, from their combinations under the influence of weak actions which are capable of being regulated; they do not readily take states possessed of distinct and easily recognisable characters. Moreover, all will not play two well defined antagonistic parts; all do not manifest themselves in the free state in several permanent states of equilibrium.

Now these are the circumstances that give such peculiar interest to the study of sulphur; free sulphur, silicium, oxygen, and phosphorus are found in various states, endowed with different physical and chemical properties, and I think that I have established that these states, in the case of sulphur in particular, may be connected with the various chemical functions which this simple body fulfils in its combinations.

Journal de Pharmacie et de Chimie, March, 1857.

TOXICOLOGY.

On the SEARCH for PHOSPHORUS in CASES of POISONING.

By M. MITSCHERLICH.

THE process which M. Mitscherlich has found the most successful for discovering phosphorus in alimentary substances—especially in flour—consists in treating the matter with a little sulphuric acid and water, and subjecting the whole to distillation in a retort, which by means of a tube bent twice at right angles is in communication with a refrigerator placed vertically. In the part of the tube where the vapors condense, we remark, in the dark, a light bulb, a luminous ring. 150 grs. of liquid containing a thousandth per cent.—that is, one hundred thousandth of phosphorus—may be reduced by distillation to 60 grs. without the light ceasing to manifest itself. Having continued an operation of this kind for half an hour, it was interrupted, and the retort left open to the air for a fortnight; the distillation

was then recommenced, and immediately the light appeared with all its original intensity.

If the liquid contains substances which, like alcohol or ether, impede phosphorescence, the light will not be produced until these matters are completely volatilised.

At the bottom of the receiver will be found globules of phosphorus. Five ounces of paste containing a third of a grain of phosphorus gave these globules in such quantity that one tenth would have sufficed to ascertain their exact nature. We can also wash some of these globules with alcohol on a filter; on placing the latter in a warm place the phosphorus will take fire.

When we operate on matters containing larger proportions of phosphorus, a certain proportion of phosphorus acid is produced by the incomplete oxidation of the phosphorus, which is easily found with nitrate of silver and bichloride of mercury, and which nitric acid transforms into phosphoric acid.

It was important to ascertain whether phosphoric and phosphorous acids were carried off when contained in an aqueous liquid subjected to distillation. M. Mitscherlich has ascertained that if this operation is conducted with care, so as to avoid projections, the product contains neither of these acids.

Journal für Praktische Chemie.

On the SEARCH for PHOSPHORUS.

By MM. DE VRIJ and VAN DER BURG.

SOME time ago M. Mitscherlich published a method for finding phosphorus in cases of poisoning. With respect to this method M. Nicklès remarked that the author does not say if this process is applicable in cases where phosphorus is associated with a fatty body; this observation led us to think that it might not be uninteresting to publish the results which we have obtained by using this process on the occasion of a case of poisoning. Last September we had to discover whether any kind of poison whatever existed in the organs of a person who had died in a suspicious manner. As there had been a report spread that the deceased had attempted suicide by eating lucifer matches, we tried M. Mitscherlich's method. As the result was negative we imagined that the bad odor of the organs on which we experimented was the cause of our not observing the faint light whose production is, according to M. Mitscherlich, prevented by the presence of essence of turpentine and some other substances. To ascertain whether this supposition were well founded we put 1 milligramme of common phosphorus, wrapped in a piece of blotting paper, into the flask containing the organs. As soon as the liquor began to boil, we perceived the light very distinctly, although the liquor contained very small globules of phosphorus. On treating this liquor with nitric acid, and then with ammonia and sulphate of magnesia, we obtained a pre-

precipitate of phosphate of ammonia and magnesia. Satisfied with this result, we were enchanted with the sensitiveness of the method mentioned; at the same time the remark of M. Nicklès struck us; so we thought it advisable to repeat our experiments, and thus determine that question. We therefore dissolved 0.0165 gr. of ordinary phosphorus in 31.2 gr. of olive oil. One drop of this solution, weighing 0.0235 gr., and containing consequently 0.000015 gr. of phosphorus, was incorporated with a mucilage of gum arabic, and then mixed with water, so as to make up the quantity of liquid to 300 gr. This liquid consequently containing 0.00000015 or 0.00000005 of its weight of phosphorus, gave us the phenomenon of brightness in the most striking manner. So as to ascertain that only the poisonous phosphorus was found by this means, we subjected red or amorphous phosphorus to the same treatment. As we expected, the result was negative whenever the red phosphorus was pure and quite free from ordinary phosphorus. But if the red phosphorus contained the least trace of ordinary phosphorus, its presence was immediately proved by the appearance of the light. From these experiments we conclude that, in cases of poisoning with lucifer matches, which are not uncommon, the presence of poisonous phosphorus may be ascertained by using M. Mitscherlich's process, in preference to that of M. Dusart, because the method of M. Mitscherlich discovers only poisonous phosphorus, and thus enables us to estimate the quantity of the poison, whereas M. Dusart's method may lead us into error, because the coloration of the hydrogen flame may be produced as well by red, non-poisonous phosphorus. Toxicologists will understand how essential it is to draw the distinction between the two modifications of phosphorus, because of the two kinds of lucifer matches now in use.

Journal de Pharmacie et de Chimie.

On the DETECTION of STRYCHNINE in CASES of POISONING.

By DR. J. E. DE VRIJ, *Lecturer on Chemistry*; and E. A. VAN DER BURG, *Pharmaceutical Chemist of the Rotterdam Hospital.*

IN the month of September, 1856, we were required by the *Juge d'Instruction* to make a chemical investigation of the contents of the body of a man, on the cause of whose death there was some suspicion of poisoning. The man having been brought to the hospital in a tetanic state, we were naturally induced to try to find strychnine in the contents of the body; but neither this nor any other poison was to be found, so that the death of this man remained unexplained.

The discordant opinions of several chemists in the famous trial of Palmer, made it necessary, for our own persuasion of the truth, to institute a series of experiments, to make out:—1st. The sensibility of the principal reagents on strychnine; 2nd. The possibility of detecting strychnine with these reagents, if the strychnine is mixed with animal

substances; 3rd. If strychnine can *always* be found in the corpse of an individual poisoned by it.

1st. Sensibility of the principal reagents on strychnine: *Chromate of potash or ferridcyanide of potassium and concentrated sulphuric acid.*—By these reagents $\frac{1}{30000}$ of a grain of strychnine can be detected, if one drop of a solution, containing one grain of strychnine in 60,000 grains of water, is evaporated in a small porcelain dish on a water-bath, and the remaining substance moistened with the smallest possible quantity of pure concentrated sulphuric acid. By introducing in this solution a *very small* fragment of a crystal of bichromate of potash or ferridcyanide of potassium, and moving this fragment with a glass rod in the solution, a beautiful dark purple color is produced on every part of the surface of the porcelain that has been in contact with the acid solution, and the fragment of one of the two salts.

Bin-iodide of potassium, and iodide of mercury and potassium.—By a solution of one of these compounds, $\frac{1}{30000}$ of a grain of strychnine can be detected. These reagents, like the following, possess only the ascertained sensibility, provided the drop of liquid is contained in a capillary test-tube, in which the liquid, although only a drop, forms a small column, in which the formation of a precipitate can be observed by comparison with a similar capillary tube filled with pure water, and mixed with the reagent.

Tannic acid reveals $\frac{1}{30000}$ of a grain of strychnine.

Solution of chlorine in water, $\frac{1}{30000}$.

Sulphocyanide of potassium, $\frac{1}{30000}$.

Neutral chromate of potash, $\frac{1}{30000}$.

The precipitate formed by bin-iodide of potassium is brownish-red, and if dissolved in weak warm spirit, acidulated by sulphuric acid, beautiful crystals are formed of sulphate of iodo-strychnine, which polarize the light, as has been discovered by Mr. Herapath. The precipitate formed by iodide of mercury and potassium, by tannic acid, and by solution of chlorine in water, are white. This last reagent must be used in relatively large quantity, and the precipitate formed by it does not appear immediately.

The precipitates formed by sulphocyanide of potassium and neutral chromate of potash are both crystalline. The color of the former is white, and the form of the crystals observed by the microscope is very characteristic. The color of the latter is a beautiful yellow. The formation of both these precipitates is accelerated by rubbing the surface of the tube with a glass rod.

The precipitate formed by chromate of potash gets immediately a dark purple color, if moistened by concentrated sulphuric acid. *All* the other precipitates get the same color if they are dissolved in a small quantity of strong sulphuric acid, and the solution brought into contact with a fragment of a crystal of chromate of potash or ferridcyanide of potassium.

2nd. The possibility of detecting strychnine, if mixed or combined with animal substances.

In all the following experiments, the method of Professor Stas was

used. This method, by which all organic basic poisons can be detected, consists in treating the animal substance which is supposed to contain a poison, with spirit of wine, acidulated by pure oxalic or tartaric acid. The tincture, after having been filtered, is evaporated at a gentle heat on a water-bath, and the remaining substance dissolved in anhydrous alcohol. This solution filtered, and again evaporated, and the remaining substance dissolved in water. The watery solution, after having been filtered and partly evaporated, is saturated with bicarbonate of soda, and afterwards repeatedly agitated with ether, which is made alkaline by a small quantity of caustic potash or soda. If there is any basic organic poison present in the animal substance under examination, it will be obtained by evaporating the ethereal solution, and may be afterwards tested by several reagents to find out its nature.

1st Experiment.—A solution of $\frac{1}{4}$ of a grain of strychnine mixed with 6 ounces of fresh meat. The poisoned meat treated as mentioned, afforded small crystals of strychnine, of which the identity was proved by the reagents mentioned.

2nd Experiment.—The white and yellow of one egg were mixed with $\frac{1}{4}$ of a grain of strychnine, and this mixture coagulated by the heat of boiling water. The coagulum being treated according to Stas's method, *almost the whole quantity* of the strychnine was recovered in white crystals.

3rd Experiment.—The urine of 24 hours from a patient in the hospital, to whom the physician administered every day $\frac{1}{4}$ grain of nitrate of strychnine, was divided into two equal parts, and one of these parts mixed with $\frac{1}{4}$ grain nitrate of strychnine. The two parts treated in the same way as described by Messrs. Graham and Hofman,* by digesting and agitating during 24 hours with animal charcoal, &c., that part in which $\frac{1}{4}$ grain of the strychnine-salt was dissolved, afforded us crystals of strychnine, *whilst we could not find the least trace in the other part.*

4th Experiment.—On the 15th of September, $\frac{1}{4}$ grain of strychnine was administered to a young dog. Twenty minutes after the introduction of the poison, its action commenced by vehement tetanic spasms, salivation, and excretion of urine, and ten minutes later the dog was dead.† The stomach, liver, gall, bladder, spleen, kidneys, intestines, and blood were taken from the body and separately examined on the 18th September, when they all were in full putrefaction. The result of this investigation was, that a comparatively large quantity of strychnine was obtained from the stomach, whilst *no one of the other entrails* contained the least trace of this poison.

* *Annalen der Chemie und Pharmacie*, Bd. 83, S. 89.

† The same dose of poison, mixed with half an ounce of animal fat, was administered to another dog of somewhat larger size, for the purpose of testing the assertion of Dr. Pindell, published some time ago in the *American Journal of Pharmacy*, that fat would neutralize the tonic effect of strychnine. The result, however, proved fatal to the animal, which died an hour and a quarter after the ingestion of the poison.

In this experiment, the admixture of fat to the poison had no other effect than to retard (and not to neutralize) its mortal effect.

3rd. Can strychnine *always* be found in the corpse of an individual poisoned by it?

After we had proved that the least trace of strychnine could be detected if it was really present, we desired to get the conviction if poisoning by strychnine could *always* be proved by the aid of chemistry. The following experiments were executed with a view of determining this point:—

1st Experiment.—A middle-sized dog was poisoned by introducing a solution of nitrate of strychnine in a superficial wound. Immediately after death 4 ounces of the blood were treated according to Stas's method, but not the least trace of strychnine could be detected.

2nd Experiment.—On the 26th September we administered to a small dog weighing about 8 pounds, at 10 o'clock in the morning, $\frac{1}{10}$ grain of strychnine mixed with $\frac{1}{10}$ grain of tartrate of antimony and potash. This dose was repeated twice on the following day, at 9 o'clock in the morning, and at 4 o'clock in the afternoon, without any symptom. On the following day the dose was repeated at 9 o'clock, and again at 12 o'clock in the morning, and at 1 o'clock the dog had a violent attack of tetanus, accompanied by salivation and ejection of urine. This attack lasted some minutes, and was soon followed by a second, which was still more violent. At 3 o'clock the dog got a third, and at 8 o'clock a fourth dose of poison, each of which was followed by an attack of tetanus, one hour after the introduction of the poison. On the 30th of September the situation of the dog was much better compared with that of the preceding evening. At 10, and again at 12 o'clock, a dose of poison was administered, which doses were followed now and then by spasmodic contractions, whilst it appeared that the irritability of the nerves was much increased, for the dog jumped up or trembled at the least noise. At 2 o'clock in the afternoon a third dose of poison was given, which was followed at half-past 2 and half-past 3 o'clock by attacks of tetanus, the latter lasting ten minutes. The fourth dose was given at half-past 5 o'clock. One hour later the dog got the most vehement attack of tetanus, accompanied by salivation and ejection of urine and feces, which lasted a quarter of an hour, and terminated by the death of the animal. This death was caused by a chronic poisoning, begun on the 26th of September, and terminated on the 30th of September, in which lapse of time the dog got no more than $\frac{1}{10}$ ths of a grain of strychnine, and $\frac{3}{10}$ ths of a grain of tartrate of antimony and potash, which small dose of poison caused seven attacks of tetanus. After each attack the widely-extended hind feet were much stiffer than the fore feet.

The *post-mortem* examination of the body, which was very carefully performed by the distinguished Assistant-Physician of the Rotterdam Hospital, Dr. Schmidt, proved that there was nothing abnormal to be found which could even lead to the suspicion that death had been caused by the administration of a poison. The chemical analysis of all the parts of the body led to the same negative conclusion, for not the least trace of strychnine could be detected, notwithstanding the great sensibility of our reagents which was proved in the former experiments.

From the alleged facts we may be justified in making the following

CONCLUSIONS.

1. That the method which we pursued for detecting strychnine was adapted to discover the least trace of the poison, for even only $\frac{1}{10000}$ of a grain could be detected.

2. That even when the strychnine is combined with albuminous matter, nevertheless, the *whole* quantity can be separated by the method of Stas, if properly conducted.

3. That if death has been caused by strychnine, this poison can be detected in the body, provided it has been administered in a quantity *more than sufficient* to cause death.

4. That if the poisoning by strychnine has been chronic, and has resulted from a quantity not greater than just necessary to cause death, the cause of this death *cannot be proved*, either by the *post-mortem* examination of the body, or by a chemical investigation of the intestines.

5. That it appears to be highly probable that that part of the strychnine which acts mortally is decomposed in the living body.

6. That the urine of patients who take strychnine or its salts as a medicine, contains not a trace of this poison.

Pharmaceutical Journal, March, 1857.

PHYSICS.

On a POWERFUL MODIFICATION of the INDUCTION COIL and some NEW ELECTRICAL PHENOMENA developed by IT.

By JONATHAN N. HEARDER, Plymouth.

THE medical or galvanic coil machine is an instrument so well-known, that it would seem to require very little description. Almost every tyro in electricity has sought to attain to the construction of one of these instruments in one form or another, and the satisfaction derived from a successful effort has been often felt equivalent to taking a degree in electrical science. The forms which have been given to the electro-magnetic coil machine have been of a very diversified character, some have been content to copy from the labors of their predecessors, whilst others have, either from preference or necessity, acted upon their own acquaintance with the laws and phenomena of electro-magnetism, and thus, the qualities and characteristics of these instruments have been as varied as their forms. The principal object with all appears to have been the production of a shock of various degrees of force, and very little more than this has ever been aspired to, though it is difficult to understand how a phenomenon, capable of so extraordinary an amount of extension, and occasionally giving such striking evidences of the existence of this susceptibility, should have remained so long undeveloped, the instrument itself assuming very little more than the character of a scientific toy. On several occasions, when the conditions have been such as to favor a more than ordinary

development of the power of the induced current, unusual effects, such as tension sparks as much as $\frac{1}{2}$ of an inch in length, have been produced, but until within the last two or three years, no attempt has been made to increase these effects by any modifications of the arrangements necessary to produce them.

Having been myself many years engaged in the manufacture of an improved form of the medical coil machine, for which, in 1846, I received the silver medal from the Royal Cornwall Polytechnic Institution, I had often been struck with the exalted statical condition of the secondary terminals, especially in some of my largest machines, although no attempts at insulation beyond the ordinary cotton covering of the wire were made, and I often promised myself to try the effect of a more perfect insulation. From want of leisure and other circumstances, I was not induced to carry out the experiment until I heard of the effects produced by a machine manufactured by M. Ruhmkorff, of Paris, and finding on inquiry that it was nothing more than a powerful induction coil, the precise arrangement of which, however, from its peculiar mode of construction, could not be well ascertained, I immediately proceeded, in the absence of any definite information about it, to act upon my original suggestions, and construct an instrument upon my own plan. In this I have not only had the satisfaction to succeed beyond my expectations, but also to find that the arrangement which I have adopted appears to admit of a very considerable amount of extension.

In order clearly to understand the circumstances which give rise to this extraordinary increase of power in the induction coil, as it is now called for the sake of distinction, it will be necessary to make a brief analysis of the principles involved in the construction of electro-magnetic coil machines in general. They are based upon two facts first discovered by our illustrious Faraday, viz., 1st. When a current of voltaic electricity passes through a metal helix, an inverse current of transient duration takes place in the same wire, in a direction opposite to that of the battery current, at the instant when the battery current begins to circulate; and when the battery current is suspended, another wave or current of a similarly transient character takes place in a direction opposite to the first one. 2nd. If the first metal helix be surrounded by a second helix which shall be insulated from and totally independent of it; these secondary waves or currents will be found to take place in the second helix as well as in the first, although none of the battery current passes through it. These effects are greatly enhanced when the helices are made to include an iron bar, or still better, a bundle of iron wire. These experiments are the foundation upon which all electro-magnetic induction coils are constructed, and their various qualities depend on the judicious adjustment of the two helices and the battery to each other. As the development of these phenomena depends upon certain definite laws, it will be necessary to describe these a little more in detail:—For this purpose, induction coils may be considered to consist essentially of three parts, whose functions must be separately examined, viz., first,

the primary coil, whose office is to conduct a current of dynamic electricity from a voltaic battery. 2nd. A soft iron core intended to take up, concentrate, and radiate, the magnetic influences of the battery current. 3rd. The secondary coil, whose function is to develop the electrical effects due to the reaction of any uncontrolled magnetic intensity excited in the iron core.

In the new form of induction coil, another element, and a most essential one, invented by Fizeau, has been introduced, called the condenser, but as it is independent of the others, and has not hitherto entered into the construction of the ordinary coil apparatus, it will be considered in another place. It is scarcely possible to contemplate the action of any one of these elements apart from the rest; they are so intimately connected with each other, that trifling variations in the character or arrangement of any of them will influence very materially the action of the rest. In considering the iron core as a separate element, I am aware, that although its presence is found to exalt the effects of the induced current, it is not generally considered that it originates any portion of that current; but I have long entertained an opinion, that, in all cases in which secondary or statical currents are developed by the action of primary ones, they do not depend upon any peculiarities of the primary currents themselves, but upon the magnetism developed by them, and that they are, indeed, dependent upon magnetic action solely, no matter how produced. I have kept this principle in view, not only in the construction of my medical coils, but in my new induction coil, and find the correctness of the principle constantly established.

In the construction of the ordinary galvanic coil machine, very little attention appears to have been paid to the relation existing between the primary wire and the battery current, so as to judiciously apportion the resistance of the former to the electro motive character of the latter, hence, machines having the same quantity of secondary wire give very different effects with the same battery, in consequence of the diversified arrangement of their primary-coils. It must be remembered, that the quantity, as well as the intensity of voltaic pairs will vary as much as from one to four, and as the effect of resistance in diminishing the power of a battery current is very considerable, too great attention cannot be paid to the judicious arrangement of this element.

The effect of the passage of the voltaic current through the primary coil is to develop magnetism; whether within the coil or externally to it is a matter of question, though I believe the latter to be the case. If an iron wire be included within this primary coil, the magnetism thus developed is taken up by it, and distributed throughout its whole mass, and by this means the core becomes a homogeneous magnet, representing the sum of all the forces of the surrounding coils, which are equivalent to so many little isolated magnets acting under disadvantageous circumstances. This magnetic condition of the iron core being due to the action of the voltaic current traversing the wire, will be modified, not only by the varied character of the current itself, but

by any variations in the length and resistance of the wire, and will only last as long as the exciting cause is kept up. This development of magnetism is, however, not the only phenomenon which takes place here, for it was found by Faraday, that as a voltaic current originates magnetic action, so magnetic action itself is capable of giving rise to an electric current, and this latter, being the result of reaction, takes place, as might be supposed, in the opposite direction; so that, in point of fact, when a voltaic current is traversing a wire in one direction and producing magnetism, the wire is also traversed by a second current, due to the reaction of the magnetism, in an opposite direction. It is not very easy to understand how a wire can thus carry two currents at once in opposite directions, unless the currents may be supposed to partake of different natures, which is not at all improbable. This secondary or induced current is, however, extremely transient, existing only during the time occupied in the change of the iron from the normal to the magnetic state; for, after the first impulse, the wire, so far as regards the second current, is neutral. When the battery current is suspended, the sudden change in the core from the magnetic to the normal state originates a new induced current in the wire in a direction opposite to the first. Now, as the first induced current was in opposition to the battery current, so the second, being the converse of the first, is in the same direction as the battery current; but in this latter case the wire is carrying one current only, viz., that induced by the magnetism. There is a considerable difference in the character of these two induced currents. The former, being the result of a change from a state of rest to a state of activity, is comparatively gradual in its development; but the latter, resulting from the sudden destruction of the magnetic intensity, is instantaneous, and characterised by a greater intensity of action, increasing in proportion to the length of the coil, and the exalted magnetic state of the core. The intensity of the induced current derived from the interruption of the current is so great that it overleaps the interval at the points of separation, in the form of a statical spark, thereby completing the circuit. The length of this static spark is greater in proportion to the length of the secondary wire, but with a single wire there are no means of ascertaining what length of spark might really be obtained, as the whole of the current generated in the wire passes at the instant of the solution of contact; its physiological and galvanometric effects, however, afford some evidence of its high intensity, and some galvanic-coil machines are constructed with a single wire only, under the erroneous impression that, because only a primary coil is used, the shock which is produced is also the effect of a primary current, or the direct current of the battery; whereas the shock is the result of a secondary or induced current as much as if it circulated in a secondary wire, since it occurs, not when the battery current is passing, but when it ceases to pass.

If a second wire be made to surround either the first helix, or any portion of the iron core, so as to be brought under the influence of its magnetism, induced currents will be excited in it coincident with

those of the primary coil, although it shall be perfectly insulated from it. The static intensity of these currents will be greater in proportion to the length of the wire and the exalted magnetic condition of the core, and if the wire be covered with cotton, and be of considerable length, static sparks may be obtained at the terminals, even though they may be in contact with the wooden portions of the instrument. I had often noticed these effects with my own medical-coil machines as early as the year 1842, especially with the more powerful ones, from which bright and vivid sparks were elicited between coke terminals, and Leyden jars could be charged so as to give an appreciable spark and affect the gold leaf electrometer. Judging from such effects as these, in the absence of any approximation to good insulation, that much longer sparks might be obtained if the secondary wire were perfectly insulated, I had often promised myself to try the experiment, but neglected to do so until stimulated by the improvements of M. Ruhmkorff.

Various attempts have been made from time to time to increase the static power of the secondary wire by increasing its length, but, for want of sufficient attention to its insulation, no effects have been hitherto obtained at all commensurate with the extra quantity of wire employed. Some years since Mr. Henley constructed a powerful arrangement of primary and secondary coils, which gave static sparks in free air of 1-eighth of an inch in length, with a battery of moderate power. With a more perfect insulation, however, and with the employment of the new essential element, Fizeau's condenser, doubtless Mr. Henley's effects would have been very considerably increased. Whatever effects have been produced with ordinary modes of insulation, they have all fallen very far short of those produced by M. Ruhmkorff, who has the merit of having been the first to give to the machine an entirely new character, and to endow it with properties which it never before possessed. This he has accomplished by great attention to the insulation of his coils, but what the precise means are which Ruhmkorff has employed, or what are the exact dimensions of his primary and secondary coil, there are no easy means of ascertaining, since the whole is embedded apparently in melted shellac, by which it is so thoroughly compacted together as to prevent the possibility of unwinding it without the risk of spoiling the instrument. Report says that Ruhmkorff's instruments usually contain from 4 to 5 miles of secondary wire, though some have been made of more than twice this size, and the different coils are, through the intervention of the shellac, so far insulated from each other as to enable the induced current to obtain a degree of tension sufficient to overleap an interval of $\frac{1}{4}$ an inch or more. This increase of power is, however not entirely owing to the more efficient insulation, for without the addition of the new element, viz., the condenser, the sparks are not one-eighth of an inch in length. Ruhmkorff appears to have satisfied himself that he had obtained pretty nearly the maximum of effect from the quantity of materials which he has employed, since, although it would appear that he has constructed some scores of the instruments, I believe they

do not usually give sparks exceeding 5 or 6 tenths of an inch in length. The induction coil, however, as presented to the scientific world by Ruhmkorff, has observed to develop an entirely new class of phenomena, and to open a field for investigation of a novel and interesting character. Before proceeding to describe any further improvements of the apparatus, it is necessary to say a few words on the nature and action of the condenser, which appears to be the most important agent of the whole. This consists of an extensive surface of oiled silk, or other thin non-condensing tissue, coated on each side with tin foil after the manner of a Leyden jar, leaving round the tin foil an uncoated margin. This for economy of space, may be folded into any convenient form, provided the opposite coatings be kept from touching each other. When used, a wire or conducting strip of metal from each coating, is connected with the portions of the primary coil where the interruption is made, so that when the current is broken the interrupted ends are respectively in metallic communication with the opposite coatings of the condenser. What the rationale of its action may be is at present rather doubtful, but, in the absence of a more satisfactory solution of the problem, it may be supposed that at the moment of breaking the contact, the induced current exhibits its intensity at the points of separation by overleaping the interval, but if these two interrupted ends be in contact with the extended conductors of the condenser, a portion of this intensity may possibly be reduced by its being determined in the direction of the two conductors, which, by inducing upon each other, have their capacities for electrical change considerably increased, and thereby act as capacious reservoirs in which these intensities may, as it were, expand and exhaust themselves. Be this true or not, the strongest machine without a condenser dwindles into insignificance. Its reaction upon the secondary coil, however, demands a little more notice. Without the condenser the secondary current due to the inductive action of the magnetic change in the iron core, from the active to the passive state, would be divided between the coils surrounding the core, a portion of it circulating in each, but the greater portion, perhaps, would circulate in the one which afforded the least resistance, and as this would be the primary coil, it would follow that the intensity of the secondary one would be diminished just in proportion to the quantity circulating in the primary one. The condenser seems partly to intercept the circulation of the induced current in the primary coil, and thereby concentrate its action more particularly upon the secondary one, the statical condition of which is thus greatly exalted. The form which Ruhmkorff has given to his instrument is at once compact, light, and elegant; the coil itself is about 12 inches long and about 4 inches thick, having plate glass ends, through the centres of which project the ends of the iron core. One end of this core attracts a little iron hammer which rests on an anvil, the touching surfaces of which are armed with platina. The anvil and hammer, being made part of a circuit, constitute together an interruptor, contact being broken whenever the hammer is attracted up from the surface of the anvil. In addition to

this the instrument is furnished with a commutator, for the purpose of reversing at will the direction of the current. The ends of the secondary coil pass out through holes in one of the glass plates, and are connected with binding screws, fastened on the tops of two little glass pillars serving as insulated terminals, with which may be connected any apparatus for the purpose of experiment. The following are a few of the phenomena developed by the instrument. When two small wires connected with the terminals, have their free ends brought near to each other, streams of sparks pass between them at a distance varying from 0.4 to 0.6 of an inch, when 10 or 12 cells of a nitric acid battery are connected with the primary coil of the instrument. These sparks inflame combustible substances such as gunpowder, alcohol, guncotton, writing paper, cork, &c. When connected with two insulated wires projecting into the interior of a globular or egg-shaped receiver of 5 or 6 inches diameter, a brilliant atmosphere of light is produced on exhausting the air, and when the intensity is carried beyond a certain point crimson and violet sparks pass between the terminals some three or four inches in length. A Leyden jar having its two coatings respectively in connection with the two terminals, and having a second discharging wire also leading from its outer coating to within a short distance of the wire connected with the inner coating, becomes charged so as to discharge itself spontaneously over this small interval at every interruption of the primary circuit. Many other effects might be noticed, but they will be alluded to more at length in connection with a more powerful instrument which I have now to describe, and which is one of my own construction. My first step in the construction of this new instrument was to make all the parts separate, and independent of each other, so that I could by this means ascertain what influence modifications in any one of them would exert upon the rest, and I was soon able to put these various parts together, so as to form one entire machine. My coil was six inches in length, and contained about $1\frac{1}{2}$ miles of fine secondary wire; it was wound upon a hollow bobbin of wood, covered with gutta percha, and having its centre large enough to contain the primary coil with its iron core, of which I constructed several modifications, to suit batteries of different electro motive force. The secondary wire was covered with silk, and the layers insulated from each other with oiled silk and gutta percha. I also made a variety of condensers, and found the following substances well adapted for them, viz., common cartridge paper, varnished paper, oiled silk, gutta percha, and thin vulcanised india-rubber. I likewise varied the form of the condensers so as to bring both surfaces of each sheet of tin foil to act, thereby doubling the inducing surface, and this I effected by placing sheets of tin foil between sheets of the insulating tissue and connecting the alternate sheets at opposite ends; or by employing an extra insulating layer upon one of the sheets of tin foil, and folding the whole over and over; or again by rolling the same arrangement upon a round roller of wood, &c. With this machine I was able to obtain sparks between the terminals more than one inch in length and to

charge a Leyden jar containing three square feet of surface, so as to give a torrent of brilliant discharges between platina terminals. With this instrument I discovered some curious phenomena connected with the alterations in the character of the induced current, under the influence of the Leyden jar, and first shewed its thermal effects upon a wire forming part of the circuit. In September last I exhibited this machine at the Royal Cornwall Polytechnic Society, and was there honored with the Society's first silver medal. Although it contained only one-third of the quantity of secondary wire said to be used in Ruhmkorff's machines, the effects produced from it were very much more powerful. Whether this arose from a more perfect insulation, or from anything different in the arrangement of the coils, I am hardly prepared to say, knowing so little of Ruhmkorff's plan; but I have discovered by experience that the better the insulation, the more powerful the machine.

Since this first instrument I have constructed others of various degrees of power, and the effects produced by some of them have not only greatly surpassed all that have been previously observed, but I have also been able to develop some entirely new phenomena through their agency. The principle upon which these machines are constructed is still the same, but the difficulties in the way of preserving a perfect insulation under very high powers are such as I had not at all anticipated. The mode, however, which I have adopted in the construction of my instruments I have found most advantageous, since it affords the means of examining from time to time the phenomena which have taken place in the interior of the coil during its long-continued action, or when subjected to peculiar conditions. Although a very perfect insulation develops a high state of tension in the secondary current, yet even this may be forced too far, just as we may overcharge a Leyden jar, and fracture the glass, for if we attempt to force the instrument beyond a certain point, some portions of the insulation break down, and the electricity, having once made a way for itself, will always pass subsequently with greater facility, thereby lessening the maximum power of the current. In Ruhmkorff's instruments accidents of this kind are irreparable, from the solid character of the insulating medium; and their arrangement, moreover, prevents the development of any high power, since the insulating interval of glass between the body of the coil and the copper band which secures it to the base of the instrument is very small. In my own instruments the secondary coil is so arranged that it can be unwound, and all the insulating layers taken out to examine their condition, or repair accident, &c. My primary coils, also, are detached, which is another important desideratum, as they can be changed for the purpose of experimenting with batteries of different electro-motive characters. They are also furnished with extra binding screws, for connecting any number of them consecutively or collaterally. The condenser is also capable of being thrown out of connection. Having described the rationale of the action of the induction coil, together with the mode

of construction, it now remains to illustrate its action by a few experiments.

Experiment 1. When the terminals of the secondary or induced wire are connected respectively with the wires of a universal discharger, the ends of which are furnished with pointed platina wires to serve as electrodes, bright thick crepitating sparks pass between the platina points when they are brought within a certain distance of each other, and with a powerful machine and a battery in good action the points may be separated nearly four inches, when the sparks assume a zigzag character, the deflections being so great as to make the real length of the sparks sometimes as much as one-fourth or one-third more than the interval itself.

Experiment 2. If, whilst sparks are passing between the points, the condenser be disconnected, the sparks disappear, but re-appear on connecting it again. An instrument which will give a two-inch spark with a condenser will not give one of an eighth of an inch without it.

Experiment 3. If when sparks are passing at the distance of half an inch the points be gradually approximated, one of them, viz., that forming the negative electrode, will become red, and even white hot, and if the machine be powerful both become ignited on a nearer approximation to each other; this, however, is not the case with Ruhmkorff's machine. If very fine iron wires be substituted for the platina electrodes, the negative wire will burn away, rapidly fusing into globules, and throwing out brilliant scintillations.

Experiment 4. If the points be placed beyond the striking distance sparks and brushes of light will be seen passing from the positive electrode, over a portion of the interval, and the negative electrode will be illuminated by its characteristic star of light, also throwing off smaller brushes, which re-curve over the wire, and either terminal will give sparks to an uninsulated conductor brought near to it, which will be longer if the opposite terminal be connected with the ground: this, however, is not the case with Ruhmkorff's apparatus, only one terminal of which—viz., that which proceeds from the outer portion of the coil—is capable of giving sparks by itself; if an imperfect conductor, such as a piece of paper or wood, be introduced lengthwise between the points, the sparks will then pass from one to another over its surface.

Experiment 5. If the flame of a spirit lamp be placed between the points, the sparks will then pass from one to another over its surface.

Experiment 5. If the flame of a spirit lamp be placed between the points of a discharger, the sparks will pass several inches through it, and with one of my powerful machines, sparks of a foot or more in length may be obtained in this way through the flames of several spirit lamps placed side by side.

Experiment 6. When sparks are passing between the terminals at a distance of about half an inch, combustible substances, such as paper, cotton, shavings, gunpowder, gun-cotton, inflammable liquids, &c., may be kindled by being introduced between the points.

Experiment 7. If we place the points of the universal discharger

three or four inches apart, on the surface of a piece of soft wood or cork, which has been previously moistened with a little sulphuric or nitric acid, the substance is ignited and kindled into flame about the points of the discharger; sparks at the same time pass along the surface of the wood or cork from point to point, and the flames and combustion quickly extend in a direction towards each other, until, in the course of a minute or two, they meet, and produce an ignited line of connection between the points.

Experiment 8. If metallic filings be fastened, by varnish, upon the surface of a plate of glass, sparks ten or twelve inches in length may be passed over them, the colors of which will vary according to the metal used.

Experiment 9. If the two wires leading from the terminals of the coil to the universal discharger be connected respectively, one with the outer, and the other with the inner coating of the insulated Leyden jar, the sparks between the points of the discharger will be much shorter, but they will be very much louder and brighter, assuming quite the character of the Leyden discharge.

Experiment 10. If a portion of gunpowder be now placed upon the table of the universal discharger, it will not be inflamed by the spark of the jar, but will be dispersed, as in ordinary frictional electricity; though I have discovered that if one of the wires be detached from the universal discharger, and a piece of wet string be made the medium of communication, the gunpowder will then be inflamed. In this experiment care must be taken that the wet string be inserted in the portion of the circuit between the discharger and the jar, and not between the jar and the coil.

Experiment 11. The length of the spark and its strength when the Leyden jar is used, will depend upon the size of the jar. The smaller the jar the longer the spark, but larger jars give shorter and louder sparks. With a large jar, containing more than three square feet of surface, a roar of discharges may often be procured 0.4 of an inch in length.

Experiment 12. If these discharges be made to pass through an iron chain of ten or twelve feet in length, suspended in a festoon, sparks accompanied with brilliant scintillations will occur at every link.

Experiment 13. If the discharges of the jar be made to pass through a large lump of loaf sugar, a crystal of alum, muriate of ammonia, bichromate of potash, prussiate of potash, sulphate of copper, &c., these crystals will be splendidly illuminated.

Experiment 14. If a Leyden jar, coated with detached diamond-shape spots of tin-foil, be insulated and connected with the terminals of the coil, it will be brilliantly illuminated during the whole time that the machine is in action—much more so than with a common electrical machine.

Experiment 15. If a plate glass be coated on one side with tin-foil, leaving an uncoated margin of glass, and on the other side be only partially coated, the tin-foil being laid on in the form of a star or

other device, this will be surrounded with a beautiful light fringe of electric light when connected with the machine, and, at the same time, a most powerful odor of ozone will be given off.

Experiment 16. Coat a large glass flower vase, from eight to ten inches high, internally with tin-foil to within an inch or two of the top, and place it on an air-pump. Cover it with a receiver having a sliding wire passing air-tight through the top of it; let the wire be depressed so as to touch the tin-foil coating of the glass. Connect the outer end of the wire with one terminal of the coil, and the pump-plate with the other terminal. When the machine is in action a splendid cascade of electric light will be seen flowing over the edge of the glass, and apparently falling on the pump-plate, when the upper wire is made positive, but when the current is reversed the electrical appearances undergo a marked change, and the current appears to flow upwards instead of downwards.

Experiment 17. If a tall receiver, from two to three feet in height, having a cap and sliding wire on the top, be placed on the air-pump, and the air exhausted, a splendid aurora borealis, fills the tube with coruscations, and if the machine be sufficiently powerful a spark in the form of a crimson ribbon may be obtained, from two to three feet in length. The spark of this kind with Ruhmkorff's apparatus, however, rarely exceeds a few inches.

Experiment 18. If this experiment be modified by partially filling the receiver with hydrogen gas and exhausting again, the electrical appearances are very beautiful, the light in the interior appearing intersected by dark bands, which arrange themselves in a symmetrical form.

Previously to some of my recent investigations the induced current was considered to be incapable of heating a metallic wire, unless passing between its extremity and another conductor; but I have lately shown that when the current is modified by the action of the Leyden jar, it develops thermal effects in metallic wires of a novel and interesting character, as will be seen by the following original experiments.

Experiment 19. Place in the circuit between the two terminals of the induction coil, Harris's thermo-electrometer, either in its original form or in the form in which I have modified it, for voltaic purposes. Connect also in the circuit Lane's discharging electrometer, for the purpose of modifying the length of the spark. Pass a current thus through the electrometer and no effect will be observed.

Experiment 20. Now insulate a Leyden jar, and connect its outer coating by means of a wire, with one of the terminals of the coil and its inner coating with the other terminal, all the other connections of the former experiment remaining as they were. On adjusting the points of the discharger, so that the sparks may pass, the wire will now be found to be heated, the amount of heat produced being pretty nearly in the simple proportion of the coated surface of the jar. This law appears to be quite new.

Experiment 21. The apparatus being arranged as in the previous

experiment, place a little gunpowder between the points of the discharger and it will not be inflamed, but simply dispersed, though the wire in the electrometer will be heated. Now insert a wet string between the discharger and the electrometer, and on passing the discharges the gunpowder will be inflamed, but the electrometer will not be heated.

Experiment 22. If instead of the thermo-electrometer a piece of fine iron wire, about two or three inches long, be placed in the circuit, it will be instantly fused when the discharges pass.

The induced current thus appears to be considerably modified in its character by the intervention of the Leyden jar, and the nature of this change may be beautifully shown by the following arrangement:

Experiment 23. Connect a thermo-electrometer and a Lane's discharging electrometer with the terminals of the coil. Upon an adjoining table, place a disc of wood covered with tin-foil, exposing a flat surface of five or six square feet, and connect it also with one terminal of the coil. Take a second similar disc of wood coated with tin-foil and suspend it over the first one, by means of a string, passing over pulleys, in a frame so constructed as to admit of the second discs being raised to the height of five or six feet above the lower one. Connect this disc by means of flexible wire with the other terminal. By this arrangement the two terminals have virtually their conducting surfaces increased, and the sparks consequently are much brighter, though the thermo-electrometer is unaffected. If now the upper coated disc be gradually lowered, the sparks rapidly increase in power, and when they are within three or four inches of each other, the sparks assume the character of the discharges of a Leyden jar and the thermometer begins to be affected. As the discs are gradually lowered still further, their faces being kept parallel to each other, the sparks become still louder and the thermometer rises 15 to 20 degrees, the discs thus acting as coatings to a charged plate of air. On removing the upper plate these effects subside and the spark re-assumes its original character. The difference in the thermal properties of the induced current, with or without the intervention of the Leyden jar, is shown by the following experiment:—

Experiment 24. Insulate a Leyden jar, and apply to it a Lane's discharger; connect between the discharger and the outside of the jar a thermo-electrometer, so that the discharge of the jar may pass through it. Insert two other thermo-electrometers respectively in the portions of the circuit which are to conduct the electricity from the terminals of the coil to the inner and outer coatings of the jar, so that the course of the electricity may be assumed to be as follows, viz.—from one terminal of the coil through one thermo-electrometer to the inside of the jar, thence through the discharger and thermo-electrometer connected with it to the outside of the jar, and lastly, from the outside of the jar through the remaining thermo-electrometer to the other terminal of the coil. Now set the machine in action, and the electrometer which connects the two coatings of the jar will show a rise of temperature, whilst the two electrometers which serve to con-

duct the current between the coil and jar are not in the slightest degree affected. Since the Leyden jar, under the influence of the coil, is capable of producing the same class of effects as when charged by the common electrical machine, we have thus a link of connection opened which may one day serve to establish some relation between equivalents of frictional and voltaic electricity. At present I have only attempted to compare the effects of the coil with the electrical machine by estimating the amount of glass surface necessary to be rubbed to produce effects equal to the induction coil. In order to do this, I have been obliged to contrive an apparatus, which I have the honor to exhibit to the Society, which will register the number of discharges of the jar per second, the value of such unit being determined by other means. In one of Professor Grove's papers in the *Philosophical Magazine*, he proposed to count the discharges by causing a piece of paper to pass with a given velocity per second between the discharging points, and the number of perforations thus made per second might be registered. As it was a matter of some difficulty with me to construct an apparatus in which a uniform and definite velocity in a paper disc should be maintained, I had recourse to another expedient, which I find to be most simple and efficacious. The instrument is thus constructed:—two circular discs of wood, about five inches diameter, are screwed together upon one common axis, so as to embrace a circular disc of paper, about 12 inches diameter, between them. One of the discs of wood is made moveable, for the purpose of changing the discs of paper when required. This is mounted with a handle, so as to rotate vertically like the plate of an electrical machine. Upon the same stand are placed two vertical brass pillars, having steel bearings on the top, which serve to support the knife edges of the axis of a half seconds pendulum, so situated on the stand as to hang about two inches from the edge of the paper disc, and make its oscillations in the same plane. Through a wooden head piece, fastened on the axis of the pendulum, pass two copper wires, enclosed in gutta percha. These wires are about two inches apart, and they stand out horizontally about eight inches in length, like two arms stretching towards the paper disc, one on each side of the top of its periphery. Here the points are turned towards each other, and brought within one-eighth of an inch of the paper on each side, so that the naked ends of the wires are about a quarter of an inch apart, having the paper disc between them, and are so adjusted as to stand about half way between the edge of the paper and the wooden plates which hold it. The other two ends of the wires pass about one inch through the head piece, and are there bent into two hooks, the gutta percha coating being removed from them. Upon these hooks hang the ends of two light and flexible wire-cord conductors, connected respectively with the inner and outer coatings of the jar, which latter is also in connection with the coil.

Experiment 25. The apparatus being thus arranged, if the pendulum be made to swing, the two arms will describe an arc over the periphery of the paper and if the coil be set in action, discharges will pass between the points, marking the arc with perforations. If now

the disc be turned on its axis, a series of curves will be described by the perforations from the oscillating points. The number of perforations in each curve may be counted, and if reckoned from the top of one curve to the top of another, we get the number per second, or if from the top of one curve to the bottom of the same one, the number in a half second. The rapidity of the discharges will depend upon the nature of the interrupting spring employed in the coil, and as many as 100 to 200 per second may be obtained.

In order to ascertain the value of each unit of discharge, the following experiments must be made without the before-mentioned apparatus.

Experiment 26. Connect the jar used in the preceding experiment with the coil, and place in its circuit a thermo-electrometer as described in Experiment 20, but instead of allowing the spring to vibrate by itself break contact with it at separate intervals with the hand, so as to get single discharges from the jar; note how high the fluid rises in the electrometer with each discharge. Next apply the same jar to the conductor of an electrical machine, the thermo-electrometer being connected with it in the same way, and then ascertain how many turns of the electrical machine are required to produce a charge in the jar capable of raising the fluid in the electrometer to the same height; the surface rubbed may then be easily calculated by multiplying the circumference of the cylinder by the width of the rubber and number of turns, which will give the equivalent in glass surface for a single unit of discharge from the coil. And this multiplied by the number of discharges per second obtained from the coil, will give the number of square feet necessary to be rubbed per second to produce an equal effect. In estimating the surface of a plate machine, the surface rubbed at a single revolution must also be multiplied by the number of rubbers.

I trust that the few experiments and illustrations which I have now had the honor of laying before the Society, are sufficient to show the character of the new instrument, but as to the extent of its capabilities, or the degree of extension of which its principles may be susceptible, time and experiment alone must decide the point. I cannot conclude, however, without alluding to some practical purposes to which the machine appears eminently adapted. The first and most important is its use as an electro-motive agent for the electric telegraph. The secondary current produced by it is precisely similar to that developed by the magneto-electric apparatus, in which magnetism alone is the exciting agent, but with this difference, viz., that its intensity, or power of overcoming resistance is infinitely greater, and I should consider a modification of it as the most efficient mode of exciting a current for a long circuit, such for instance, as the one between England and America, that can possibly be employed. The apparatus may be made compact and powerful, and the expense of working not worth consideration, since it might even be arranged so as to work with a battery that might be carried in the waistcoat pocket.

The next important use to which it may be applied is for firing charges of gunpowder at a distance; the peculiar fire-like character of the spark occurring between interruptions in the circuit seem to adapt it especially for this purpose, and the great intensity of the current gives it a power of producing these results through a length of wire, which would require an enormous amount of voltaic battery power to traverse it, so as to produce the same effect. Extensive experiments of this kind have already been performed in France with great success.

A third application of it would be for the purpose of electro-metalurgy, but the secondary coil of the instrument would require some considerable modification to adapt it efficiently to this purpose.

I trust that I have now said enough to commend the instrument and the phenomena which it produces to the attention of the scientific world, and I feel quite satisfied that the principles will one day be developed to an extent, of which at present we have no conception.

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PUBLIC HEALTH.

CONSIDERATIONS RESPECTING *the* OPERATION of MALARIA on the HUMAN BODY.

BY C. HANDFIELD JONES, M. D, F. R. S.

THE almost universal prevalence of malaria throughout the temperate and tropical regions of the globe, is a circumstance, it appears to me, of the most striking import. Go almost where we will, to the fairest and brightest spots on the surface of the earth, the same evidence of the pronounced doom ever appears. Still it is found to be the case, "that cursed is the ground for thy sake." What is the country in Europe that is exempt from this scourge? As far north as Sweden it prevails; and the farther we proceed south the more intense and deadly does the poisonous influence become. From all the shores of the Mediterranean, we have accounts of its existence; and in Sicily, one of its islands, a non-medical authority has specified particularly no less than eighty-two unhealthy situations. It was probably one of these that a British commanding officer three or four times attempted to occupy as a telegraph post, and did not desist, though warned by the natives of the consequences, until thirty men were successively destroyed by the unseen foe. Of the tertian fever of Minorca, Cleghorn has written; and of the extermination of their garrisons at St. Fiorenzo, in Corsica, French authorities can tell. In Algeria, the same malaria is met with, as abundantly witnessed by MM. Boudin, Maillot, and Laveran. To attempt any further detail of the places where malaria has been found by sad experience to prevail, would engage me in almost endless quotations and references, and is perfectly needless. We all know by even popular report of the fevers of the East and West Indies, the coast of Africa, North and

South America, Burmah and China ; and the dependence of these on malarious influence is as well ascertained as anything possibly can be. Now, one cannot but conclude from what has been stated, that the conditions which give rise to malaria must be exceedingly common ones ; and that it cannot be regarded as a miasm capable of being evolved only in a few special localities.

2. It is an established fact, that the presence of vegetable matter is by no means necessary for the production of malarious disease. Ample proof of this is cited by Dr. Watson in his "Lectures," and by Mr. Martin in his work on "Tropical Climates." The former adopts the plain conclusion, that, "for producing malaria, it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried ; and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent, probably because more plentiful) is the poison that is evolved." And again : "There is reason to believe that the flooding of a porous earthy surface with water, and the subsequent drying of that surface under a certain degree of heat, constitute the sole or main conditions of the generation of the poison." Mr. Martin is inclined to attribute the malaria-generating property of soils to their containing a quantity of iron ore. He has obtained evidence showing that, at all the most unhealthy stations on the west coast of Africa, the earth and the mud of the rivers was invariably ferruginous. Similarly in the slave States of North America, it is mentioned by Mr. Featherstonehaugh that malaria is found in continual connection with the ferruginous soil of those countries, with the red sandstone rock, and the red mud of the Arkansas. Now, must we not really feel that these statements respecting the non-essentiality of vegetable decay to the production of malaria are of the very widest and gravest import ? If it be indeed so that the mere rapid evaporation of water from a soil that has been saturated is adequate to generate a deadly poison, where shall we undertake to say that this may not happen ? Even should it be necessary that the soil should be ferruginous, how very common still would be the conditions capable of producing malaria ! Some fragments of medical experience seem to point, however, nearly in the same direction. Dr. A. T. Thompson recommends that the floor of a sick room should be swept—never washed. "An invalid may as well sleep in a swamp as in a room the floor of which is frequently washed." Dr. Alison has observed the frequent occurrence of croup on Saturday night, the only day in the week on which it is customary for the lower orders in Edinburgh to wash their houses. These and like experiences may be worth bearing in mind in attempting to form to ourselves a theory of malaria. The production of dysentery by malaria, which no one doubts, and of croup by the vapour from a drying surface, are not things very dissimilar.

3. There seems good reason to believe that a very brief exposure to malarious emanations is sufficient to affect the system, at least when the poisonous influence is very intense. Dr. McCulloch states that he has frequently observed that fever may be induced within half an hour

after exposure to malaria, and that a single inspiration, or the space of a very few seconds, is amply sufficient for the purpose. His experience, he affirms, is corroborated by that of many practised and acute French and Italian physicians. They record instances of laborers dying instantaneously from merely sitting or lying down on the ground, and of others who, from looking into a ditch or drain, have been struck dead by the poison which, of course in a minor degree, would have merely produced a fever. Lind also describes, as well as many others an instant seizure with nausea and delirium.

4. It is certain that a person may be exposed to malarious emanations, and yet suffer none of the usual effects until some considerable time, weeks or months after, when some special exciting cause comes into operation, as the setting in of an east wind, exposure to cold and wet, great fatigue. Nay, the malaria may fail to affect the system at all in the absence of these exciting causes, when otherwise it is morally certain that it would have. I must quote a very interesting fact in proof of this from Dr. Copland :—"Between twenty and thirty persons were exposed all night without cover to the air of one of the most fatal sources of miasmata furnished by a warm climate, during the unhealthy season, but were soon afterwards removed to sea, far from any further exposure to this specific cause. They continued well for six or seven days, when about half their number experienced great fatigue. All these were nearly simultaneously—on the following day—seized with remittent fever; while those who had not been subjected to the consecutive cause, with the exception of two, who were not attacked till several days subsequently, entirely escaped, although all had been equally exposed to the specific cause of that form of fever." I have now a young female under my care with tertian ague, who lived with her family at Wisbeach, in Cambridgeshire, till she was about fourteen. Since then she has been living in London, but has not had any ague till the present attack, which is five years from the time of her change of residence. It seems reasonable to suppose that the debilitating influences of a town life have brought the original infection into play by impairing the vital resistance. Her father had ague when a lad, was then free for a number of years, and healthy. After he came to London, he continued free for four years, but was then attacked by quotidian.

5. When very intense, the action of malaria on the system is most deadly, prostrating organic life, just as in the worst forms of malignant cholera. This is what has been termed *febris algida*, the patients never attaining to reaction after the deadly stroke, but dying in the cold stage. In some of these cases, the peril is from a special affection of the heart; but, in the majority, the prostration involves the whole nervous system.

There are numerous forms of disease which are capable of being produced by malaria. Omitting any further mention of fevers, graduating according to the intensity of the cause and the degree of predisposition, from intermittents, into continued, we may notice dysentery and neuralgia as being as much modes of manifestation of malarious

action as fevers. The almost constant supervention of dysentery on remittent fever is so well known that to adduce evidence would be merely tedious. Dr. McCulloch alludes to the not uncommon occurrence of a fit of neuralgia and a paroxysm of intermittent happening on alternate days, reminding us of the double tertian. The case has been mentioned to me by one of the members of this society, of an officer, aged 23, who having returned from Africa in ill health, had an attack of ague on Friday and on the following Sunday. On the next Tuesday, the day for the paroxysm, he had instead severe neuralgia of both forearms, continuing from the evening to the next morning. After this, the ague paroxysms recurred again. Dr. Copland, whose world-wide experience makes his testimony especially valuable, recognises malaria as one of the most efficient causes of neuralgia. The same authority sustains also the opinion, that rheumatism may result from malarious influence. I believe I have witnessed the same occurrence myself. That *pestilential cholera* is a malarious disease, closely allied to the pernicious fevers, I am fully convinced, following the opinions of Bell, Dixon, Billing, and others. While cholera was epidemic in London in 1854, I had a severe attack of an aguish character; and the peculiar depression I then experienced continued for some weeks, until the disease passed away. I could only shake it off by going to Hampstead, where I immediately regained a degree of vigour, but lost it again as soon as I returned to Paddington. From that period to the present, I may say that aguish disorder and neuralgic affections have been prevalent to a degree that I never knew before. *Apoplectic* and *paralytic* affections are known to occur as the results of the poisonous influence we are considering; and I believe these morbid phenomena are of much significance in attempting to form an opinion respecting the general action of malaria. While I do not doubt that local determination of blood may have some effect in causing the impairment of the function of the nervous structure, yet I think there is much ground for believing that both nerves and nervous centres suffer primarily by the operation of malaria, sometimes in a way of irritation, but chiefly by enfeebling and debility. Slighter cases show this even more certainly than severer ones. In the *Lancet*, for October 20th, I have recorded two cases which appeared to be ordinary neuralgic affections of the upper extremities, peculiar only in this, that the motor nerves of the muscles were involved to a considerable extent, and not only the sensory. Here there could be no question of any congestion of the encephalon. A naval surgeon, a robust strong man, engaged at the Crimea, and especially in the Kertch expedition, got diarrhoea, and, soon after, paralysis of one arm came on, which lasted about a day, and then disappeared. An assistant-surgeon, who had been at Balaklava since the commencement of the war, was seized there with fever, and remained there till convalescent. He was invalided to return home, and on reaching Constantinople went on shore to make a few purchases. He over-fatigued himself, and on his return on board was seized with paralysis of all his extremities, the intellect remaining perfectly clear. He arrived in England in this perfectly helpless state,

unable to feed himself, or do anything for himself. In about two or three months he recovered perfectly. The cases of two members of our profession have been mentioned to me, who, after suffering remittent fever in the Crimea, became to a great degree idiotic. One of them imagined he had two bodies, and if any one sat near him, was fidgeted lest his second body should not have room enough. The other thought he had two heads. Another gentleman, who suffered also from fever in the Crimea, and was very much prostrated both bodily and mentally when he returned home invalided, regained his bodily health very well after for some months, but was far from having the same mental energy that he used; he could not apply himself with attention to anything, and could not use the same muscular exertion as before. In these cases, one cannot but recognise a special state of cerebral debility, or impairment of its functional power, quite apart from all suspicion of congestion. I must adduce the following instances from Dr. McCulloch, on account of the great importance of the point. In a situation exposed to malaria, and never free from its diseases, while the other members of a large family had the intermittent under different but ordinary forms, two were attacked with paralytic affections suddenly; the one in the leg and thigh, the other in the arm. I viewed these palsies as modes of the apoplexy of intermittent; and the judgment was confirmed by the almost spontaneous disappearance of the disease in both, and the immediate establishment of the regular quotidian. The apoplectic state is not limited either to the first attack or to the acuter forms of these fevers, although in Italy that appears to be the most common mode of its occurrence; as I have seen in this country, a perfect apoplexy, to the eye, in all its characters, sufficient to deceive both the attending practitioner and friends, lasting for eight hours, and occurring in a chronic tertian of many years standing, as a substitute for the cold fit. A case has been mentioned to me in which a person in the first paroxysm of ague had an attack of convulsions in the place of the cold stage; in the next, the cold stage occurred as normally.

To be continued.

UNADULTERATED BREAD.

WE have received from Mr. E. Stevens, of 5, 6, and 7, Cambridge Road, several loaves of white and brown bread, with a pamphlet, in which he informs us that his white bread is made wholly from the best wheaten-flour; and we fully credit his statement, for we never tasted in this country bread so completely realising all that can be desired in that important commodity.

In this pamphlet, Mr. Stevens informs the public that unadulterated wheaten bread is never white, and that it can only be rendered white by means of such adjuncts as alum, sulphate of copper, &c.

We have eaten this bread at various ages, and found it excellent at the end of a week.

All who have tasted bread in Paris, and who have been disgusted

with the English bread on their return, will know how to appreciate the bread under notice.

We recommend this bread to the attention of the medical profession, as it is frequently a matter of great importance, and always a matter of great difficulty to find bread fit for invalids.

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS.

On the EFFECTS of SWEET SPIRIT of NITRE when INHALED.

BY MR. BROWN.

IN the *Edinburgh Medical and Surgical Journal*, vol. xxxv., p. 452, a case of death from breathing the vapor of sweet nitre is quoted from the *Midland Medical and Surgical Reporter*, No. II., and which is also noticed by Professor Christison in his work on poisons. Of that case we only learn that the young woman "was found dead. She lay on her side, with her arms folded, the countenance and posture composed, and the whole appearance that of one in a deep sleep." A jar of sweet nitre kept in her bedroom had broken, and filled the apartment with the vapour. In this case the death of the girl is ascribed to the nitrous ether, but apparently rather from producing asphyxia by the mere negation of atmospheric air, than from any deadly power in the nitrous ether itself; for it is added, "the woman seems to have died as in cases of poisoning with carbonic acid, from slow obstruction of the breathing—from gradual asphyxia." This I believe is the only case upon record. Fortunately the observations now to be brought before you do not reach so far as death, but they reach so far as to make it certain that the vapor of sweet spirit of nitre exerts a power belonging to itself, and of sufficient energy to destroy vitality, when taken into the system through the lungs.

Before entering upon the symptoms to be detailed, it may be mentioned that they manifested themselves in different persons of various ages and habits, and at different times and places, and that their manifestation was most marked during the preparation of pure hyponitrous ether, or after long exposure in a large room to the uncondensed gases evolved in the preparation of the sweet spirit of nitre of the *Edinburgh Pharmacopoeia*; but they have also displayed themselves in a very decided way indeed, from merely breathing the vapor thoughtlessly, while measuring sweet nitre from one jar into another. One of the individuals having been under the influence of the vapors of nitrous ether more frequently than any of the others, I will state first the particulars of his experience. The first observed effect is a bluish-purple color, which the lips take on. This occurs even when the vapor is very dilute, and it may exist quite unconsciously, and without any bad effect whatever. After being exposed for one or two hours to a rather diluted vapor, or after exposure for only a few seconds to a very partially diluted vapor—after it may be only three or four natural inspirations—the lips assume a deeper purple tint, the eyes a dull appear-

ance, and the whole countenance has a ghastly, dirty-looking pallor about it, as if smoked all over. The hands, more especially beneath the nails, partake of the purple color. The first felt effect is the loss of muscular power, which goes on until complete prostration ensues. At this period the lips are still purple, the face ghastly, and with the hands of a leaden blue or purple colour. The pulse is weak and quick, the breathing slow and regular, and the extremities cold. The mind is calm, no ways distressed, and as clear and able as at other times; and only a very slight degree of swimming in the head exists, scarcely perceptible even when at the worst. No pain is felt anywhere, and there is no consciousness of muscular debility until an effort is made. Upon walking a short distance, more or less, as the case may be, the breathing becomes very hurried, and the pulse very quick; and if the attempt is persevered in, the heart makes itself felt by its rapid, tremulous, tumultuous action, the breathing becomes quick, difficult, and painful, and a most distressing sense of painful constriction is felt underneath the sternum; the knees shake, and refusing any longer to remain rigid, give way. The mind is as before. Perfect rest, heat, and a warm atmosphere, with brandy and a little ammonia, gradually restore the powers of the muscular system. After five or six hours have elapsed, a dull annoying pain is felt in the head, more acutely along the course of the temporal arteries, which continues for five or six hours. During the night, while in bed, the skin is dry, but by no means hot; yet while there is no restlessness, it is a relief, and pleasant to the feet, to hunt out the coolest spot they can find. Sound sleep follows at, or perhaps a little after the usual hour; and on getting up next morning, all bad symptoms have vanished, save it may be a little debility. The diuretic action of sweet spirit of nitre was not apparent until twelve or fourteen hours after exposure to the vapor.

The second case—a gentleman who also has experienced the effects of the vapor of nitrous ether oftener than once—will be best given in his own words, which I will now read. He says:—

“All the sweet nitre I had taken must have been by inhalation, while engaged in making it. When it began to affect me, it was between seven and eight P. M. I at that time felt headache and slight confusion of ideas, and remember noticing that my nails were colored blue, and also that Mr. G's lips were the same. About half-past eight I proceeded home, at first feeling neither increase nor diminution of the symptoms; but by the time I had reached half a mile, I felt very weak and unable to walk, which at last compelled me to sit down for a little on a stair. I recovered considerably by this, and was able to proceed; but all this time I experienced some mental alarm about my condition, so much so that I went out of my way to call at my brother's, where I got a little brandy. Shortly after, I felt quite strong in the limbs, but the headache was still present; indeed, I may mention that it did not go entirely away till the middle of next day. By the time I got home I again felt the weakness of limbs coming on, but fortunately I was soon in the recumbent posture—so soon that I alarmed my friends by laying myself down the moment I came in. I then asked for more

brandy, and also hot water for my feet. I then sat up for nearly an hour, when an exceeding drowsiness came on, quite like what one feels after long hard work; but this perhaps was partly the effect of the brandy. On getting to bed, I slept, and that soundly for nine hours and a half, and on awaking, all the symptoms were gone, except the slight headache."

In this second case, there was giddiness, slight confusion of ideas, continuing all the evening, with some little anxiety; the depression did not go off altogether until twenty-four hours had elapsed; the headache came on about half an hour after exposure, and continued for seventeen hours; and the diuretic effects were produced within an hour or two after the inhalation of the vapor: these constitute the differences between the first and second instance. The only additional symptom which was noticed in the third case was vomiting, which occurred at an early stage, and was not either severe or persistent.

Such, then, are the results of inhaling the vapor of nitrous ether. Doubtless, a very few full inspirations would terminate in death,—brought on, not merely by preventing the access of atmospheric air, but by its own inherent energy.

Among the symptoms, two may be noticed: first, the marked effect produced upon the blood, evidenced by the leaden-purple color of the lips, mouth, hands, &c.; and second, the extreme muscular debility. Chloroform has been employed to subdue the furious mania of delirium tremens; might not sweet spirit of nitre; or the cautious use of hypnitrous ether, be in such and such like cases a better agent? The muscular debility, in my own experience, comes on very rapidly, unaccompanied by the slightest effect on the brain, and it endures for hours. Such was not, however, the experience of others; and the headache which invariably follows its inhalation, either immediately or soon after, may be a contra-indication to its use.

And now a question arises,—Can we account for such symptoms? To the purely medical part of the answer which this question demands, it is not my intention to offer a reply; the *modus operandi* must be left to others better fitted and more entitled to enter upon it. But there lies wrapt up in it, besides its physiological question, another,—namely, its chemical; which, in truth, is not one question, but many questions; which asks,—Do the effects produced by the inhalation of sweet nitre depend upon its chemical composition? Here a wide field opens up to us, upon which at present we can only cast a look; for the same question may, and, we think, will, be put to every substance made use of by inhalation. The experimenters of former days have left us records of the results arising from the inhalation of a few gases, simple and compound,—not very pure, we fear; but inhalation in our own day has taken a rapid stride forward, and experiments upon a number of substances have been made. The experiments of our own day, however, have been made in order to arrive at a practical conclusion—the production of anæsthesia—and not in search of a cause or a philosophical explanation of phenomena. The effects produced have indeed, we believe, been ascribed to carbo-hydrogens, and apparently

with some truth; but, most unquestionably, the simple and compound bodies which enter into union with the carbo-hydrogens do also enter into the effects produced by them when inhaled. We find that experiment bears this out: for instance, we may see in Professor Simpson's works, edited by Drs. Priestly and Storrer, that the effects of ether or the oxyde of ethyle (C^4H^6O) are not the same as the effects produced by the nitrate of oxyde of ethyle ($C^4H^6O+NO^3$); neither do these give us the effects which are produced by the hyponitrite ($C^4H^6O+NO^2$). I am not aware whether the results of the inhalation of olefiant gas have been made known or not, but it would serve very much towards a right understanding of the action of all the ethyle compounds if they were. Sir H. Davy has left us his experiments on the inhalation of the carbo-hydrogen got by the decomposition of water by charcoal, which he terms hydro-carbonate,—a mixture, perhaps, but likely C^2H^2 . We find in all these a family likeness, but yet a very marked difference; pointing, of course, to a difference in the producing cause, which may to some extent be due to the idiosyncrasy of individuals, but not wholly owing to it, inasmuch as the same person is differently affected by the different substances employed. We can scarcely doubt that these differences have their source in the difference of chemical composition. Thus—

Olefiant gas	.	.	.	C^4H^4
Ether	.	.	.	C^4H^6O
Alcohol	.	.	.	C^4H^8O+HO
Hyponitrous ether	.	.	.	$C^4H^6O+NO^3$
Nitric ether	.	.	.	$C^4H^6O+NO^5$

are kindred both in their chemical composition and in their action upon the living system. Formyle (C^2H) has not yet been inhaled and its action noted, but suppose it were, would the effects be the same and no other than those of chloroform, the perchloride of formyle (C^2HCl^3); would the three atoms of chlorine make no difference? It would be absurd to assert yea or nay, regarding a matter which experiment alone can decide, but all analogy leads us to believe that the chlorine does exert an influence, and that the results of the inhalation of chloroform or any other substance, are either the results of the combined forces of the elements within it, or the sum of their differences. Could we establish such a law, could we discover and control the force, we would then have one element of a true science of medicine, namely a scientific *materia medica* and therapeutic. But with regard to a right answer to all such questions, our present experiments are yet in chaos. The time must, however, soon arrive, when such investigations will be entered upon, opening up a new branch of science, and just as chemists are now able to say how far an atom of carbon or hydrogen, &c., will affect the boiling point of the compound into which it enters, so will they be able to say what effect it will produce upon the living system of man. Nor do we think this when done will exhaust it, for not only do material substances act with energy upon man's material frame, they act also as we all know, upon his mind

and emotions. Is it too much to expect that we may also, by means of inhalation, be able to experiment upon and so discover more about the mind and emotions than we do at present? Man has laid the strong hand of his intellect upon the lightning and held it fast while he questioned and compelled an answer, and subdued it to his will. By his subtlety he has untwined the sunbeam and made it tell him of its curious secrets. He has grappled with the strong and wooed the beautiful, and conquered both. Must mind escape him? Perhaps it must; but it can never be folly to experiment, observe, and tell the truth. Failure in experiment is but finding another truth than that we sought for, and while its lesson is often bitter, it nevertheless teaches confidence in self and humility and forbearance towards others; while not seldom it is that the truth found is of more value than that we asked after. But we must not wander. It is indeed true that the great majority of simple substances as we find them cannot be inhaled; but in hydrogen, oxygen, and nitrogen, we have three gases which, one or other of them, combine either directly or indirectly with all the others, and bring them within our reach. Is it not in our power to experiment upon these, and by accurate observation and arrangement of the phenomena, secure data for deducing the effects due to other simple or compound bodies when experiments are made with their compounds of hydrogen, oxygen, and nitrogen? We cannot doubt it.

But before we can look intelligently at the effects produced by inhalation, much must be done. Even at the outset, experimenters will find themselves beset with a difficulty not easily got over. They require an arrangement of the phenomena presented to us by inhalation, *i.e.*, a classification involving a nomenclature. Anæsthesia is a very good name, so far as it goes; and anæsthetics very applicable to substances which bring on anæsthesia. But insensibility to pain is not the only product of their action. It is only one of a number. By what shall we designate the residue? In a scientific examination of the results of inhalation, anæsthesia is but one of many events, each of which demands careful attention, and of which many can and do happen without anæsthesia. Who will venture the dangerous deed of giving names to these?

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ON PEPSINE.

BY W. STEVENS SQUIRE, PH. D.

SOME time since a new medicine, under the name of pepsine, was introduced in Paris, and was extensively used there by Dr. Corvisart and others, apparently with considerable success in dyspepsia, apespsia, and certain cases of consumption. This preparation consists principally of a substance well known to chemists and physiologists, the nature and composition of which, however, is but imperfectly made out. It is, in fact, to a principle termed pepsine that the gastric juice chiefly owes its property of converting meat and other matters

received into the stomach, into a form in which they can be readily assimilated.

By the term gastric juice is understood a fluid secreted by the stomach, and poured into it through a number of minute tubes, situated in the mucous membrane which forms the interior coat. These tubes are, during fasting, plugged up by a mass of epithelial cells; but when the mucous membrane is irritated by food or by certain other stimulants—pepper, for example—the secretion of gastric juice commences, the plugs of epithelium are expelled, and the juice distils into the cavity of the stomach.

Gastric juice contains about 97 per cent. water, 1.25 per cent. pepsine, and 1.75 per cent. salts, consisting of carbonate and phosphate of lime, chloride of sodium, minute traces of sulphates, and sometimes a trace of ammoniacal salts. The proportions, however, in which these inorganic constituents are present, vary considerably. Lastly, the gastric juice contains a free acid, which is one of the most important components.

As it would be impossible to obtain a sufficient quantity of gastric juice for medicinal purposes from living animals, either by fistulous openings or any other method, we seek to intercept the pepsine before it passes into the stomach, and to extract it from the little tubes in which it is secreted, or at all events along which it passes on its way. For this purpose the rennet-bags of sheep are the most convenient, on account of the large supply which is always at hand; and the following process has been proposed by M. Boudault:—The rennet-bags are opened and reversed, and washed under a thin stream of water, to free them from alimentary matters, &c. The mucous membrane is then carefully scraped off with a knife, the cells are bruised in a mortar, and digested for twelve hours in distilled water. The liquid is then filtered, and neutral acetate of lead is added, which precipitates peptate of lead. This precipitate is collected and decomposed by means of sulphuretted hydrogen, pepsine is thus liberated in solution, and is separated from the insoluble sulphide of lead by filtration. This liquid represents a neutral gastric juice. It is, however, necessary that it should be acid, and for this purpose lactic acid is added until the liquid exhibits the same degree of acidity as a specimen of gastric juice obtained from the stomach of a dog by means of a fistulous opening. The reason why lactic acid is selected for this purpose will be shown below. If the artificial gastric juice thus obtained, be evaporated to dryness at a gentle temperature (it should not exceed 100° Fah.), a gummy mass is obtained, which attracts moisture from the air, and is altogether a very unfit article either for sale or administration. In order to reduce it to powder, M. Boudault simply evaporates his artificial gastric juice to a syrupy consistence, and to this he adds dried starch in such proportion that one gramme (15 grains) shall be in a position to digest four grammes of dry fibrine, when the two are submitted together in the presence of water to the temperature of the human body.

The substance thus produced is a fawn-colored powder, cohering

somewhat together, and possessing a peculiar taste and odor. It yields to water the lactic acid and the pepsine, producing a solution of a yellowish tint, with the color, odor, and taste of gastric juice.

A solution of pepsine—that is to say, the neutral artificial gastric juice—presents the following reactions;—Salts of lead and mercury give rise to precipitates, which, when decomposed by sulphuretted hydrogen, reproduce the pepsine with its physiological properties. Tannin likewise precipitates pepsine, but the precipitate has no power whatever of digesting fibrine. Rectified spirit precipitates the pepsine from its solution. This precipitate is soluble in water. Absolute alcohol dehydrates pepsine, and destroys, or at all events greatly diminishes its digestive power. One property, however, which pepsine possesses, which is very characteristic, and which is a fruitful source of failure in its preparation, is, that at the temperature of about 120° Fah., its solution becomes slightly turbid, and loses entirely its digestive properties.

There has been considerable difference of opinion with regard to the nature of the acid in the natural gastric juice; some chemists assert, that the acidity is mainly due to hydrochloric acid, some are advocates for phosphoric acid, others again for lactic acid, but it is universally admitted that the gastric juice must be acid, to produce its physiological effects, and that lactic acid is always present to a greater or less extent. Lehmann found, that when gastric juice is distilled, the first portions which passed over were free from hydrochloric acid, but as the liquid became more concentrated, hydrochloric acid was obtained; and this he ascribed to the property possessed by concentrated lactic acid of decomposing the compounds of the strongest acids, and in this case to the decomposition of the chloride of sodium.

In order to determine this question, M. Boudault made the following experiments. He first wished to ascertain whether the gastric juice, as it issued from the mucous membrane was acid or not. With this view, the rennet-bags were carefully cleaned and washed as long as any acid reaction was manifested to litmus paper; the cells were then bruised, and treated with cold distilled water; and in this way a perfectly neutral fluid was obtained. This fluid did *not* possess the power of digesting fibrine at the temperature of the body, but on the addition of a little lactic acid a complete digestion was obtained.

Now it is well known that pepsine has the property of converting glucose or grape sugar into lactic acid, and when we consider that the saliva can by its action convert amylaceous substances into glucose, and that pepsine determines the conversion of glucose into lactic acid, we have a very fair explanation of the acidity of the fluid found in the stomach.

Boudault found that a mixture of glucose, neutral pepsine, and fibrine, yielded complete digestion after twelve hours. The process of digestion did not, however, commence until the liquid had assumed a strongly acid reaction. I will not, however, say that lactic acid is the only acid which is capable of playing this part, on the contrary, hydrochloric and acetic acids yield perfect digestions, but not with the same rapidity as lactic acid.

From a number of digestive experiments carefully performed, Boudault found that the pepsine obtained from herbivorous animals is identical in its properties with that obtained from the carnivora, and may be substituted for it, so far at least as the digestive functions are concerned.

The employment of an artificial digestive agent is not new in medicine. The liquid obtained by treating rennet with water, acidified with some acid, has been given with success, but it has never been much used—probably in consequence of the disagreeable nature of the dose; but now that we have the means placed before us in an elegant form, it is no longer a matter of difficulty. The details and the cases where it may be employed will suggest themselves to every medical man. There are at present four preparations of pepsine in use—namely, No. 1, acidified with lactic acid; No. 2, which contains in addition 1 per cent. hydrochlorate of morphia; No. 3, containing $\frac{1}{4}$ per cent. strychnia; and No. 4, which contains no lactic acid, and is useful where there is an excess of acidity.

Other medicines, however, may be given in conjunction with pepsine, without in any way impairing its action; among these are subnitrate of bismuth, lactate of iron, carbonate of iron, iodide of iron, and reduced iron. Attempts have likewise been made to form a syrup of pepsine, as a more agreeable way of exhibiting this medicine; but it was found that the sugar was after some ten or twelve days converted into glucose, and finally into lactic acid. A solution of pepsine will keep any length of time, provided air be excluded; but, in the presence of air, decomposition soon sets in: and I have found that the best form to keep it in is that of a powder, as originally proposed by M. Boudault. It may be taken in this form very conveniently between two slices of bread, or in the first spoonful of soup, which, however, should not exceed the temperature of the body, the dose being about fifteen grains.

The gastric juice possesses all the characters of a diluted acid; it attacks iron filings, and decomposes the alkaline carbonates, and it appears to me that this fact would justify a much more extended use of the lactates than at present prevails, for such preparations as ferrum radactum, ferri carbonas saccharatum, mistura ferri, &c., must be resolved into lactates soon after they reach the stomach, and that, too, at the expense of one of the most important constituents of the gastric juice.

277, Oxford Street, 24th Feb., 1857.

[Mr. Squire has notified, in letters to the *Lancet* and *Medical Gazette*, the existence of a spurious preparation of pepsine, for distinguishing which he gives the following tests:—

True Pepsine.	Test.	False Pepsine.
Abundant precipitate (Peptate of lead)	Acetate of lead	Slight cloudiness.
Abundant precipitate (Tannate of pepsine)	Tannin	Ditto.
Precipitates the pepsine	Alcohol	No effect.

The solution of true pepsine is strongly acid to litmus, whilst the false is only so in a slight degree ; but, more than all, M. Boudault's preparation does what it professes to do ; fifteen grains digests its drachm of dried fibrine, while the spurious compound is entirely destitute of this property.—*Lancet*.

The most serious contamination of pepsine is that contained in No. 3, namely, strychnia ! For what purpose this is introduced we cannot imagine ; but the circumstance of keeping such company is likely to cause alarm, and thus to create a prejudice unfavourable to the general introduction of pepsine into dyspeptic society.

Pharmaceutical Journal, March, 1857.

On the BYTTERIA FEBRIFUGA, and its ACTIVE PRINCIPLE.

By M. GERARDIAS.

FOR a long time the inhabitants of the Antilles have used, to cure the intermittent fevers to which they are subject, a bitter wood, furnished by a tree which grows in Saint Martin, and which is known in that island by the common name of *bitter ash*.

M. Amic, physician in chief in the island of Martinique, wishing to ascertain the origin of this wood and the proximate principle to which it owes its properties, directed M. Gerardias, Pharmacien to the Fleet, to isolate this proximate principle.

M. Gerardias has obtained the bitter principle of the *bitter ash*, by exhausting the wood by decoction in water, treating the concentrated decoction with sub-acetate of lead, and evaporating the filtered liquid after having separated the excess of lead with sulphuric acid.

The substance which separated towards the close of the evaporation, under the form of crystalline pellicles, was dissolved in alcohol, decoloured by animal charcoal, and obtained perfectly white and crystallised.

The average yield was three grammes per kilogramme of wood.

Thus obtained in a state of purity, this substance presents the following characters ; it is white, crystalline, inodorous, and excessively bitter.

When slightly heated it melts into a colorless liquid, and on cooling becomes a transparent non-crystalline mass. If the heat is continued the liquid at last disappears, after diffusing white vapors. When the heat is greater and suddenly applied it gives a brown liquid, diffusing yellow vapors which are not ammoniacal, which condense in small empyreumatic and slightly acid drops, and it leaves a very abundant carbonaceous residue.

It is nearly insoluble in cold water ; nevertheless, this liquid retains sufficient to become extremely bitter ; it dissolves much better with the aid of heat.

Concentrated sulphuric, nitric and hydrochloric acids dissolve it cold and without coloration ; but the addition of water precipitates it immediately.

It dissolves readily without heat in alcohol, and this solution, which is neutral to litmus, is not precipitated by water. This alcoholic solution, diluted with water, is not precipitated with sub-acetate of lead, but is precipitated white by tannin and brown by ioduretted iodide of potassium.

As until then no one knew to what family the *bitter ash* belonged. MM. Chapuis, surgeon of the first class to the fleet, and Belanger, director of the *Jardin des Plantes* of Saint Pierre, were, at the desire of M. Amic, sent to Saint Martin by the Government of Martinique, to study this plant. These gentlemen determined that the *bitter ash* was a plant not hitherto described, and should be placed among amyridæ, of the family of terebinthaceæ. M. Belanger gave it the name of *bytteria febrifuga*. M. Gerardias consequently considers that the bitter principle which he has isolated is a new one, and has given it the name of *bytterine*.

M. Belanger has since found the *bytteria* in Martinique, where it is known by the name of male *simaruba*, whereas the officinal *simaruba* is known there as the female *simaruba*. When M. Gerardias, which to avail himself of this resource to prepare the bitter principle on a large scale, so as to try its power as a febrifuge, he found that this wood was of three qualities, whose differences he attributes to the conditions of vegetation, and especially to the time of the crop; the first quality, treated by the process indicated, readily yields its bitter principle; the second contains a larger proportion of mucous matters, which the sub-acetate of lead does not separate entirely, but which may be entirely precipitated by adding ammonia to the liquid, which is no longer rendered turbid by the subsalt of lead; the traces of metal which remain in the liquor, after the treatment by ammonia, are precipitated by carbonate of soda; then the bitter principle is separated perfectly by evaporation.

Finally, in the third quality of wood, the author has found very considerable quantities of fecula which, not being precipitated by the reagents already quoted, form a paste with the bitter principle and prevent it from being separated. The best process for the treatment of this quality of wood, appears to be the following.

The concentrated decoction is precipitated by an excess of milk of lime, a little sub-acetate of lead is added before filtering, so as to cause further precipitation, and to facilitate the deposition of that formed by the lime. The decanted liquid is then neutralised with nitric acid, then treated with sub-acetate of lead and ammonia. When all the mucous and extractive matters are eliminated, we have only to separate the last traces of lead by carbonate of soda, which precipitates the lime at the same time, and then to evaporate.

The discovery of a considerable quantity of starch in the wood of the *bytteria* is worthy of observation, it may become the object of study, and of important practical applications.

M. Gerardias gives the following description of the *bytteria*, from the samples which he procured in the neighborhood of Fort de France.

The *bytteria febrifuga* is a tree of 15 to 20 metres in height, whose diameter is about 60 centimetres; the bark is a brownish grey, slightly furrowed; the wood is light, white with yellow veins; the leaves are alternate, they are formed of from four to eight pairs of large leaflets, rather unequilateral, almost sessile, lanceolated, and of one terminal petiolated leaflet.

The flowers form a compound corymb, they are of a greenish white, hermaphrodite and complete, the calix has usually four free sepals, the corolla has four open, free petals, four stamens are inserted in a hypogynous disc. In a very few flowers we find five sepals, petals and stamens. The carpels are always two in these flowers having four petals, and in most of them with five, but in some few of the latter we find three. In all cases the carpels are attached to a very developed disc, the ovaries are perfectly free, the styles only are united from their origin, and are only divided towards their extremities, to form as many stigmas as there are ovaries. One carpel in general arrives at maturity, sometimes two may be found, but never three. The fruits are globular and dry. The seeds have no endosperm.

According to these characters, M. Gerardias thinks the *bytteria* should be reckoned as approaching the tube of *simarubas* of the family of *rutaceæ*; that if, notwithstanding the characters which distinguish it from them, we do not form a new group for this plant, and still unite it to the *simarubas*, it should at least form a distinct genus.

M. Gerardias says, that the *bytteria* cannot be the same tree as the *quassia* of Jamaica; this latter indeed (the *simaruba excelsa* of Candelolle, *pierasma* of Lindley, and Endlicher, *Jamaica quassia* of M. Guibourt), is always described as a plant with polygamous flowers and bivalve fruits, whereas in the *bytteria* the fruits are always hermaphrodite and the fruit indehiscent. The fruits show no trace of separation, and if we press them hard when ripe, they break irregularly, which never occurs with other *simarubas*.

Finding that the *bytteria* so nearly approached the *simarubas*, M. Gerardias wished to ascertain whether *bytterine* were not identical with the *quassit* removed from the *quassia amara* by Wiggers, and which appears to belong to all this tribe. He has found many points of resemblance between the characters given for *quassit*, and those which he found in *bytterine*.

To ascertain the identity of these two substances, he tried to obtain *quassit* by operating on the *quassia amara*. Having at first tried the same processes as with the *bytteria*, he obtained nothing, and he was obliged to have to use the process mentioned by Wiggers, and which requires large quantities of pure spirit and ether. With this process he obtained a small quantity of *quassit*, and he found that the yield of *quassia* was much less than that of the *bytteria*. The result of the comparative examination of these two bitter principles of the *quassia* and *bytteria* is that they are identical.

M. Gerardias thinks, that the *bytteria* being richer in *quassit* than

the quassia amara, and moreover being commoner and growing larger, it would be the best source from which to obtain this bitter principle, and according to the nature of the wood on which we have to operate, and using the processes which he has described, that we might obtain very large quantities of quassit or of bytterine, at such low prices that the kilogramme would not exceed 250 to 300 francs. Quassit, which has hitherto been but little known and unused, because of its small quantity and high price, might take its place in therapeutics, especially if it realises the hopes raised by M. Amic's very interesting results, as a means of cure for intermittent fevers. This would be a great acquisition.

Even supposing that these hopes are not all realised, we cannot imagine that a principle with such powerful properties should not be capable of practical usefulness.

Journal de Pharmacie et de Chimie, February, 1857.

PROCEEDINGS OF SOCIETIES.

OBSERVATIONS ON GLACIERS.*

BY PROFESSOR JOHN TYNDALL, F.R.S.

THE speaker commenced, by stating the circumstances in which the discourse of the evening originated. On the 6th of June, 1856, he had given a lecture upon slaty cleavage, at which Mr. Huxley was present. A short time afterwards his attention was drawn to the observations of Professor J. D. Forbes on the veined structure of glacial ice, by Mr. Huxley, who surmised that the theory of slate cleavage might also apply to the ice structure. On consulting the observations referred to, the probability of the surmise was immediately perceived, and an arrangement was made for a joint visit to the glaciers of Grindelwald, the Aar, and the Rhone. This arrangement was carried out, and the subject being a physical one, it fell to the lot of the speaker to follow it up after his return to England. By reading, reflection, and experiment, the boundaries of the investigation were extended, until finally it embraced the main divisions of the problem of glacier structure and motion. The results of the enquiry constituted the subject of the evening's discourse.

Certain phenomena connected with the motion of glaciers were first passed under review. The power of a glacier to accommodate itself to the sinuosities of its bed, the motion of the mass through a valley of variable width, and a number of similar facts had been adduced as evidence of the ductility of glaciers by M. Rendu and others. To these evidences Professor J. D. Forbes added, in 1842, the important observation that the centre of a glacier moved more quickly than its sides; and he was led finally to a definite expression of his views in the well-known Viscous Theory of glacial motion. Numerous appearances, indeed, seem to favor this idea of viscosity. The aspect of many gla-

* Royal Institution of Great Britain.

ciars, as a whole,—their power of closing up crevasses, and of reconstructing themselves after having been precipitated down glacial gorges,—the bending and contortions of the ice, the quicker movement of the centre where the ice is uninfluenced by the retardation of the banks, are all circumstances which have been urged with such constancy and ability, as to leave the viscous theory without any formidable competitor at present. To these may also be added, the support which the theory derived from its apparent competency to explain the laminar structure of the ice—a structure regarded by eminent authorities as a crucial test in favor of the viscous theory, and which was affirmed to be impossible of explanation on any other hypothesis.

Nevertheless, this theory is so directly opposed to our ordinary experience of the nature of ice, as to leave a lingering doubt of its truth upon the mind. To remove this doubt, it is urged, that the true nature of ice is to be inferred from experiments upon large masses, and that such experiments place the viscosity of ice in the position of a fact, rather than in that of a theory. It has never been imagined that the bendings and contortions, and other evidences of apparent viscosity exhibited by glaciers, could be made manifest on hand specimens of ice. But this was shown by the speaker to be experimentally impossible. Moulds of various forms were hollowed out in boxwood, and pieces of ice were placed in these moulds and subjected to pressure. In this way spheres of ice were flattened into cakes, and cakes formed into transparent lenses. A straight bar of ice, six inches long, was passed through a series of moulds augmenting in curvature, and was finally placed before the audience, bent into a semi-ring. A small block of ice was placed in a hemispherical cavity, and was pressed upon by a hemispherical protuberance not large enough to fill the cavity; the ice yielded and filled the space between both, thus forming itself into a transparent cup. In short, it was shown that every observation made upon glaciers, and adduced by writers on the subject in proof of the viscosity of ice, is capable of perfect imitation with hand specimens of the substance. These experiments then demonstrate a capacity on the part of small masses of ice, which has hitherto been denied to them. They prove, to all appearance, that the substance is even much more plastic than it was ever imagined to be by the founders of the viscous theory. But the real germ from which these results have sprung, was to be found in a lecture given at the Royal Institution, in June 1850, and reported in the *Athenæum* and *Literary Gazette* for that year. Mr. Faraday then showed, that when two pieces of ice, at a temperature of 32° Fahr., are placed in contact with each other, they freeze together, by the conversion of the film of moisture between them into ice. The case of a snowball was referred to as a familiar illustration of the principle. When the snow is below 32°, and therefore *dry*, it will not cohere, whereas, when it is in a thawing condition, it can be squeezed into a hard mass. During one of the hottest days of last July, when the thermometer was upwards of 100° Fahr. in the sun, and more than 80° in the shade, the speaker observed a number of blocks of ice, which had been placed loosely in a heap, frozen

together at their places of contact; and he afterwards caused them to freeze together under water as hot as the hand could bear. Facts like these suggested the thought, that if a piece of ice—a straight prism, for example—were placed in a bent mould and subjected to pressure it would break, but that the force would also bring its ruptured surfaces into contact, and thus the continuity of the mass might be re-established. Experiment, as we have seen, completely confirmed this surmise: the ice passed from a continuous straight bar to a continuous bent one, the transition being effected, not by a viscous movement of the particles, but *through fracture and regelation*.

All the phenomena of motion, on which the idea of viscosity has been based, are brought by such experiments as the above into harmony with the demonstrable properties of ice. In virtue of this property, the glacier accommodates itself to its bed while preserving its general continuity, crevasses are closed up, and the broken ice of a cascade, such as that of the Talèfre, or the Rhone, is recompacted to a solid continuous mass. But if the glacier accomplish its movement in virtue of the incessant fracture and regelation of its parts, such a process will be accompanied by a crackling noise, corresponding in intensity to the nature of the motion, and which would be absent if the motion were that of a viscous body. It is well known that such noises are heard, from the rudest crashing and quaking, up to the lowest decrepitation, and they thus receive a satisfactory explanation.*

* It is manifest that the continuity of the fractured ice cannot be completely and immediately restored after rupture. It is not the *same* surfaces that are regelated, and hence the coincidence of the surfaces cannot be perfect. They will enclose for a time *capillary fissures*, and thus the above theory accounts satisfactorily for the known structure of glacier ice. I have recently made the following experiments bearing upon this point. A piece of ordinary ice was taken, and a cavity hollowed in it was filled with a strong infusion of cochineal; the ice was perfectly impervious to the liquid, which remained in it for half an hour without penetrating it in the slightest degree. A piece of the same ice was subjected to a gradually increasing pressure. Flashes of light were seen to issue from it at intervals, indicating the rupture of optical continuity, while a low, and in some instances, almost musical crackling was, at the same time heard. Relieved from the pressure, the ice remained continuous; but a cavity being formed and the infusion placed within it, the coloured liquid immediately diffused itself through the capillary fissures, producing an appearance accurately resembling the drawings illustrative of the infiltration experiments of M. Agassiz upon the glacier of the Aar. This fissured structure, which is inconsistent with the idea of viscosity, is thus shown to be the natural result of the pressure exerted upon the non-viscous and brittle mass of the glacier.

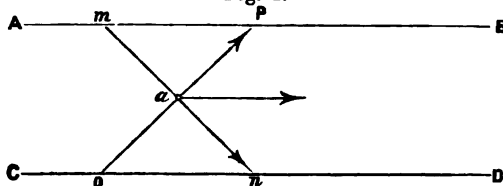
To account for a "*bruit de crepitation*" heard upon the Aar glacier, M. Agassiz refers to an observation which might be made on a fine day in summer, and which would show the air within the glacier ice escaping from its surface. M. Agassiz supposes the ice to be diathermanous; that the sunbeams therefore get through it and heat the air bubbles it encloses, which by their expansion rupture the ice, and produce the crepitation referred to. The observation is an interesting one, whatever difficulty we may find in accepting the explanation. An experiment made on the 31st of January, appears to me to account for the observation in a satisfactory manner. Snow having fallen, I was early at work compressing it; and on removing a plate of the compacted mass from the press, I noticed, as the ice melted, a sparkling motion of the surface. To imitate the action of the sun, an iron spatula was heated, and on bringing it near to the compressed snow,

The veined or laminar structure of glacier ice was next considered, which Professor J. D. Forbes in his earlier writings compared to slaty cleavage. His theory of the structure is, perhaps, the only one which has made any profound impression, and it may be briefly stated as follows:—Owing to the quicker flow of the centre of a glacier, a sliding of the particles of ice past each other takes place; in consequence of this sliding, fissures are produced, which, when filled with water, and frozen in winter, produce the blue veins of the glacier. To account for the obliquity of the veins to the sides of the glacier, a drag towards the centre is supposed to take place, producing a differential motion in this direction, which results in the formation of fissures. But at the centre of the glacier this drag towards it cannot be supposed to exist; and to account for the veins or laminated structure of the centre, which under normal conditions is transverse to the axis of the glacier, it is supposed that the thrust from behind, meeting an enormous resistance in front produces a differential motion of the particles in a direction approximating to the vertical; and that in consequence of this motion fissures are produced, which, when filled and frozen, produce, as in the other cases, the blue veins. Now, the only fact here is that of differential motion *parallel to the length* of the glacier. It is not established that the colds of winter reach to a depth sufficient to produce the blue veins, which it is affirmed form a part of "the inmost structure" of a glacier. Again, the lamination in some cases presents itself in the form of transparent lenticular masses imbedded in the general white ice; and the differential motion referred to would be mechanically inadequate to produce detached cavities corresponding to these masses, which vary greatly in size, and in some cases accurately resemble the greenish spots in slate rock, when a section perpendicular to the cleavage of the rock is exposed. Further, as the motion of the glacier takes place, both in summer and winter, it is to be inferred that the fissures are formed at both seasons of the year. If formed in winter, they cannot be filled and frozen that season for want of water; and if formed in summer they cannot, while summer continues, be frozen, for want of cold. Hence, at the end of each summer, if the above theory be correct, there ought to be a whole year's unfrozen fissures in the ice. Such fissures surely would not require the act of freezing to render them visible; they would be seen when filled with blue water just as well as when filled with blue ice. But they never have been observed; and it is therefore to be inferred that they have no existence. With regard to the drag towards the centre, which is supposed to arise from the viscosity, and in which direction it is stated that "filaments slide past each other," it is by no means clear on mechanical grounds that such a drag exists. For the transfer of matter from the sides to the centre, in consequence

the jumping of the surface caused by the issue of the air through the film of water which covered it, was greatly augmented. On removing the spatula, the motion subsided. To a similar action on the part of the sun, which melts the surface of the glacier, and thus liberates the air bubbles with which it is filled, the observation of M. Agassiz is in all probability to be referred.

of such a drag, must finally absorb the former, unless to make good the loss a motion in some other portion of the glacier from the centre to the sides, that is in a direction opposed to the theory, be established. Let the line A B, (Fig. 1) represent the centre of a glacier; C D its side, and a a point between both. Draw $m n, o p$, making equal

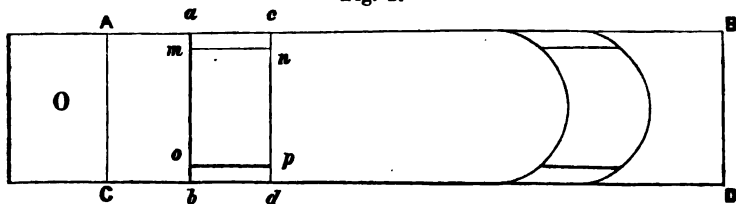
Fig. 1.



angles with the centre and side. In consequence of the quicker flow of the centre the line $m n$ tends to shorten itself, causing a *thrust* on the point a , which urges it towards the side C D; the line $o p$ for the same reason tends to elongate itself, which produces a *drag* of the point a towards the centre. The point a is here solicited by two equal forces, and the resultant motion will be along the line A B, parallel to the length of the glacier. This result receives the most complete confirmation from observation, so that the drag towards the centre expresses only half the conditions of the problem.

To test the question on a small scale, the following experiment was

Fig. 2.



made, A B C D, (Fig. 2) is a wooden trough, six feet long, and one foot wide. The end, A C, of the trough is elevated. O is a box, with a sluice front, containing fine mud, formed of a mixture of pipe-clay and water. The mud was permitted to flow slowly through the trough. Two highly colored straight lines $a b, c d$, were stamped on the mud transverse to the length of the trough, and two others $m n, o p$, were drawn parallel to its length. Now, if filaments move past each other towards the centre, such a motion must be observed by its effect upon the longitudinal lines $m n, o p$. On permitting the mud to flow, the lines lower down presented the appearance shown in the figure; $m n$ and $o p$ moved parallel to the sides, and at the same distance from them from top to bottom; there was no evidence whatever of the supposed drag; the differential motion which existed was parallel to the length of the trough, as is proved by the distortion of the lines $a b, c d$, while every point in each of the longitudinal lines moved exactly as the theoretical deduction from Fig. 1 would lead us to expect. If

then the cleaved structure were due to differential motion, we ought to expect it parallel to the sides, instead of oblique to them, as it actually exists in a glacier of the form typified by our wooden trough.

Finally, with regard to the transverse lamination of the centre of the glacier, the theory now under consideration assumes, that in a mass supposed to be viscous, with an enormous thrust behind, and an enormous resistance in front, fissures varying from a fraction of an inch to several inches in width, are formed at right angles to the direction in which the thrust is exerted. Surely, so far from producing such fissures, the direct effect of such a force would be to close them up if they existed.

The speaker next attempted to apply the theory of slaty cleavage, already referred to, to the laminated structure of the ice. The lamination, like that of slate rock, is always approximately at right angles to the direction of maximum pressure; this fact is established by the testimony of independent observers, and was first, it was believed, recognised by Professor Forbes himself. Local circumstances, which give rise to a violent thrust, produce at the same time a highly developed lamination. When two confluent glaciers unite to form a single trunk, as in the case of the Finsteraar and Lauteraar glaciers, the effect of their mutual thrust is to develop the veined structure in a striking degree along their line of junction. The mechanical condition of such a glacier was illustrated by experiments with mud, flowing through two branches, which afterwards were united in a single trunk. Small red circles were stamped all over the surface of the mud in the two branches; and as these descended they were squeezed along the centre of the trunk into ellipses so elongated that the conjugate axis in many cases disappeared wholly, and the figure became a straight line. In nature it is exactly at the places where this squeezing takes place, that the cleavage of the ice is most highly developed; a fine example of this is the structure under the central moraine of the Aar glacier. In fact, the association of pressure and lamination is far more distinct in the case of a glacier than in the case of a slate rock. We know that in the latter case pressure is the sufficient cause of the lamination; are we not justified in concluding that it is also the cause in the former? The oblique position of the veins near the sides, the transverse lamination of the centre, the lenticular structure, the relation of the veins to the crevasses, are all in harmony with this compression theory of the veined structure of glacier ice. Unless, indeed, we suppose the compacted mountain snow to be perfectly homogeneous in a mechanical point of view,—a supposition plainly opposed to common sense—some portions of it will, when under violent pressure, be rendered more compact than others, and the blue veins are the natural consequence.*

* I have recently tried to reproduce the blue veins on a small scale by compressing snow. In some cases the section of the mass perpendicular to the surface, on which the pressure was exerted, exhibited in a feeble, but distinct manner, an appearance the same in kind as that of the veined structure of glacier ice. Stripes more transparent than the surrounding ice were observed

In the investigation, the well-known "dirt bands," to which so much theoretic importance is attached, were finally considered, and an explanation of these bands, as they are seen upon the glaciers of Grindelwald and of the Rhone, was attempted. On the former glacier the bands were particularly well developed, and a portion of the glacier where they did not exist was presented simultaneously with the bands upon another portion. Their proximate origin and final completion were thus observed at once; and to account for them the following explanation is proposed; these bands, wherever they have been observed, are, it is believed, first developed at the base of an ice cascade. The dirt scattered by winds and avalanches over the upper regions of a glacier, is redistributed by the passage of the glacier through a precipitous gorge, where the ice is shattered and the dirt broken up into detached patches. On reaching the bottom, where the force becomes one of longitudinal compression, the patches of dirt are squeezed longitudinally and drawn out laterally, being thus converted into stripes of discoloration, which, owing to the speedier motion of the centre, are convex towards the lower extremity of the glacier. On consulting Professor Forbes's map of the Mer de Glace, it will be seen that the "bands" commence at the base of the icefall of the Talèfre, while none exist above the fall. Those shown on the Glacier du Géant, we are led to infer, commence at the base of the cascade of La Noire, which, however is not sketched on the map. The theory of Professor Forbes is, that a glacier throughout its entire length is composed of alternate segments of hard and porous ice, that the dirt is washed from the former, but finds a "lodgment" in the latter. The experiments on which this important conclusion is founded were unknown to the speaker, who finds observation and experiment in harmony with the explanation given above.

It may be urged that, after all, the foregoing experiments on the yielding of ice do not prove the viscous theory to be wrong. The mere fact of bending a prism of ice by fracture and regelation does not prove that it is non-viscous. This is perfectly true, nor was it conceived that the onus rested on the speaker to prove the negative here. All that was claimed for the experiments is the referring of certain observed phenomena to true causes, instead of to imaginary

at right angles to the direction of pressure. It is well known that the ice structure sometimes exhibits a true cleavage, and since the above discourse was given I have succeeded in impressing upon a perfectly transparent prism of ice a cleavage, the perfection of which surprised me. As in the case of the glacier and of slate rocks, the cleavage is perpendicular to the direction of pressure. On placing a specimen of the squeezed ice before a friend, he at first sight imagined it to be a bit of gypsum. The case then, as regards slaty cleavage and the structure of glacier ice, stands thus:—the testimony of independent observers proves that both ice and slate are laminated at right angles to the direction of pressure; and the question occurs, Is this pressure sufficient to produce the lamination? Experiment replies in the affirmative. I have reduced slate rock to an almost impalpable powder, and reproduced from it the lamination by pressure. The experiments above referred to prove the sufficiency of the pressure to produce the cleaved structure of the glacier ice. By combining the conditions of nature we have produced her results.

ones. An illustration may help to place this question in its true light. By Newton's calculation the velocity of sound was found to be one-sixth less than observation made it, and to account for this discrepancy he supposed that the sound passed instantaneously through the particles of air themselves, time being required only to accomplish the passage from particle to particle. He supposed the diameter of each air particle to be $\frac{1}{173}$ ths of the distance between two particles; and nobody ever proved him wrong. Still, when Laplace assigned a *vera causa* for the discrepancy, the hypothesis of Newton, and other ingenious suppositions, were at once relinquished. The proof, indeed, in such cases consists in the substitution of *facts* for *conjectures*, and whether this has been done in the case now under consideration the intelligent reader must himself consider.

In the foregoing remarks, Mr. Tyndall has expressed his dissent from some of the theoretical views of Professor Forbes; but he feels great pleasure in recording the high value which he attaches to the experimental labors and observations of this philosopher. Indeed, it is the fact of Professor Forbes having done so much that rendered such frequent reference to him necessary. To the same frank and friendly criticism the views propounded this evening were submitted; and a hope was entertained that the discussion of the question would result in a more exact acquaintance with the structure and motion of glaciers.

*On the RELATION of SCIENCE and ORNAMENTAL ART.**

By CHRISTOPHER DRESSER, Esq.

THE subject was introduced by a brief reference to the fact, that ornamental art is a necessity of man's nature; after which, one or two instances were brought forward in which science has directly aided ornamental art.

Chemistry was referred to as supplying many pigments; photography, as furnishing fac-similes of the choicest works of art of all ages; and "Nature-Printing," as presenting the ornamentist with the flora of the entire world, which may be remodelled by him into æsthetic ornaments.†

After advancing these instances in which science has aided ornamental art, the object of the following remarks was set forth by one or two figures. The manner in which science has revealed the composition of light, and with this the laws of harmonious coloring was alluded to, with the power thus offered of imparting to the student the rules necessary to be attended to, in order to produce agreeable effects by juxtaposing colors. It was then hinted that the object of the future remarks would be in a humble way to throw out a few

* Royal Institution of Great Britain; Friday Evening, Feb. 20, 1857; Rev John Barlow, M.A., F.R.S., Vice-President and Secretary, in the Chair.

† A new process of "Nature-Printing" was here brought forward, and its supposed merits pointed out.

suggestions relative to the laws which govern the aggregation of forms.

In continuation, it was observed that ornamental art consists of two elements, viz. design and manipulation. The former, which is chief, embracing construction and decoration.*

A few remarks were then offered upon construction, tending to show that a true principle of construction gives satisfaction, in the absence of which beauty cannot exist.

What is necessary, in order to produce beauty in construction, was then considered; but before proceeding to solve this question, the import of the term "beauty" was discussed. To the speaker's mind it conveyed some such idea as the following; that beauty is that quality in an object which causes a thrill of delight to pass through the soul of its beholder.

It was then observed, that in taking this view of beauty, all natural objects could not be considered as beautiful; as some strike the beholder as grotesque, and others as repugnant to all emotions of delight.

The term, beauty, having been thus considered, the subject was again recurred to, by calling attention to an idea that forms in architecture which certain members assume are the result of natural forces, as attraction, &c., and the more beautiful the object the more powerfully it appears to reveal the fact that it is the result of natural laws. The Greek vases were next alluded to, as presenting the appearance of being formed of a plastic material, acted upon by the combined influences of the attraction of the earth and the centrifugal force; and the speaker considered that these combined influences appeared to a considerable extent to modify, or give rise to their forms. In these instances it was also endeavored to show, that where the laws of attraction have free room to act, or where nature herself modifies the form, beauty is gained. The character of the curves given to the boundary lines of bodies by the influences just alluded to was then noticed; and it was inferred, that from the very nature of the curves thus produced beauty must be gained.

Construction having been thus considered, decoration was noticed, and reference was made to a plant (the *dilytra spectabilis*), in which it was pointed out that for the grace and flow of the larger members of the structure we are indebted to attraction, and that the operation of this influence upon arms, which would otherwise be straight, made them graceful and beautiful. This principle was pointed out as apparently existing in favorite Greek ornaments.

Other principles were alluded to as existing in certain ornaments, as the "wave," "surface attraction," &c., which tended to show, that in order to gain beauty the decorating ornament must be intimately connected with the body decorated.

In conclusion, one or two other points were touched on, from which there was no endeavor made to extract broad principles.

* See Redgrave's Report on Design for the Exhibition of 1851.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the EXAMINATION of the SILVER and BARYTA-PRECIPITATES in the QUALITATIVE SEARCH for ACIDS.

By FREDERICK W. GRIFFIN, Ph.D., Director of the Bristol School of Chemistry.

IN the systematic examination for acids, the precipitates thrown down from neutral liquids by chloride of barium and nitrate of silver are digested with dilute hydrochloric or nitric acid, that the salts of various acids may be removed from the sulphate of baryta and the silver-compounds containing the hydrogen-acids. It is invariably prescribed in works on qualitative analysis to re-precipitate these dissolved salts from the filtrate by the addition of *ammonia*. Now, as is well known, the baryta salts are considerably soluble in the solution of chloride of ammonium formed by the neutralisation, while the silver salts are not only slightly soluble in nitrate of ammonia, but are readily dissolved by the smallest excess of free ammonia. The consequence is that even with the greatest care in diluting down and adding the ammonia, no precipitate may appear when only traces of such compounds are present, and in careless hands a pretty considerable amount may be overlooked. Yet the success of the re-precipitation is often of great value as an indicating reaction. For example, traces of arseniate or phosphate of silver, mixed with much chloride, are not perceptible by color, nor yet is there any proof of the acid having dissolved them out, unless they are subsequently re-produced. I have been led to examine this subject during the preparation of a series of analytical tables, and I have found that the substitution of *carbonate of soda* for ammonia as the neutralising agent permits very small quantities of any of these compounds to be detected with certainty and ease, even by an inexperienced operator. It is true that in many cases a portion, or the whole, of the peculiar acid in the original precipitate may be replaced by the carbonic, but as (with one exception, the chromate of silver), this only occurs with precipitates which are themselves white, and as the carbonate is strictly proportionate in quantity to the salt actually dissolved, this in no way interferes with the employment of the reaction as an indicating test, for which solely is it ever employed. I have carefully studied the action with each compound singly, and will summarily state the results.

I prepared portions of phosphate, arsenite, arseniate, chromate, oxalate, tartrate, citrate, and succinate of silver, by double decomposition from the nitrate,—always taking care to have an excess of the alkaline precipitant. After thorough washing, each compound was

dissolved in the smallest possible quantity of nitric acid. The phosphate, arsenite, and arseniate dissolve extremely readily; the tartrate, citrate, and succinate, more slowly; the oxalate requires a large excess of acid, while the chromate hardly diminishes except by boiling, when it is decomposed, and an abundant deposition of bichromate in violet brown rhombic scales takes place on cooling. A few drops of each acid solution were mixed with a large and increasing quantity of water, and one half was treated with carbonate of soda; the other with extremely dilute ammonia, poured little by little down the side of the tube, so as to form a distinct stratum, only slowly mixing by diffusion. In every case carbonate of soda gave a considerable turbidity in a liquid so diluted that ammonia, with the most careful management, produced no action, even on long standing. With comparatively strong solutions of the oxalate, tartrate, citrate, and succinate, it requires much caution to obtain any result with ammonia, while carbonate of soda will sensibly precipitate a mixture of a single drop of the acid solution with an ounce of water. Moreover, the color of the phosphate, arsenite, and arseniate thrown down by even the largest excess of carbonate of soda is quite unimpaired, either while suspended in the liquid, or after subsiding. The chromate is not reprecipitable as such. Ammonia produces no effect in its solution: carbonate of soda only throws down a corresponding amount of carbonate of silver. Its color, is, however, so intense, that a very small proportion in the original precipitate will sufficiently announce its presence. The tartrate and citrate also appear to be wholly, or nearly so, replaced by carbonate. To ascertain the nature of the precipitate a considerable amount of the acid liquid was supersaturated with carbonate of soda, filtered off, and washed till the runnings were not precipitated by chloride of barium. It was then heated with a little water in a test-tube, and nitric acid added. The phosphate, arsenite, arseniate, oxalate, and succinate dissolved without any trace of effervescence; they are therefore thrown down unaltered by carbonate of soda. The arseniate is precipitated more rapidly from a boiling solution; but as the carbonate of silver which replaces the chromate, tartrate, and citrate, is reduced and blackened at that temperature, it is better in a general examination to add carbonate of soda to the liquid, in the cold. Then, however, it will show an amount of arseniate so small that no satisfactory indication of its presence can be obtained even by slightly supersaturating with ammonia and evaporating to dryness. Ammonia acts the best perhaps in the case of the phosphate, (probably in part from the small amount of acid required to dissolve it), but the mere zone that separates at the junction of the two strata is no denser or deeper colored, than the precipitate produced by carbonate of soda throughout the liquid.

The baryta salts were next examined. The phosphate, arsenite, arseniate, chromate, oxalate, tartrate, and citrate, were prepared and treated in the same manner, except that hydrochloric acid was employed as the solvent. The arsenite, arseniate, tartrate, and citrate, dissolved considerably during washing, and carbonate of soda

gave a marked and constant turbidity in the filtrate; but care was taken that there was no excess of chloride of barium in the first instance. With due caution, ammonia precipitated the phosphate, chromate, and oxalate of baryta very well, though not so largely as carbonate of soda; but it scarcely gave any result in the strongest solutions of the arsenites, arseniate, tartrate, and citrate. Carbonate of soda gave a copious turbidity even with one drop of these last in an ounce of water; and much more rapidly, and also abundantly, when added to a boiling solution, hence, it is better so applied in the case of the baryta compounds. In each instance, however, it appears to be solely carbonate of baryta that is thrown down, except with the chromate, which is of its full color, though mixed with carbonate.

To test the process in the case of mixed salts, I added a portion of a solution of arseniate of potass of known strength, containing the fiftieth of a grain of arsenic acid to a solution of two grains of chloride of sodium; precipitated boiling with nitrate of silver, digested with dilute nitric acid after thorough washing, and added an excess of carbonate of soda to the filtrate. A considerable reddish-brown precipitate of arseniate of silver was produced.

There is only one caution to be observed in the process; namely, to wash till all the chloride of barium or nitrate of silver, used to precipitate, is removed. The last rinsings should remain clear when tested with carbonate of soda.

Since, however, the solubility during washing of some of the baryta-compounds may prevent this desideratum, the treatment with carbonate of soda is certainly not so much to be recommended for the baryta as for the silver-precipitate.

The re-solution of the tartrate and citrate of baryta may be diminished by mixing the washing-water with a sixth part of ammonia; still, even in this filtrate, not only does carbonate of soda, but also any additional portion of ammonia, produce a considerable turbidity.

ESTIMATION of the COMMERCIAL VALUE of MANGANESE ORES.

By CHARLES O'NEILL.

OF the many methods by which the value of ores of manganese can be ascertained, there is, perhaps, none so simple as the following, and though it may not possess the delicacy claimed for other processes, it is sufficiently close and exact for all commercial requirements. I take an equivalent of protochloride of tin, 112.3 grains and 100 grains, or any other known weight of the sample of manganese under examination, dissolve the protochloride of tin in about an ounce and a half or two ounces of strong hydrochloric acid, in a small flask, and add manganese from the weighed quantity until the protochloride of tin is converted into perchloride; theoretically the amount required is 44 grains, consequently the quantity necessary to effect the perchlorination of the tin salt contains that much pure peroxide of manganese, from which the percentage is calculated.

The termination of the process is well marked by the change of
2 E 2

color which takes place; the first portions of manganese dissolve rapidly without producing any change of color; if there was no oxide of iron in the manganese ore, the solution remains colorless until the point of saturation is attained, when it instantly becomes brown and opaque, but iron is generally present with the manganese, and then the first change of color is to yellow, but the manganese must be added in small portions until the opaque brown color it produced. One portion of manganese ought to be fully dissolved before another is added, and as towards the end of the operation is dissolves more slowly than at the commencement, the solution should be kept near the boiling point. The testing may be completed in fifteen minutes.

The delicacy of the process will depend upon the quantity of peroxide of manganese required to give the characteristic change of color; upon trial I find 0.1 grain of nearly pure peroxide is sufficient to give the change when the manganese ore has not contained any oxide of iron, and when a considerable quantity of iron was present 0.2 grains gave a change of color sufficiently marked, the yellow color of the perchloride of iron interfering to that extent.

An opacity produced by the addition of the manganese to the chloride of tin solution only indicates the presence of earthy matters insoluble in hydrochloric acid, quartz, sulphate of baryta, &c.

The best specimen of native manganese ore that I have had an opportunity of testing contained 89.5 per cent. of pure peroxide; good commercial samples contain from 55 to 65 per cent.; one sample contained only $7\frac{1}{2}$ per cent., the remainder being sulphate of lime, carbonate of lime, &c., colored with some animal black.

The only precautions necessary are to have pure crystals of protochloride of tin, and to reduce the manganese ore to *fine powder*.

Mayfield Print Works, Manchester.

On the SOLUBILITY of SULPHATE of BARYTA in ACID SOLUTIONS.

By F. CRACE CALVERT, F.C.S., M.R.A. Turin, &c.

THERE are few substances more frequently met with and which require to be determined with greater precaution than sulphur and its compounds, it is therefore of the highest importance to possess exact means of knowing their proportions.

Up to the present time it has been admitted that sulphate of baryta was so insoluble that it was sufficient to add a soluble salt of baryta to a solution containing a sulphate, with an excess of acid, either nitric or hydrochloric (so as to avoid the precipitation of other acids by the baryta), in order that the sulphuric acid existing in a liquor should be exactly determined. My researches have convinced me that sulphate of baryta *is* soluble in acids. I have succeeded, as will be seen further on, in dissolving two grains of sulphate of baryta in 1,000 grains of nitric acid of a specific gravity of 1.167; whilst it requires 140,000 grains of water to dissolve one grain of sulphate of

baryta; and what is not less interesting is, that very weak nitric acid influences the solubility of sulphate of baryta; it is therefore essential not to acidify liquors with nitric acid.

This fact seemed to me so important for analytical chemistry, that I made a great number of researches in order to know all the circumstances which influence the solubility of sulphate of baryta; and, although I have made several hundred experiments, I shall only give here the most striking, and those which tend to demonstrate the influence exercised by masses of matter, upon their chemical affinity. I hope that these last facts will offer the more interest, as we only possess few researches demonstrating, that Berthollet expressed a correct opinion, when he asserted that the quantity of matter present exercised an influence upon chemical affinity. However, since I have commenced these researches, in 1851, several works supporting these views have appeared, those of Messrs. Bunsen, Gladstone, Henry Deville, &c.

The first idea of these researches was suggested to me by an inquiry which I had undertaken to determine the quantity of sulphur in cast and malleable irons. As the quantity of sulphur to be determined only varied between the limits of 0.1 and 0.3, it was indispensable that I should take the greatest precautions; but, notwithstanding, I still found it impossible, in making two comparative analyses of the same iron, to obtain two sulphates of baryta having corresponding weights. Desiring to know the cause of error I made several experiments, and discovered, to my great surprise, that sulphate of baryta was freely soluble in acids, and then the error in my analyses was explained. Some quantitative analyses of water, which I had to make at the same time, led me to the same result; for having followed the ordinary process which consists in adding, in the case of a water which contains carbonates and sulphates, nitric acid to the residue left by the evaporation of a known quantity of water, I determined the quantity of sulphuric acid, and was again struck by the divergence of my results. If I had contented myself by taking the mean of the results obtained, I should have been obliged, in calculating the relation of the bases to the acids, to admit, as some chemists have done, that there were silicates of magnesia and lime in the water, whilst by avoiding the solution of the sulphate of baryta, I arrived at the conclusion that the bases existed as sulphates and that the silica was free, or combined in small quantity with potash or soda.

Before giving the results of my experiments, which demonstrate the solubility of sulphate of baryta in acid solutions of different degrees of concentration, and the influence exercised by multiple volumes of nitric acid of specific gravity 1.167 on the same solubility, I shall give the method of operating which I followed.

Having placed in ten jars, of the same size, known quantities of perfectly pure sulphate of potash, I added to them a given bulk of nitric acid of sp. gr. 1.305, and a quantity of distilled water, such as when added to that which was employed to dissolve the nitrate of baryta, made a known total, which was in each successive jar a

multiple of the quantity of water put in the preceding jar. When, instead of employing acids decreasing in strength, I wished to study the influence of increased volumes of the same acid, that used was of the sp. gr. 1.167 at 60°, or equal to 27.27 of anhydrous nitric acid and 72.73 of water per cent. This strength of acid was employed in preference to any other, because it was that which gave the *maximum* of solubility to the sulphate of baryta. I also took the precaution to use new jars, in order to avoid scratches, which influence the solubility of the sulphate of baryta; and equally avoided agitating the intimate mixture of the two salts, in order to be always, as far as possible, placed in the same circumstances, so that the results might be comparative.

I will give, first, several series of results which demonstrate the solubility of the sulphate of baryta; and afterwards will examine the facts which have special relation to chemical affinity.

SOLUBILITY OF SULPHATE OF BARYTA IN MULTIPLE VOLUMES
OF NITRIC ACID.

The first series of experiments was made by placing in each jar a quantity of sulphate of potash, capable of producing 1.00 gr. of sulphate of baryta, and adding successively in each jar, multiple volumes of nitric acid having a sp. gr. of 1.167 at 60° and on the sulphate being dissolved, pouring into it an equivalent quantity of nitrate of baryta, previously dissolved in a known volume of water.

TABLE No. 1.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	0.735 gr.	1.121	0.999	Only in No. 1 did a cloud appear after 12 hours, and even after 24 hours the precipitate was very faint. No precipitate in the other nine jars.
2	80	933.6	..	..	..	
3	120	1400.4	..	..	..	
4	160	1867.2	..	..	..	
5	200	2334.0	..	..	..	
6	240	2800.8	..	..	..	
7	280	3267.6	..	..	..	
8	320	3734.4	..	..	..	
9	360	4201.2	..	..	..	
10	400	4668.0	..	..	..	

On examining the above results, it will be observed that one grain of sulphate of baryta was almost entirely dissolved in 40 volumes of acid, or in 466.8 grs. of nitric acid, and entirely so in 933.6 grs. of the same acid, and that no precipitate was formed in the other eight jars, even after standing forty-eight hours. There was therefore more than 1 gr. of sulphate of baryta dissolved in 1000 grs. of acid. Desiring to obtain a precipitate in all the ten jars, I employed $4\frac{1}{2}$ times the above quantity of nitrate of baryta and sulphate of potash, and obtained the following results.

TABLE No. 2.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	3.84	5.00	4.46	Instantly.
2	80	933.6	..	..	..	20 minutes.
3	120	1400.4	..	..	..	2 hours.
4	160	1867.2	..	..	..	8½ hours.
5	200	2334.0	..	..	..	24 hours.
6	240	2800.8	..	..	..	No precipitate.
7	280	3267.6	..	..	..	Ditto.
8	320	3734.4	..	..	..	Ditto.
9	360	4201.2	..	..	..	Ditto.
10	400	4668.0	..	..	..	Ditto.

I was much surprised to find, that even after standing twenty-four hours, no precipitates were formed in jars Nos. 6, 7, 8, 9, and 10, although the quantity of salt employed was capable of giving rise to 4.46 of sulphate of baryta.

These results are also interesting in another point of view, viz., if we calculate the quantity of sulphate of baryta dissolved by 1000 grs. of acid in jar No. 6, the quantity found is 1.593, or in other terms, there is .592 more salt dissolved per 1000 grs. of acid, than in the jar No. 2 of No. 1 Table; this difference of solubility can only be owing to the influence exercised by a greater quantity of sulphate of potash and nitrate of baryta being employed, since everything else was equal. In pursuing the reading of this paper it will be seen that the influence exerted by quantity on mass is clearly confirmed. Having remarked that in the jars, Nos. 1, 2, 3, 4, and 5, of the above series, the precipitates diminished rapidly in quantity, I collected them, and washed them until the liquor gave no precipitate, either with sulphate of potash or nitrate of baryta; but these washings are very tedious, the nitrate of baryta or the sulphate in excess adhering to the sulphate of baryta with extraordinary tenacity; and this difficulty, as I will show further on, increases still more when the recommendation given by some chemists is followed, viz., to add a great excess of nitrate of baryta to the liquor containing the sulphates.

TABLE No. 3.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Baryta Precipitated.	Quantity of Sulphate of Baryta Dissolved.	Quantity of Sulphate of Baryta dissolved in 1000 grains.
1	40	466.8	4.44	0.02	0.043
2	80	933.6	3.17	1.29	1.382
3	120	1400.4	2.02	2.34	1.670
4	160	1867.2	0.80	3.66	1.957

These figures show not only the rapid increase of solubility of sulphate of baryta in multiple volumes of nitric acid sp. gr. 1.167, but also

that the formation of a precipitate does not prevent the great solubility of this salt.

Thus, in jar No. 2, there is 1·382 dissolved in 1000 grs.

" No. 3, " 1·670 " "

" No. 4, " 1·957 " "

Therefore in each successive jar there is 0·3 gr. more of sulphate of baryta dissolved for the same volume of acid.

In the hope of producing a precipitate in all the jars of the series, I employed a greater quantity of salts for the same proportions of acid as above, and the results are given in the following table.

TABLE No. 4.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1·167.	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466·8	5·12	8·00	7·13	Instantly.
2	80	933·6	..	..	..	2 minutes.
3	120	1400·4	..	..	..	14 minutes.
4	160	1867·2	..	..	..	1 hour.
5	200	2334·0	..	..	..	1 hour 15 minutes.
6	240	2800·8	..	..	..	4 hours.
7	280	3267·6	..	..	..	8 hours.
8	320	3734·4	..	..	..	24 hours.
9	360	4201·2	..	..	..	None.
10	400	4668·0	..	..	..	None.

Although the quantity of salt employed in this case was capable of producing 7·13 grs. of sulphate of baryta, still, after twenty-four hours there was no precipitate produced in jars No. 9 and 10, and in order to obtain a precipitate in all the jars composing a series, I was obliged to employ quantities of salts of baryta and potash, capable of producing 10·0 grs. of sulphate of baryta.

In order to confirm the results given in Table No. 3, the precipitates formed in the above series were collected, and their weight determined.

TABLE No. 5.

Order of Jars.	Number of Divisions of the Alkalimeter.	Corresponding Weight of Acid. Sp. gr. 1·167.	Quantity of Sulphate of Baryta Precipitated instead of 7·13.	Quantity of Sulphate of Baryta Dissolved.	Quantity of Sulphate of Baryta dissolved in 1000 grains.
1	40	466·8	6·86	0·27	0·591
2	80	933·6	5·63	1·50	1·615
3	120	1400·4	4·66	2·47	1·767
4	160	1867·2	3·22	3·91	2·099
5	200	2334·0	2·33	4·80	2·059
6	240	2800·8	1·10	6·03	2·155
7	280	3267·6	0·14	6·99	2·141

These results prove that 1000 grs. of nitric acid sp. gr. 1·167, are able to dissolve about 2 grs. of sulphate of baryta.

ON THE INFLUENCE EXERCISED BY AN EXCESS OF
PRECIPITANT.

As it is generally admitted that an excess of precipitant increases the chances of obtaining the total precipitation of the sulphate in solution, I thought it useful to make a series of experiments to ascertain if these views were correct, and it will be seen, by comparing the results given below with those of Table No. 2, in which equivalent quantities were used, that such is not the case, since the results in the two tables are identical.

TABLE No. 6.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	8.84	10.00	4.46	Instantly.
2	80	933.6	..	..	..	5 minutes.
3	120	1400.4	..	..	..	37 minutes.
4	160	1867.2	..	..	..	2 hours 30 minutes.
5	200	2334.0	..	..	..	5 hours.
6	240	2800.8	..	..	..	24 hrs. distinct cloud.
7	280	3267.6	..	..	..	No precipitate.
8	320	3734.4	..	..	..	Ditto.
9	360	4201.2	..	..	..	Ditto.
10	400	4668.0	..	..	..	Ditto.

But this method of operating, namely, employing an excess of precipitating agent, offers, as I have already said, great inconveniences in consequence of an excess of precipitant adhering to the sulphate of baryta, which necessitates numberless washings. To avoid prolonging this paper, I shall state that the same difficulties present themselves when two equivalents of sulphate of potash are employed for one equivalent of nitrate of barytes.

INFLUENCE OF BOILING, UPON THE FORMATION OF
SULPHATE OF BARYTA.

TABLE No. 7.

Order of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp. gr. 1.167.	Quantity of Sulphate of Potash.	Quantity of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	40	466.8	8.84	5.00	4.46	Instantly.
2	80	933.6	..	..	..	8 minutes.
3	120	1400.4	..	..	..	8 hours.
4	160	1867.2	..	..	..	24 hours.
5	200	2334.0	..	..	..	No precipitate.
6	240	2800.8	..	..	..	Ditto.
7	280	3267.6	..	..	..	Ditto.
8	320	3734.4	..	..	..	Ditto.
9	360	4201.2	..	..	..	Ditto.
10	400	4668.0	..	..	..	Ditto.

On comparing the results contained in this table with those in Table No. 2, it is observed that the formation of sulphate of baryta is not facilitated by boiling. This fact was often confirmed during the course of my experiments, by taking the clear solutions which remained when the precipitate of sulphate of baryta had subsided and bringing them to ebullition, without in any case obtaining a precipitate.

(To be concluded in our next.)

TRANSLATIONS AND ABSTRACTS.

MINERALOGY.

OBSERVATIONS on ROCK SALT.

By M. MARGUERITE.

It is well known that when this salt is melted it crystallises on cooling under various forms, chiefly in cubes; these crystals are more or less confused, opaque, and always colored when the common or crude rock salt is employed. The result is very different if, calcining the quite pure salt, it is kept in a tranquil state of fusion, and then cooled slowly; the crystals then formed are sometimes under considerable volume, and perfectly transparent.

When sheltered from the air it may be fused without decoloration when in the natural state, namely, of various tints, grey, red, or brown; but if the calcination is performed in contact with the air, if as in the preceding case, the fusion is tranquil, and the cooling slow, the salt is completely decolored, the earthy matters are deposited at the bottom of the crucible, the chloride of magnesium is spontaneously decomposed, and in contact with moist air the coloring matters are destroyed under the oxidising action of the air, and all the impurities are eliminated by the crystallisation which takes place in the mass; two distinct masses are then formed, which are easily separated.

This operation may perhaps be advantageously applied to the purification of crude rock salt, and also of sea salt.

The fusion of the salt performed either in contact or sheltered from the air explains to a certain point how it is that the salt found underground is generally soiled with coloring matters, and how, on the contrary, that exposed to oxidising atmosphere is white and transparent.

From these facts, nothing can be determined as to the origin and formation of rock salt, for if we can by means of fusion obtain a salt with the appearance, transparency and physical properties of this, it is nevertheless certain that the presence of organic debris in the midst of the natural product excludes the probability of its being of igneous formation; it would be moreover difficult to understand how, if there had really been fusion of the mass, the chloride of magnesium had not been decomposed. As for the phenomenon of decrepitation, as it is observed to a certain degree in rock salt, as well as in that crystal-

lised by the humid way, it cannot serve as a characteristic to confirm the hypothesis of an igneous formation.

However this may be, without pretending to decide the question of origin, the only fact which I have ascertained is the analogous if not identical production of natural rock salt.

Comptes Rendus.

ORGANIC CHEMISTRY.

On the RATIONAL CONSTITUTION of FULMINIC ACID, and a NEW SERIES of BODIES DERIVED from ACETIC ACID

By LEON SCHICHKOFF, Lieutenant of Artillery in the Russian Imperial Guard.

ACCORDING to my recent researches, I have decided the position of these bodies already well known to belong to the numerous series of bodies derived from cyanic acid, or rather from ammonia. I speak of *trigenic*, *fulminic*, and *isocyanuric* acids.

In a former work on the fulminates, I have given it as my opinion that fulminic acid divides into isocyanuric and cyanic acids, when we endeavor to replace all its hydrogen by an alkaline metal.

I have succeeded in proving the presence of cyanic acid by ordinary reagents, in the products of the decomposition of fulminic acid, under the action of the alkaline chlorides and iodides on the fulminates.

As for the second equivalent of cyanic acid, I proved its presence in isocyanuric acid by the easy formation of cyanate of potassium and of urea, by heating respectively the isocyanurates of potassium or of ammonia. Moreover, the reaction of Gladstone, in which he produces urea and sulphocyanic acid, by treating a fulminate of ammonia and copper with sulphuretted hydrogen, and the decomposition of the fulminate of mercury by caustic potassa, with formation of cyanate of potassa, plainly shows this pre-existence of the cyanic acid groups in the fulminates. At the same time the division of fulminic acid into isocyanuric and cyanic acid, is a proof that fulminic acid really contains 2 equivalents of cyanic acid, as required by my formula:—



As for the last supposition, namely, the pre-existence of the mononitro-acetonitrile group in fulminic acid, I have proved it by the action of a mixture of sulphuric and nitric acids into isocyanuric acid.

In fact, I obtain in this case, on the one hand the products of the decomposition of cyanic acid, namely, carbonic acid and ammonia, and on the other, instead of the mononitro-acetonitrile body, a substance which analysis and reactions have proved to be trinitro-acetonitrile $\text{NC}^4 \text{ } 3 (\text{NO}^4) \text{ Cy O}^3 \text{H}, \text{NC}^4 \text{H}^2 (\text{NO}^4) + 2 \text{NO}^6 \text{H} = \text{C}^3 \text{O}^4 + \text{NH}^3 + \text{H}^2 \text{O}^2 + \text{NC}^4 \text{ } 3 (\text{NO}^4)$.

Isocyanuric acid, treated with hydrochlorate of lime, gives directly chloropicrine $\text{C}^3 (\text{NO}^4) \text{Cl}^3$, that is to say a nitrous body. The isocyanurates absorb nascent hydrogen with a reduction of the group

NO^4 , for the new products formed no longer, no longer possess their original property of exploding with heat.

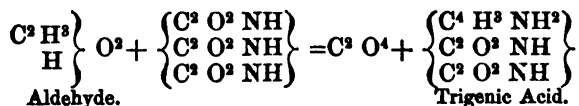
Besides this, the analysis of isocyanurates, by means of soda lime only gives, under the form of ammonia, two thirds of the nitrogen. From all this the formula of fulminic acid should be—



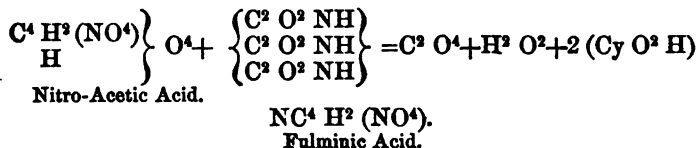
derived from three molecules of ammonia, or more directly from the binuret $2 (\text{Cy O}^2 \text{H}) \text{NH}^3$.

We see plainly that the isomery of fulminic and cyanic acids is not purely accidental, for if instead of containing one single group NO^4 , fulminic acid contained two, or even three, they could not be considered isomeric.

Trigenic acid is formed by the action of cyanic acid on the aldehyde with a disengagement of carbonic acid :—



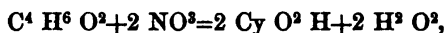
According to the constitution attributed to fulminic acid, I think that its formation may be explained in a manner analogous to that of trigenic acid. Thus :—



There would consequently be on the one hand a formation of nitro-acetic acid, on the other of cyanic acid, and a mutual reaction between these two bodies.

The remainder of the cyanic acid being found in the fulminic acid, renders this supposition very probable.

Moreover, the formation of hydrocyanic acid, in the products of oxidation of alcohol by nitric acid is well proved, and cyanic acid is only the acid of the former. This formation of cyanic may be represented by the following equation :—



an equation which is by no means new in chemistry.

As for trinitro-acetonitryle it is solid at the ordinary temperature, fusible at 41°C . (105°F .), decomposing at a temperature of 220°C . (428°F .), with a loud detonation, and the production of a blue flame.

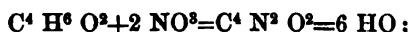
Water and alcohol decompose this body; anhydrous ether is its only solvent without decomposition.

Comptes Rendus.

REPORT on the ABOVE. By M.M. BALARD and DUMAS.

THE fulminating mercury used in the manufacture of explosive trains, is obtained by causing a very acid solution of nitrate of mercury to act on alcohol. It has been long known that this remarkable compound contained mercury, but we were ignorant of the precise nature of the elements associated in it, when a work of MM. Gay Lussac and Liebig, gave an elementary analysis which time has confirmed; this established, 1st, that fulminating mercury may be considered as a salt; 2nd, that the acid which it contains may in its turn be regarded as cyanic acid, but as a cyanic acid of which two molecules are united in one.

Doubtless, the formation of this condensed cyanic acid was easily understood, because—



doubtless, likewise, on considering the composition of fulminate of mercury as being represented by cyanic acid and oxide of mercury, it was not difficult to explain the considerable explosive power of a body which is susceptible of being converted into nitrogen, carbonic acid, oxide of carbon, and vapor of mercury.

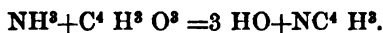
As for the facility with which fulminating mercury detonates, it is explicable by admitting that this compound is the product of the union of elements held together by very weak affinities.

Still it must be agreed that the nature of fulminating mercury left many points in obscurity. For example, why, when the cyanates and cyanurates do not detonate, should the fulminates, precisely situated between the two, have the power of detonating with so much violence?

Since the discovery of gun cotton, the idea that all, or at any rate a portion of the fulminating nitrate of mercury might belong to a nitrous compound, naturally presented itself to the mind, and thus M. Gerhardt was led to propose a formula to represent its composition which brought the composition of this explosive body near to gun-powder, nitrate of methylene and gun-cotton, that is to say to those numerous detonating bodies, whose destruction is due to the sudden action of nitric acid or its derivatives on charcoal.

The researches of Lieutenant Schichkoff demonstrate: 1st, that the formula of fulminic acid should be doubled again; 2nd, that besides the two molecules of cyanic acid which really form part of it, there is likewise a molecule of mononitrated acetonitrile.

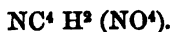
Acetonitrile $NC^4 H^3$, is a body derived from the action of ammonia on anhydrous acetic acid.



This is the homologue of nitrate of potassium NK^3 and ammonia itself NH^3 .

As for mononitrated acetonitrile, it is simply this same body which

by losing one molecule of hydrogen under the influence of NO^5 , and gaining a molecule of NO^4 is converted into—



Now as this last compound is isomeric with cyanic acid itself, since it gives $\text{C}^4 \text{N}^3 \text{O}^4 \text{H}^3$ which represent two molecules of hydrated cyanic acid, it is easy to understand that elementary analysis alone would not have been able to resolve the question which the experiments of Lieutenant Schichkoff appear to have decided.

Indeed it was required to know, not whether fulminic acid contains the elements of four molecules of cyanic acid, but if, by a peculiar means of association, two of these molecules constitute in this complex acid one molecule of mononitrated acetonitrile.

Before describing the facts which reply to this question, we must ourselves make two remarks.

In the first place we must consider the danger to which the author was exposed in investigating this difficult subject, since he frequently had to operate on 8 or 10 kilogrammes of fulminating mercury to study its reaction or extract the bodies derived from it. Fortunately he soon found; 1st, that fulminate of mercury crystallised in water detonates less readily with heat, because it does not decrepitate like ordinary fulminate; 2nd, that this fulminate may be weighed in water without danger and with great precision, its density 4.4018 being known; he likewise determined this point with great care.

The second observation is to encourage chemists to be less content than they are with those provisional explanations drawn from equivalent formulæ, and to advance, by suitable combined reactions to the principle of things, the true constitution of the molecules of bodies.

Treated with potassa or the alkaline chlorides and iodides, fulminate of mercury gives cyanate of potassa; the existence of cyanic acid in this salt is thus proved.

But, by a first step in this difficult study, M. Schichkoff found that fulminic acid may be decomposed into cyanic acid, and a new acid which he calls isocyanuric, and which M. Liebig called fulminuric.

The products being such that their sum represents four molecules of cyanic acid, for isocyanuric acid represents three of them, there was already some reason to change the formula of fulminic acid.

But as isocyanuric acid, which contains 3 equivalents of nitrogen only gives 2 under the form of ammonia when decomposed with soda lime, it results that at least 1 equivalent of nitrogen is found in it in a form which is not cyanogen.

If we add that under the influence of nascent hydrogen the detonating isocyanurates show a phenomenon of reduction which takes place very quickly and lose their explosive power, it is difficult not to recognise the analogy of this reaction with that which takes place in nitrous compounds.

Moreover, we know that nitrous compounds of an organic nature give, under the influence of chlorine, a compound known under the name of chloropicrine $\text{C}^3 (\text{NO}^4) \text{Cl}^3$, in which hyponitric acid incon-

testably forms a part, and the author by treating isocyanurate of potassa with chloride of lime removed chloropicrine.

There remained to ascertain how the body NO^4 was engaged whether in isocyanuric acid or in fulminic acid, whence it was derived. This the author has very happily accomplished by subjecting isocyanuric acid to the action of a mixture of sulphuric and nitric acids.

The reaction is powerful, it gives carbonic acid and ammonia with the cyanic part of the acid; it furnishes trinitrated acetonitrile, quite a new body, with the remainder of the elements of the acid.

Now, that trinitrated acetonitrile $\text{NC}^4\text{H}^3(\text{NO}^4)_3$ is derived by metalepsy from $\text{NC}^4\text{H}^3(\text{NO}^4)$, is what no one will contest. The demonstration of the existence of such a group in isocyanuric acid can no longer be considered doubtful, and consequently it has been admitted in fulminic acid itself.

If the empiric formula of fulminic acid continues to be represented by condensed cyanic acid, its more complex rational formula will be represented by—



In this memoir the author shows that all the known properties of the fulminates receive by the application of this formula a more natural and more simple explanation. He shows at once in this discussion, and in the choice of the personal experiments by which he determines each reaction a profound knowledge of his subject as well as of the most delicate laws of the science and of the most certain practices of the art of experimenting.

The work of Lieutenant Schichkoff was concluded in the laboratory of M. Bunsen. He asks from the Academy that sincere judgment which it never refuses to serious works.

This memoir contains, besides the discovery of new and very interesting bodies, a very clever rational analysis of fulminic, and clearly showing its true formula. This difficult and dangerous work has been accomplished by availing himself of all the resources of the science, which are remarkably familiar to Lieutenant Schichkoff.

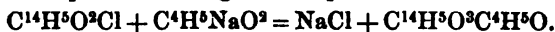
Comptes Rendus.

On a NEW SERIES of ARTIFICIAL OXYGENOUS BASES.

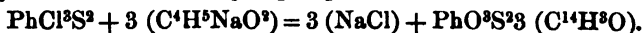
By M. S. CLOEZ.

THE number of the artificial tertiary bases has increased considerably of late years, and is likely to be increased every day; but we are at present acquainted with scarcely ten oxygenous alkaloids prepared artificially in our laboratories. The reproduction of this kind of bodies is, however, interesting in several respects—in the first place, in a purely chemical point of view, since it will one day lead, perhaps, to the economical production of some of those substances which are so valuable from their application to medicine, which are now extracted, at great expence, from natural products, as for example, opium and cinchona.

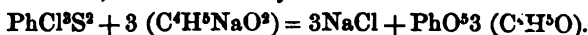
The moderate price of sodium admits of our employing it as a reagent in laboratories; its action on the various alcohols gives rise to perfectly definite combinations, capable of being transformed by double decomposition, or by substitution, into new bodies, the nature of which may most frequently be foreseen. It is thus that most of the compound ethers may be obtained by the reciprocal action of soda-alcohol, and of the chloride corresponding to the acid which we wish to unite with the ether. Ethylate of soda, for example, treated with chloride of benzoïle, gives benzoïc ether and chloride of sodium; the reaction takes place according to the equation :—



With the same compound and the chloro-sulphuret of phosphorus, I lately obtained neutral sulpho-phosphoric ether :—



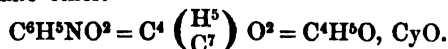
The substitution of the oxychloride of phosphorus gives neutral phosphoric ether, which is already known :—



I tried to produce the cyanic and cyanuric ethers by a similar process, by making the gaseous liquid or solid chlorides of cyanogen act on soda-alcohol; the reaction, indeed, took place as I expected; but, on examining the products, I ascertained that they differed completely in their nature from those which I sought to obtain.

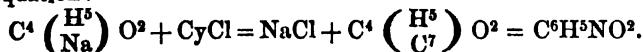
When gaseous chloride of cyanogen is passed slowly into a solution of ethylate of soda in absolute alcohol, the gas is quickly absorbed, and an abundant white deposit of common salt is formed, and at the same time, a very great increase of temperature is observed; after the reaction, the chloride of sodium is separated by filtration, and the clear liquor is submitted to distillation on a sand bath, in order to separate the alcohol and the excess of chloride of cyanogen employed. We obtain as a residue a syrupy liquid, with a slight deposit of chloride on the sides of the retort; this liquid differs from cyanic ether in all its properties. Thus, it is almost inodorous, and is not volatile without decomposition; most of the acids decompose it, forming perfectly definite crystallisable combinations; concentrated potassa decomposes it, but with the disengagement of ammonia, without producing ethilia; it has only one character in common with cyanic ether, and that is its insolubility in water; this property enables us to free it by washing from the traces of chloride of sodium which it retains; it is afterwards easily dried by exposing it for twenty-four hours in dry *vacuo* over sulphuric acid.

The product thus prepared and purified was analysed: the most simple formula corresponding to the centesimal composition found experimentally, is that of the cyanuretted alcohol, isomeric and not identical with cyanic ether.



The formation of this new body, which I propose to designate under

the name of *cyanetholine*, is easily understood; it occurs according to the equation:—



Cyanetholine belongs, with the analogous bodies furnished by the various alcohols, to a new series parallel to that of glycolle, *alumina*, *lencine*, &c. These two series are very much alike: the chemical properties of their respective corresponding terms are similar; knowing the one, we may almost with accuracy guess the other.

Cyanetholine is a slightly colored liquid of oily appearance; its density at 15° C. (59° F.) is 1.1271; it has a rather bitter taste, followed by a very disagreeable and persistent acridness; its odor is feeble, and resembles that of sweet oil of wine; when it is submitted in a retort to the action of heat, it is decomposed into a colorless volatile liquid, and a black carbonaceous residue.

Cyanetholine is insoluble in water; it mixes in all proportions with alcohol and ether; concentrated potassa decomposes it with the aid of heat; it dissolves in hydrochloric acid, with which it forms a crystalline hydrochlorate, capable of uniting with the bichloride of platinum, giving rise to a yellow double salt. The sulphate is obtained under the form of small prismatic crystals, by the spontaneous evaporation of the solution of the base in sulphuric acid.

The nitrate forms a double salt with nitrate of silver; this combination is obtained in very bulky crystals by mixing a dilute solution of nitrate of silver with a solution, also dilute, of cyanetholine, in a slight excess of nitric acid.

Nitrous acid, produced by driving linexide of nitrogen into the solution of cyanetholine in nitric acid, converts this substance into a solid and crystallisable acid matter; the reaction occurs, as with *alumina*, with an effervescence due to the disengagement of nitrogen. The acid produced has not yet been analysed, owing to insufficiency of quantity. It will be curious to see whether this body is not an isomer of acrylic acid, or rather an homologue of maleic or fumaric acid. If these views are realised, we may hope to reproduce this latter acid artificially, beginning with methylic alcohol, and passing successively through the stages of soda, methal, and cyanetholine.

Comptes Rendus, No. 9, March 2, 1857.

ACTION of MONOHYDRATED SULPHURIC ACID on JAPAN CAMPHOR.

By M. J. CHAUTARD.

I HAVE already established that camphor may naturally assume several isomeric states, very distinct in their optical properties.

I have since proceeded to examine whether common right camphor (Japan camphor) could not be modified so as to transform it into inactive camphor. An old experiment of M. de Lalande gave a peculiar interest to this question. According to this chemist, camphor, treated with a great excess of monohydrated sulphuric acid and

heated to 100° C. (212° F.), for about an hour, would be converted, without any disengagement of sulphurous acid, into a volatile oil possessing exactly the same composition as itself—the same boiling point, the same density, and differing from camphor only in its liquidity and a sensible weakening of the rotatory power. This oil after prolonged digestion with potassa at a temperature of about 200° C. (392° F.) would be transformed into a crystalline product, completely identical with natural camphor, but endowed with a power of rotation decidedly weaker than this last substance, although greater than that of the oil producing it. Does this result depend on a molecular constitution peculiar to regenerated camphor, or rather, may it not proceed from the mixture of a little of the original oil united to a certain quantity of natural camphor, whether the latter be really produced under the influence of potassa in the act of distillation, or whether it pre-existed in the oil, being kept in solution in it? This was the question which I have endeavored to solve, with the aid, simultaneously, of the indications given by chemical analysis, and by the observation of the rotatory power.

250 grammes of Japan camphor were mixed with 1 kilogramme of common sulphuric acid. The solution of the camphor, which was commenced without heat, was quickly performed at the temperature of 100° C. (212° F.), and at the same time the liquor assumed a very deep yellow color. One portion of the solution was precipitated by water, after one, two, four, six and eight hours' treatment in the sand bath. The first precipitates were merely camphor slightly colored—the last a mixture of camphor and of a blackish oil which was more abundant when the exposure to heat had been the most prolonged. The camphor separated in each of these operations was recognised as identical in every respect with the camphor originally employed. The power of rotation of the oil, however, continually decreased. In the course of the operation, sulphurous acid was constantly disengaged. After twelve or thirteen hours' treatment, precipitation with water produced a liquid which was completely without action on polarised light.

When purified by several washings with a solution of potassa and distillation over chloride of calcium, oil of camphor is quite colorless, of a slightly aromatic odor, and boiling at 240° C. (464° F.) The greater part of the liquid distils at this temperature; towards the close of the operation, the thermometer rises rapidly. The last products were kept separate.

The density of this oil was at 6° C. (42° F.) equal to 0.974. At —10° C. (14° F.) it had not solidified. When treated with boiling nitric acid it dissolved with a production of nitrous acid; water precipitated from it a soft resin, soluble in alcohol, uncrystallisable. Neither camphor nor camphoric acid were produced in this reaction. Fuming sulphuric acid reddened it without forming any combination with it.

Oil of camphor was heated for nearly six hours with potassa in fusion, out of contact with the air. On cooling, the whole mass so-

lidified, and a yellow resinoid matter was separated from it, which, when carefully distilled, gave no trace of camphor.

Finally, this oil when analysed gave me the following results :—

$$\begin{array}{ll} \text{C} = 77.33 & \text{H} = 9.70 \\ \text{C} = 76.86 & \text{H} = 9.46 \end{array}$$

These numbers, as well as the density of vapor obtained directly, 4.517, according to the process of M. Dumas, agrees with the formula



The oil, which is the principal object of this note, is not the only body formed. Besides the deposit of charcoal, often considerable, which is separated mechanically from the product precipitated by water, there remains a resinous matter at the bottom of the retort in which the rectification has been performed; and, moreover, when about two thirds of the liquid has passed over, the thermometer rises rapidly. I made two analyses of the last product of distillation, and obtained the following numbers :—

$$\begin{array}{ll} \text{C} = 78.25 & \text{H} = 10.29 \\ \text{C} = 78.82 & \text{H} = 9.88 \end{array}$$

These numbers nearly approximate those given by Japan camphor; the hydrogen differs the most.

Wishing to ascertain whether this liquid had a stable composition, I treated it with monohydrated sulphuric acid on the sand bath. A little sulphurous acid was still disengaged. After two hours the oil was, as usual, separated and purified. The analysis which I made of it gave me

$$\text{C} = 77.23 \text{ and } \text{H} = 9.76,$$

the numbers of the formula



The result of these experiments is, that the substance studied by M. de Lalande was only a mixture of oil of camphor and ordinary camphor producing a mixed system, whose composition, chemical properties, and rotatory power, should approach those of camphor the more nearly as the quantity of this last body contained in it is greater. This oil is therefore a new body, very distinct from camphor, and I propose giving it the name of *camphrene*.

Comptes Rendus.

NOTE on the TRANSFORMATION of SOLUBLE GUMS into INSOLUBLE GUMS.

By M. A. GELIS.

ALTHOUGH gummy matters are found in almost all plants, they have hitherto been the subject of very few observations. M. Guérin Vary is almost the only chemist who has studied in a consecutive manner their analysis and their transformations. He has extracted from natural gums various substances, all of which possess the property of

giving mucic acid when they are heated with nitric acid, and from all these substances he formed a genus whose species are—Arabine, Bassorine, and Cerasine. Berzelius has not adopted this division; he gives the name of *gum* only to matter soluble in water, which forms almost the whole of gum Arabic and gum Senegal, and he separates, under the designation of vegetable mucilage, the principle insoluble in water, which communicates to the gum of Bassora, to gum tragacanth, to linseed, and to quince-seed, &c., the property of swelling up very considerably in this liquid.

The analogy which exists between gums and starch, and the apparent resemblance which is observed between paste and vegetable mucilage, led us to think that this property of swelling up in water which certain gummy matters possess, was due to a kind of organisation existing, in different degrees, in the different species. According to this view, and by establishing, for gum, a series parallel to that of starch, we should be in some measure forced to consider cerasine and bassorine as representatives of organised fecula, and arabine as the analogue of dextrine. The observation which forms the subject of this note is of a nature to lead us to the contrary opinion, since, by heating gum Arabic—that is to say, by placing it in conditions of disorganisation, I have converted it into a principle possessing all the properties of the mucilage of Berzelius.

When powdered gum Arabic is heated to 100° C. (212° F.), a certain quantity of water is removed from it, and its composition may then be represented by the formula $C^{12}H^{11}O^{11}$. At 120° C. (248° F.), it loses another equivalent of water, and becomes isomeric with very dry starch, $C^{12}H^{10}O^{10}$. In this state it has experienced no change in its nature; it is still entirely soluble in water, with the exception of a slight calcareous deposit which precipitates from its solutions. But when the temperature of 120° C. (212° F.) is exceeded, and that of 150° C. (302° F.) reached, or if it be only continued for a long time, it is remarked that almost the whole of the gum loses its solubility, and is converted into a mucilaginous matter, insoluble in cold water. This reaction is effected without loss of water, as I ascertained by weighing several times the flask in which I heated the dried gum; and it is quite comparable to that which converts starch and soluble starch into dextrine.

To obtain the perfectly colorless product several precautions must be taken. It must not be heated, for gum is very rapidly colored by the action of fire. At the temperature of 150° C. (302° F.), the parts nearest to the metallic bath or the oil bath are perceptibly yellow, but the centre of the mass has been modified without coloration.

Gum thus treated is insoluble in cold water, and it is separated, by means of this liquid, from that which has not been altered. It swells up in cold water much less than tragacanth, but quite as much as the cerasine extracted from the gum of this country. When, instead of heating gum Arabic in powder, we heat it in pieces, the product obtained acts in water like the gums of cherry and apricot trees, &c., from which it would be very difficult to distinguish them.

In presence of these characters, we are tempted to admit the identity of the two matters, especially when we reflect that the metamorphosis which forms the subject of this note is produced without loss of water, and that cerasine and arabine have exactly the same composition in hundredths.

Be this as it may, modified gum, or artificial cerasine, has all the known properties of the mucilage of Berzelius.

Treated with nitric acid, it furnished me an abundant crystallisation of mucic acid, and, like gum tragacanth and indigenous gum, it gives rise, by boiling in water, to a new soluble gum.

This gum is generally regarded as arabine; I thought at first that it should rather be analagous to the gummy matter which MM. Biot and Persoz obtained by boiling 8 parts of gum Arabic in $17\frac{1}{2}$ parts of warm water, acidulated with 2 parts of sulphuric acid, a matter which they compared to dextrine; but I dare not yet express myself positively, for this regenerated gum acts at 150°C . (302°F .) like arabine itself, and constantly reproduces artificial cerasine.

The question might perhaps be decided by examining comparatively the rotatory power of these substances, but, having no instrument, I have not yet been able to make this verification.

Comptes Rendus, No. 4, Jan. 26, 1857.

ANALYTICAL CHEMISTRY.

On the ESTIMATION of CHLORINE, BROMINE and IODINE.

By M. F. PISANI.

Estimation of Chlorine.—After slightly acidifying the chloride to be analysed, with pure nitric acid, I pour in, by means of an estimated liquor of nitrate of silver, a known quantity of silver used in slight excess (2 or 3 milligrammes at most), then I filter to separate the chloride of silver, and wash it with care, I estimate the excess of silver in the filtered liquor and have, by the difference, that which has combined with the chlorine and consequently the quantity of chlorine itself. This estimation I perform by means of iodide of starch, which enables me to appreciate the excess of silver employed to the tenth of a milligramme. This manner of estimating chlorine gives very exact results without having recourse to weighing.

Estimation of Bromine.—Operate in exactly the same manner.

Estimation of Iodine.—For the estimation of iodine the same method will answer, but the following plan is more simple:—

Add to the liquor containing the iodide to be analysed, a few drops of soluble iodide of starch in sufficient quantity to give it a distinct blue tinge; then drop in an estimated solution of silver until the iodide of starch is decolorized; this does not take place until after the complete precipitation of all the iodine contained in the alkaline iodide. From the number of divisions employed may be calculated the corresponding quantity of iodine. For greater exactness the

silver employed to decolor the iodide of starch may be deducted, a quantity which may previously be ascertained by putting, to color the liquor, half a cubic centimetre of a normal solution of iodide of starch.

Estimation of Chlorine and Bromine.—These two bodies may be precipitated by a known slight excess of nitrate of silver, filtering to precipitate the chloride and bromide of silver, washing and weighing with great care. In the filtered liquor we estimate, with iodide of starch, the excess of silver employed, and we have, by the difference, within one-tenth of a milligramme of the silver which has combined with the chlorine and bromine. Knowing the weight of the chloride and bromide of silver as well as the quantity of silver which they contain, the weight of the chlorine and bromine contained in the mixture may easily be calculated.

Estimation of Bromine and Iodine.—The same method.

Estimation of Chlorine and Iodine.—In this case the same method may be used; but I prefer acting in the following manner, which dispenses with weighing. To the mixture of chloride and iodide are added some drops of soluble iodide of starch and the iodine is estimated as above, that is to say the estimated solution of silver is poured in until the iodide of starch is decolorized. The quantity of silver added corresponds exactly with the iodine contained in the mixture; for the iodide of starch is decolorized before the precipitation of the chlorine. To estimate this latter, the solution of silver is to be poured in until there is a slight excess. After filtering, the excess of silver is estimated so as to know by the difference how much is combined with the chlorine, and the corresponding quantity of chlorine is then calculated. This estimation is very quickly made, and is, at the same time, very accurate.

Estimation of Chlorine, Bromine and Iodine.—I make two parts of the liquor: into one I pour a known quantity of silver in slight excess, after filtering and washing, I weigh the chlorides, bromides and iodides of silver, and in the filtered liquor I estimate with iodide of starch the excess of silver. I find the silver combined with the three metalloids by the difference.

In the other portion of the liquor I eliminate the iodine precipitating it by nitrate of palladium, filtering, then getting rid of the palladium itself out of the filtered liquor. The bromine and chlorine alone remain in the liquor, which I estimate as above described. Only that in this case it is useless to take the weights of the chlorides and bromides of silver, because, knowing the iodine by the difference between the weights of silver at first combined with the chlorine, bromine and iodine, and then with the chlorine and bromine, we thus know the weight of the iodide of silver, and it is sufficient to deduct this from the first weight obtained to have that of the chlorides and bromides of silver.

Comptes Rendus.

INORGANIC CHEMISTRY.

On the ANOMALIES which ALUMINIUM presents as regards CHEMICAL PHILOSOPHY. By M. CHARLES TISSIER.

Relation between the Density and Alterability.—Until now, the least oxidisable metals have been found to be the heaviest, such as mercury, silver, gold, and platinum; nevertheless, aluminium, whose density is only 2.56, is less alterable than any other ordinary metal, after gold, silver and platinum.

Relation between the Atomic Weight and Alterability.—In general metals are all the more alterable as their atomic weight is lower; nevertheless, the atom of aluminium, which is so difficultly oxidisable, only weighs 14, that is to say, it has the lowest atomic weight of any of the common metals. It is half that of iron, which is 28.

Relation between the Density and the Physical Properties of Metals.—No other body has hitherto been met with of so small a density as aluminium, possessing the hardness, malleability, tenacity, conductivity and sonorousness which form the characteristics of what we term a metal.

Aluminium and M. Thénard's Classification.—According to M. Sainte-Claire Deville, aluminium does not decompose water; from this it should be put at least in the fourth section, according to M. Thénard's classification, and yet its oxide (alumina) is unreduced by hydrogen and carbon, and even by sodium and potassium.

Aluminium does not decompose water, (but we find that it absorbs the oxygen of the air at a high temperature); it decomposes carbonic acid and silicic acid exactly like potassium or sodium.

What Rank should be given to Aluminium?—This metal ranks near silver from its action on water and oxygen; near the alkaline metals from its action on silicic acid, boracic acid and carbonic acid; and near iron by its action on the metallic oxides; in fact it decomposes all the oxides which iron decomposes, except oxide of zinc. According to this last peculiarity, aluminium would have less affinity for oxygen than iron, and would rank next to iron, but after it.

If we consider the place which aluminium should hold in electrochemical order, we find that it precipitates all the metals from their chlorides, and we have ourselves found that this rule includes lead and cadmium; it should consequently be placed between cadmium and iron.

In fact, until chemistry has made further progress, we must not attempt to assign an *exact* place in classification to aluminium. However, as M. H. Sainte-Claire Deville has observed, it will be least out of place next to iron. For we know that at a high temperature iron decomposes silicic acid, boracic acid and carbonic acid; and if aluminium has not the same action on oxygen and water that iron has, that is because, as has been observed by M. Deville, we are not acquainted

with oxide of aluminium of the formula R^3O^4 , an oxide which iron is always disposed to form at a high temperature.

Finally, if we consider its electro-chemical properties, aluminium would still find its place near iron.

We will conclude by showing the most remarkable properties of this curious metal, those which distinguish from all others.

1st. Its low density ;

2nd. Its resistance to the action of oxacids and sulphuretted compounds, a resistance which approximates it to gold and platinum ;

3rd. The difficulty with which it endures alloying, a property which is unshared by the malleable metals ; for iron, zinc, lead, tin, copper, silver, gold and platinum form with each other more or less malleable alloys ; whereas aluminium will not take more than 10 per cent. of a foreign metal, or cannot itself enter in larger proportion without seriously modifying the ductility of the metal with which it is mixed.

Comptes Rendus.

On the COMPOSITION of WATER.

By M. PELIGOT.

THE waters of torrents and rivers contain in solution saline matters, which they remove from the soil and gaseous products which they take from the atmospheric air. But the air is not the sole cause of the presence of these gases. In a work which I read to the Academy about two years ago, I showed that if nitrogen and oxygen, which all running waters hold in solution, are always found in the proportions required by the law of Dalton and Henri, on the solubility of gaseous mixtures, proportions which establish in the clearest manner their atmospheric origin, it is not the same with respect to carbonic acid, which is always found in considerable proportions in the air extracted from these waters by boiling them. I have called attention to the presence of this carbonic acid, which, in default of direct estimations, have generally been neglected as far as strict accuracy is concerned, is already proved by the presence of carbonates of lime and magnesia, which form the greater part of the residues left by the evaporation of these waters. By means of a slight modification which I have made in the apparatus employed for the extraction of air from water, I have ascertained, by numerous analyses made during several months, that carbonic acid forms about half of the volume of the gases dissolved in the water of the Seine. I have supposed that the same should be the case in the water of torrents and rivers, adding, that although these proportions may be much larger than those given by most chemists who have been in the habit of analysing waters, which is in consequence of the unsuitable apparatus which they have used, they differ but little from those given by MM. Thénard and Colin for the water of the Seine, by M. H. Deville for the same water and that of several other large rivers, and by M. Dupasquier for the water of the Rhone. But these results had not sufficiently attracted general attention, and no one had sought for the origin of this carbonic acid.

I attributed this origin to the dissolving action which rain water exercises on the air confined in vegetable earth. The latter containing organic matters, which, by their slow combustion, produce carbonic acid, is found enveloped in an atmosphere which, according to the experiments of MM. Boussingault and Lewy, contains 250 times more carbonic acid than the external air. The rain water which falls on a more or less fertile soil, thus becomes charged with carbonic acid. The latter, in its turn, exercises its solvent action on the calcareous carbonate which this water, become running water, finds in the soils through which it passes.

But does not the rain water itself already contain the carbonic acid found in the waters of which it is the origin? To reply to this question, it was necessary to determine the quantity of carbonic acid found in rain water.

The result of this analysis was what might have been expected. Rain water collected directly in the month of May last, in large earthen pans placed in a garden, was boiled in the modified apparatus which I mentioned in my previous memoir; it gave 23 cubic centimetres of gas per litre. A hundred volumes of gas contain 2.4 of carbonic gas; the remainder is a mixture of oxygen and nitrogen in the usual proportions, namely—32 of oxygen and 68 of nitrogen per cent. of the gaseous mixture.

This small proportion of carbonic acid is precisely that which should exist in rain water, in conformity with the law of Dalton and Henri, in the ratio with the coefficient of solubility and of the 4 ten-millionths of the gas which atmospheric air contains.

In fact, M. Bunsen, who has recently occupied himself with the verification of this law, has found by calculation that 100 parts of the air of rain water, taken at a temperature of 10° C. (50° F.), should contain 2.46 of carbonic acid.

Therefore this gas is found in much larger proportion in common water than in rain water.

I found great interest in comparing these results with those obtained by studying water of quite a different nature. For this purpose I made an attentive examination of the water of the Artesian of the abattoir at Grenelle.

This was a very long and immensely expensive case of boring. In the month of February, 1841, after seven years of incessant labor, M. Mulot found, at a depth of 548 metres, in the green sand, under the argil of the *gault*, a mass of water of not less than 800 or 1000 cubic metres per twenty-four hours. The great influence of M. Arago, and the lively interest which the Academie des Sciences showed in the success of this enterprise, were not without effect on the continuation of the works, which at length led to such important results.

The composition of this water, which comes to the surface at a temperature of 28° C. (82° F.), was determined in 1841 by M. Payen, and in 1848 by MM. Boutron and Henri. These analyses have clearly established that the water of this Artesian well is of good quality;

that it is even purer and less charged with saline matters than any of those which supply the city of Paris.

These analyses, although they agree as to the weight left by the evaporation of the same quantity of water, present when compared, such different results that they lead to believe that the water of the well at Grenelle cannot have had the same composition at the times when they were made. Indeed it is well known that these wells undergo, from time to time, more or less prolonged perturbations. Besides the interest which my own investigations made me feel in the matter, it would not appear unsuitable to ascertain whether the water of this well, now in use for fifteen years, now has the same as in 1841 or in 1848; moreover the new well, now being bored by the city of Paris, makes this investigation all the more interesting.

On calculating, by means of the numbers given by M. Payen and MM. Boutron and Henri, the quantity of each of the elements contained in 100 parts of residue, obtained by the evaporation of the water, we have the following results:

	M. Payen's analysis.	MM. Boutron and Henri.
Carbonate of lime . . .	50.0	14.9
Carbonate of magnesia . . .	10.4	4.4
Carbonate of potassa . . .	16.5	5.6
Sulphate of potassa . . .	8.8	} 23.7
Sulphate of soda . . .	„	
Chloride of potassium . . .	8.0	} 42.3
Chloride of sodium . . .	„	
Silica . . .	4.3	7.4
Peculiar yellow substance . . .	traces	„
Nitrogenous organic matter . . .	1.7	traces
Alumina and oxide of iron . . .	„	1.4
	99.7	99.7

To render these results comparable, I had to calculate the carbonates of lime, magnesia, and potassa. In M. Payen's analysis, this last salt is considered in the state of bicarbonate; and in that of MM. Boutron and Henri, these three carbonates, being in the state of bi-salts in the water, are calculated in that form. By deducting this excess of carbonic acid we find that, according to M. Payen, one litre of water, when evaporated, leaves 0.1359 gr.; and 0.1347, according to MM. Boutron and Henri.

I shall proceed to my own examination of the gases which the water of the well of Grenelle holds in solution.

This water rises to 38 metres above the level of the abattoir; flows at that height into a copper basin, running at once through the central tube of 22 centimetres in diameter, and through the space between the tube and the sides of the well itself. The sheet of water received in this basin, which, about 44 metres above the level of the Seine, flows through a large pipe to the upper reservoir at the Place de l'Estrapade, whence they supply a portion of the town.

I first collected the water which I intended to analyse at the tap used for the abattoir; namely, about the level of the soil. This tap is fixed to the tube in which the water descends from the upper reservoir. This water, received in an earthen vessel, has at first a milky appearance, which soon disappears on the disengagement of small bubbles of gases which give it that appearance. This is a mixture of nitrogen and oxygen, containing 5 to 6 per cent. of oxygen less than atmospheric air.

Having observed that this air dissolves very rapidly in water, so that to collect it I was obliged to make the tube convey it to the top of a phial already filled with water prepared to receive it, I suspected that this gas was a foreign body in the water at the moment when it entered the upper reservoir: to ascertain this, I mounted the platform which supports this reservoir—a platform which is reached by a kind of wooden ladder, of 38 or 40 metres in height.

I then found that the gas which I had collected and analysed was merely atmospheric air which had entered the descending pipe, the water pouring through it producing an aspiration of air similar to that obtained in the Catalan forges. The loss of oxygen which the air undergoes during this transit is sufficiently explained by the nature of the water which carries it down, and which partially dissolves it.

It was therefore necessary to obtain the water from its source; namely, to introduce empty bottles into the central tube which brings it to the surface.

When boiled, this water gave, per litre, 23 cubic centimetres of gas, containing 22 per cent. of carbonic acid. After the absorption of this by potassa, the gaseous mixture contained:

Nitrogen	.	.	.	.	82.6
Oxygen	.	.	.	.	7.4

This proportion of oxygen being so different from that existing in ordinary water, led me to think that this gas must be still found in it accidentally, and that it probably proceeded from the partial solution of the air contained in the bottles which had plunged into the water. Consequently, I again took some water from the central tube, employing in this instance bottles already filled with carbonic acid. Moreover, I took the precaution to pour this water through a siphon into the matrass in which it was to be boiled, this latter being likewise full of carbonic acid. Not intending this time to pay any attention to the quantity of this latter gas, the water having absorbed a considerable quantity during this various changes of vessel, I used the ordinary apparatus for the air in water, and I received the gas into a graduated eprouvette already containing a solution of potassa. This gas, the volume of which was 14 cubic centimetres at 10° C. (50° F.) per litre of water, was *pure nitrogen*. Neither pyrogallic acid, nor phosphorus, proved the existence of even a trace of oxygen.

This curious result establishes a very marked difference between the water of the well of Grenelle and ordinary fresh water, which all

of them, in contact with the air, contain a considerable proportion of oxygen dissolved in them. With respect to the gases which it contains, this water resembles a mineral water more than fresh water.

The examination of the saline substances left by the evaporation of the Grenelle water, *abstracting the minimum weight of these substances*, shows that this approximation is not as forced as might at first appear. We shall rather be puzzled as to the place to be assigned to it among the various species of mineral waters; for, at the moment that it reaches the surface, it is at once siliceous, ferruginous, alkaline, and sulphurous. Moreover, it has a temperature of 28° C. (82° F.).

According to my analysis, the saline residue when evaporated presents the following composition :

Carbonate of lime	.	.	.	.	40.8
Carbonate of magnesia	.	.	.	.	11.5
Carbonate of potassa	.	.	.	.	14.4
Carbonate of protoxide of iron	.	.	.	.	2.2
Sulphate of soda	.	.	.	.	11.3
Hyposulphate of soda	.	.	.	.	6.4
Chloride of sodium	.	.	.	.	6.4
Silica	.	.	.	.	7.0
					100.0

One litre of water gave me 0.142 of dry residue.

These numbers were furnished by the direct and frequently repeated estimation of each of the elements contained in the solution, the soda excepted. The carbonic acid was determined by the loss which the saline mixture underwent in a small weighed apparatus in which it was put in contact with hydrochloric acid. The potassa was estimated in the state of chloride of platinum and potassium, this salt being isolated from the saline matters which accompany it by means of the saturated solution of this same double chloride. I have in my former memoir explained this method, the accuracy of which I ascertained by fresh synthetical proofs.

The preceding results agree sufficiently, as to the principal salts, with those published sixteen years ago by M. Payen, for us to consider that this water has clearly the same composition that it had in 1841. With M. Payen and MM. Boutron and Henri, I find that the residue from this water has a very decided alkaline reaction, due to the carbonate of potassa. The water itself, when reduced to two-thirds of its volume, reddens litmus paper. In its natural state it turns syrup of violets green; but the water of the Seine presents the same character, in consequence of the carbonate of lime which it holds in solution.

M. Payen first called attention to the presence of silica in this water; he was all the more correct in having done so, as the proportion of this body is now more considerable than he found it to be. After very frequently repeated estimations, I have invariably found 7 per cent. of silica in the residue.

Although very difficult to demonstrate the existence of iron in the water which has remained for a few moments in contact with the air, the ferruginous nature of this water cannot be doubted; indeed it has given rise to a little trade for the keeper of the well, who, having one day forgotten a glass which was in the upper reservoir, found it the next day covered with a layer of a yellow ochre color, and who was struck with the idea of coloring white vases yellow by this means, and then selling them to the numerous visitors to the well. These vases, which are only left in the water for a few hours, take an iridescent tint due to a very thin and very adherent ferruginous deposit. A contact of eight to ten days gives a ferruginous deposit of sufficient thickness to deprive the glass of all transparency.

I must add that the basin which receives the water upon the platform is covered with a large quantity of hydrated oxide of iron, under a gelatinous form, mixed with silica, *confervæ*, and some sandy matters which the water sometimes brings into it. I have ascertained the presence of manganese in this deposit. I have sought for arsenic; but I had too small a quantity of the deposit at my disposal to obtain a definite result. I can only say, therefore, that if this deposit does contain arsenic, this body must be in an extremely minute proportion.

Finally, water taken from the space between the tube and the side of the well, when in phials containing air, soon furnishes a slight yellow deposit on their sides. A very small quantity of air suffices to produce this effect, which is doubtless due to the transformation of the carbonate of protoxide of iron in peroxide of this metal.

I have said that the water of the well at Grenelle is sulphurous. On opening the tap from which the water flows, the odor of hydrosulphuric acid is easily recognised; the quantity of sulphur which it contains is too small to be exactly estimated; but I was able to ascertain the presence of hyposulphite of soda, which is, as is well known, the product of the oxidation, by the air of alkaline sulphuret contained by what are called natural sulphurous waters.

The following experiment showed me the presence of hyposulphite of soda. I several times determined, by means of nitrate of baryta, the quantity of sulphuric acid contained in the water, taking the latter in its natural state, or reduced to half its volume. One litre gives 25 to 27 milligrammes of sulphate of baryta.

On the other hand, on seeking for the quantity of sulphuric acid contained in the residue, calcined and treated with hydrochloric acid (to separate the silica), I obtained, per litre of water, 45 milligrammes of sulphate of baryta. Another estimation, in which the evaporation was made in the presence of nitric acid, so as to transform the whole of the sulphur into sulphuric acid, gave 55 milligrammes of this salt. Thus the residue of this evaporation, when suitably treated, contains a quantity of sulphuric acid, which is nearly double that which originally exists in the water. It appears to me very probable that the sulphur of this excess of sulphuric acid is found in the water in the state of alkaline hyposulphites, as occurs in all those sulphurous waters which are called *degenerated*.

We may therefore conclude from the whole of these experiments, that if the water of the Artesian well of Grenelle is, with respect to its use in houses and manufactories, a water of good quality—it nevertheless shows, in a geological point of view, in consequence of the nature of these matters and of the gases which it holds in solution, some of the characteristics of a mineral water.

According to the opinion of M. Walferdin, the origin of this water is attributed to rain water, which, penetrating into the green sand in the neighbourhood of Troyes, at a height of 125 metres above the level of the sea, has its exit at the bored well of Grenelle.

If this is its origin, it is certainly very interesting to be able to follow the mineralisation of this water, which, although its origin is a soil whose limit is the inferior cretaceous soil, offers some analogy with the composition of the mineral waters found in older soils.

As rain water contains but a very small quantity of carbonic acid, it is likely that the Grenelle water, when penetrating the soil, has removed it from the confined atmosphere which surrounds vegetable earth. It is likewise possible that it traverses layers of earth impregnated with this gas, under the influence of which it dissolves carbonate of lime and carbonate of magnesia. It is also probably to the presence of carbonic acid that we must attribute the presence of silica, which this acid sets free, by decomposing the debris of feldspath which the water, thus charged, meets with in its passage: whence proceeds the carbonate of potassa, which gives the alkaline reaction. As for the nitrogen held in solution in this water, it would proceed from the air which the rain water contained—air, whose oxygen had either oxidised the pyritic products, or destroyed the alkaline sulphuret which, at a certain time, should be found in this water.

With respect to Artesian wells, I may be permitted to draw this conclusion from the studies which I have made—that it is not always desirable that the water of these wells should come from too great a depth; far from this condition ensuring purity, it may happen that the mineralisation of a water may be further advanced when it springs from a lower soil, the solvent and decomposing action of water and carbonic acid augmenting as the temperature of the soil and the aquiferous layer becomes higher.

M. COSTE observed with respect to M. Peligot's communication that the water of Artesian wells, notwithstanding the depth of the sources whence they emanate, and the elevation of their temperature at the moment when they reach the surface, may be rendered fit for all domestic purposes by a very simple means—that of aëration: a means whose efficacy is tested by the respiration of fishes. He last year made an experiment at Val Richer, on M. Guizot's property, which proves how immediate the influence of this aëration is. In a reservoir in which, on leaving the ground, the water is collected, and in which it is constantly renewed from a little spring, not only is it impossible for trout to live, but they are immediately asphyxiated. To give those qualities which were wanting to the water it was only

necessary to raise a certain quantity two metres higher and then allow it to fall into the reservoir as a cascade, thus imitating the plan by which M. Noël, a fisherman of Vosges, proposes to effect the transport of fish to great distances. As soon as this operation commenced, the suffering trout regained their powers, and lived in it as long as the experiment lasted.

All that would be necessary, therefore, to give to the water of Artesian wells the qualities in which they are deficient, or even to destroy some injurious properties, would be to arrange at the points when they rise certain means of aëration, by causing them to fall from point to point to the place of their destination. They would thus be completely purified, unless they contain deleterious principles given by the mineral layers through which they pass.

Comptes Rendus.

On the CHEMICAL PROPERTIES of ALUMINIUM, and the VARIATIONS of its AFFINITIES at DIFFERENT TEMPERATURES.

By M. H. SAINTE-CLAIRE DEVILLE.

It is very difficult to determine the place which aluminium ought to hold in chemical classification. In the memoirs which I have published on this substance, I have placed aluminium by the side of iron and chrome, starting from the principles on which M. Thénard arranged his groups of metals. The experiments which I have since made on silicium, which has many analogies with aluminium, have modified my opinions on this point, and have led me to seek in a peculiar manner for the chemical properties of this metal, especially in relation to the various temperatures at which they manifest themselves. MM. Gay-Lussac and Thénard, in their beautiful experiment on the preparation of potassium by means of potassa and iron, have already shown that at a white heat iron behaves like an alkaline metal, superior even to potassium as to its affinity for oxygen. A certain number of facts of this description, experiments tried in powerfully-heated apparatus, have enabled me to show that the alteration of affinities with temperatures presents itself in many cases. Aluminium gave me some curious examples of this.

Aluminium, at a low temperature, behaves like a metal which is susceptible of producing a feeble base; consequently, its resistance to acids, hydrochloric acid excepted, is in proportion to the feebleness of this base. At the ordinary temperature, and in the presence of water, the acid tendency of alumina is greater; aluminium likewise produces powerful reactions only when in the presence of powerful bases, such as potassa and soda dissolved in water. Nevertheless, this affinity is still insufficient to determine the decomposition of water by aluminium in monohydrated potassa at the temperature of its fusion. With the help of a very elevated temperature, I have no doubt that potassium could be formed with this mixture, if we could place ourselves in the same conditions as in MM. Gay-Lussac and Thénard's experiments.

Unfortunately, we have no vessels which will at all answer to attempt their realisation with aluminium.

As aluminium forms no protoxide, consequently has no tendency to form a saline oxide $Al^3 O^4$, its resistance to oxidation at very high temperatures and its unalterability in the air are extremely great, as both M. Wöhler and myself have shown. This is why steam attacks it so slightly when it is pure. All these reactions are likewise common to silicium.

At a moderately low temperature, and without the addition of water, alumina still plays the part of a feeble base, so that aluminium reduces no protoxide below bright redness. The metal alloyed with lead may be purified in a muffle, and cupelled, as shown by M. Peligot. Cupreous aluminium blackens in the same circumstances, in consequence of the oxidation of the copper. But at a higher temperature everything is changed: the alumina becomes an acid, and aluminium takes, only with less force, all the properties of silicium; it decomposes the oxides of lead and copper with the production of aluminates. This fact, which I have often observed, appears to be opposed to what has hitherto been known upon this subject, although it has likewise been observed by MM. Tissier. Some new considerations have guided me in the numerous experiments which I have already made and which I am still carrying on.

Aluminium does not oxidise in nitre—at least not without the heat being sufficiently great to decompose the nitre itself. It has then become alkaline, and the temperature is sufficiently elevated for the change of affinities to take place: the phenomenon of inflammation which accompanies it indicates a powerful reaction. Aluminium is every day infused in nitre to purify it in the midst of a powerful disengagement of oxygen, and at a red heat, without there being any cause for fear; but this experiment must never be attempted in an earthen crucible. The silica of the crucible is dissolved by the potassa, the glass thus formed is decomposed by the aluminium, and thenceforth the siliciuret of aluminium presents peculiar properties. Its oxidation, which produces a silicate of alumina, is effected with extraordinary vehemence. The following is a lecture experiment, which I have shown for two years at the Sorbonne, and always successfully. Place on a plate a little pounded glass, well heaped together, and forming a cupel, in the centre of which is placed a piece of aluminium. With the flame of the oxyhydrogen lamp the temperature of the aluminium may be brought to a great height without sensibly oxidising it; the glass is then melted and put upon the aluminium, so as to bring them in contact at a high temperature. An alloy of aluminium and silicium is formed, and, when with the oxyhydrogen lamp we uncover the metallic bath, it burns with great brilliancy, throwing out white sparks in the manner of silicium. In fact, the alkaline borates and silicates are very easily decomposed by aluminium. The metal then dissolves considerable quantities of silicium, and smaller ones of boron, which may be extracted from it, as M. Wöhler and I showed long ago. The same observation has

been made by M. Wöhler with respect to carbonate of potassa, which aluminium decomposes with a deposit of charcoal. In this respect it still more nearly resembles silicium, which has no action on nitre, as Berzelius demonstrated.

It is evident that, in experiments of this nature, it is useful to take account of all the conditions in which we operate, as well as the nature of the vessels which we employ, and the purity of the substances. I would recommend all who wish to study aluminium to ascertain at first the nature of the metal on which they act, as it is sold of very various degrees of purity, as is the case with most of the commoner metals.

Comptes Rendus.

On BORON, its ANALYSIS and PHYSICAL PROPERTIES.

By M.M. WOHLER and H. SAINTE-CLAIRE DEVILLE.

WE have already published a new method of preparation which enables us to obtain boron, under the distinct forms: graphitoid boron, and adamantoid boron, or boron diamond. From this time, we have prepared many samples of crystallised boron, and examined its physical properties with the greatest care, analysing the substances on which we have operated each time. We shall now give the result of these experiments.

Boron, as we have already mentioned, exists of very different colors, from so deep a garnet red as to produce opacity even under slight pressure, to so pale a yellow as to be nearly colorless. We have analysed this substance in its various states, and we have always found that its composition changes a little with its color. We have now three distinct varieties which appear to possess the crystalline form, at least judging by some angles which have been measured among them. But one of these varieties was obtained in such clear and reflecting crystals, that the angles may be determined with the greatest precision; the crystalline form of boron is the right prism with a square base whose parameters, calculated with the inclinations of the facettes of the most developed octohedron of the crystal, are in the relation of 1 for the horizontal axes to 0.816 for the vertical axis. The forms found in it are two octohedra (111), (221), supported upon the base, the facettes (110) of the prism, and (100) of a second prism, whose facettes form tangents with the feather edges of the first. The angles of these facettes permit us to consider boron as entirely isomorphous with tin. We owe this remark to M. Sella, who in a letter to M. de Senarmont observes that "M. Govi, who is going to Florence as Professor of Physics, had with him some of the boron of MM. Wöhler and Deville. I have measured three small crystals whose largest dimensions were between $\frac{1}{3}$ and $\frac{1}{4}$ of a millimetre." These crystals belong to the prism with a square base. This is curious; for tin is the only metal which is not cubical or rhomboïdal among those crystals with whose crystallisation we are acquainted. But what is

more remarkable is that boron is completely isomorphous with tin. Indeed I have found in the angles of M. Miller by changing his notation a little, that the crystals of tin are composed the same as those of the forms (100), (110), (221), and that 110 to $221=31^{\circ} 26'$. Only the most developed facette of the tin is here wanting, and its symbol would be (332) in relation to that of boron.

The angles which we have found (normal angles) are—

	According to us.	Calculated.	M. Sella.
110 to 221 . . .	$31^{\circ} 29'$		$31^{\circ} 33'$
331 to 111 . . .	$19^{\circ} 36'$		$19^{\circ} 27'$
The adjacent facettes of the octohedron (111)	$77^{\circ} 50'$	$77^{\circ} 50'$	
The alternate facettes			53°
The facettes of the two adjacent prisms 110 to 100,			45°
The alternate facettes	90°		

The color of the crystal measured, was a deep garnet, but it was transparent.

The density of boron is 2.68, that is a little above that of silicium. It will be observed that the density of silicium is equal to the density of silica; that the density of boron is decidedly superior to the density of boracic acid; finally, that the density of the diamond is very great in comparison to that of liquid carbonic acid. To mention here an approximation which further experiments may render certain, we will just remark that before silicium comes aluminium, whose density scarcely reaches two-thirds of that of corundum.

The hardness of boron varies very much, always remaining much superior to that of corundum. On this point the three varieties of crystals of which we have spoken are very distinguishable.

I. The boron is in lamellæ with a metallic lustre at least equal to the lustre of the diamond; it appears black and opaque, but is nevertheless transparent in the thinnest parts of the crystal. This boron is very cleavable, which renders the crystals very fragile; but it is very hard; it distinctly marks the diamond. We have the honor of presenting to the Academy a diamond with natural facettes of extreme hardness, which diamond powder itself acts upon very slowly. This diamond has been polished with boron; on the edges of the octohedron, which at first presented a groove with two salient edges, it will be observed that the salient edges have disappeared, and that in several places, the groove itself is completely effaced. M. Quillot, engraver on stones, who has made these experiments in his workroom and has watched them attentively, has informed us that boron, although acting upon the diamond, acted more slowly than the diamond itself, and at last, after a certain time, the tool on which the powdered boron was placed became sticky, which was an indication of less hardness than the diamond. This variety of boron is produced every time that boracic acid and aluminium are left in contact for some little time, or when the operation is conducted at a low temperature. These

conditions are however, hardly determined in a sufficiently definitive manner.

This boron is composed of—

Carbon	.	.	.	.	.	.	.	.	2.4
Boron	.	.	.	.	.	.	.	.	97.6
									100.0

The analysis of boron is a delicate operation which presented some difficulties. The following is the process which we adopted: the boron, weighed and introduced into a platinum boat was burned in a long tube of Bohemian glass heated where the boat is, namely, close to where the chlorine reaches, to such a temperature as to soften the glass. Fuming chloride of boron is lost, and charcoal remains, which is weighed and burned in oxygen, collecting the carbonic acid; the carbon frequently remains in the form of the crystals of boron, such as they were when put into the boat. In this operation there is always formed a small quantity of a white, slightly yellowish sublimate, which heats in contact with water and dissolves in it almost completely, especially after some time. Chloride of sulphur is found in it, proceeding from the action of the chlorine on the vulcanised caoutchouc and boracic acid, whose oxygen is furnished by the current of chlorine which always brings some to it, whether in consequence of the air in the apparatus, or proceeding from the action of the hydrochloric acid on the manganese, or from the humidity of the gas, which it is very difficult to dry by means of the apparatus generally employed. Under this influence is formed a volatile substance, solid and decomposable by water into hydrochloric and boracic acids, of which considerable quantities are obtained during the preparation of chloride of boron; we purpose to examine its nature and composition.

The sublimate may likewise contain aluminium. To seek for this its solution is evaporated almost to dryness, and a little hydrofluoric acid of fluoride of sodium (and better of hydrofluoric acid of ammonia) and an excess of sulphuric acid. The evaporation is continued until the sulphuric acid becomes a vapor. It is then taken up again with water. The filtered solution leaves a little sandy matter, proceeding either from the silica, from the fluoride of sodium, or even from the boron, which might have contained a little silicium when treated with ammonia and brought to ebullition, it did not give us alumina with the samples of the variety of boron of which we are just speaking. When alumina exists in this liquor, it is estimated by known processes.

II. Boron is likewise seen under the form of perfectly limpid transparent crystals. They are grouped under the form of long and slanting prisms, so as to represent the teeth of a saw. Sometimes we have obtained very small ones, which are really prismatic with eight facettes terminated doubtless by the octohedra of which we have given the form above. Their diamond-like lustre is extreme; but their hardness is rather less than the first variety. The prolonged

action of acids, and even of *aqua regia* takes no effect on their surface. These crystals are obtained when an excess of aluminium and boracic acid are kept in contact in a charcoal crucible at a high temperature, and for a long time. It requires at least five hours at the temperature of fusing nickel. Few crucibles resist this test.

The composition of this boron is very variable. The following analysis will give an idea of the average proportions of the substances forming it. The analysis was made of a very beautiful sample of picked crystals:—

Carbon	4.2
Aluminium	6.7
Boron	89.1
	100.0

If we can succeed in producing larger and unspotted crystals of this substance, it would certainly be employed in jewellery.

III. The hardest of all the varieties of boron, incomparably harder than the first, is obtained by exhausting by several operations the action of boracic acid in great excess on aluminium, and at such a temperature that all the boracic acid is very rapidly volatilised. For this reason to obtain one or two grammes of this matter, we are obliged to volatilise in closed vessels, in the charcoal apparatus which we have already described, twenty or thirty grammes of boracic acid, heating each time for two or three hours. A cavernous mass then remains in the crucible, of a light reddish chocolate color, in every respect similar to that kind of diamond termed *bour*, set with crystals of boron of great brilliancy and from which iron or foreign metals and a little aluminium have to be removed with soda and hydrochloric acid. Unfortunately the boron is not so easily separated from the alumina with which it is impregnated, and whose presence is easily recognised. For this reason we are unable to give an analysis of it, although this matter appears to us evidently the purest of the three varieties. It will be remarked that alumina, in the presence of the chlorine and charcoal which contain the boron, and perhaps the boron itself, may give oxide of carbon and chloride of aluminium. It was necessary to take great care in separating, before all our analyses, the aluminium from the boron, by carefully picking the crystals, to remove this cause of error as far as possible.

This mass of boron appeared in the microscope to be composed entirely of small crystals; to the naked eye we saw them very distinctly, although so extremely small that it was impossible to measure them. This matter is so excessively hard that M. Guillot is of opinion that it equals the diamond, and after being used for polishing, it is found as finely divided as it was previously, which appears to be a characteristic of the finest diamond powder. It crushes with extreme difficulty, thus showing the greatest similarity to that variety of diamond which lapidaries term *bour*.

We must now observe upon the manner in which the above analyses should be interpreted.

In the first place the carbon mentioned should evidently be considered as in the state of diamond. For according to all our analyses the greater the quantity of carbon contained the more perfect is the transparency; and it is well known that a few thousandths of black carbon are sufficient to color of a very deep tint glasses in which we cannot suppose that the charcoal combines with the matter which it colors. We are besides obliged to admit that the carbon has crystallised with the boron, the form of which it does not possess. This hypothesis is not contradictory of the facts which have been observed in some cases when we see a matter whose proportion is dominant, impose its form on substances with which it has some analogy in chemical properties. Moreover, nothing has even proved that the diamond, like many other bodies of the same nature, is not itself dimorphous and capable, in some as yet unknown circumstances, of assuming the form of boron. The seleniuretted sulphur obtained artificially with solutions of selenium and sulphur in sulphuret of carbon, is a proof of this. The sulphur then removes, operating with certain precautions, very small quantities of selenium, necessarily small in consequence of the sparing solubility of the latter; but the presence of selenium, which has no resemblance in form to sulphur, may easily be demonstrated by qualitative analysis in the seleniuretted sulphur, whose angles when measured were identical with those which M. Mitscherlich has assigned to octohedral sulphur.

Moreover, the conditions of the isomorphism of the simple bodies and of their mutual attraction during crystallisation, require to be studied experimentally on the small number of these bodies which are sufficiently approximated in the classifications of science for their combinations not to obey the law of equivalents; that is to say, whose contact will produce nothing but a solution. In this case carbon, boron and silicium, which approximate so nearly, might mutually dissolve each other without combining, and coëxist in crystallised boron without changing its form. The contrary is the case when silver, which is so near lead, is dissolved in lead. It is well known, (and the method of separation of the two metals by crystallisation is founded on this fact) that lead crystallises without attracting any notable proportion of silver. They separate from each other as an anhydrous salt separates from an aqueous solution in the state of saturation.

These observations apply to aluminium, the presence of which in boron in very variable quantities (from 0 to 13 per cent.) never indicates a combination: for the formula AlBr^7 requires nearly 20 per cent. of aluminium. This new fact may aid, we hope, in the determination of the conditions of isomorphism among the simple bodies; but it may give a certain support to an opinion which one of us has already expressed, according to which, aluminium should be placed in the series of carbon and boron, in the same rank as antimony in the series of nitrogen and phosphorus. This is an application of the method of parallelism which has already been of great use in the natural sciences.

Comptes Rendus.

On MAGNESIUM, its PREPARATION and VOLATILISATION.

By MM. H. SAINTE-CLAIRE DEVILLE and CARON.

THE chemical and physical properties of magnesium are very interesting with respect to the place which this intermediary metal should hold in actual classification—such as it has been arranged by M. Thénard.

The chemical properties of magnesium were determined with extreme perfection by M. Bussy, to whom the discovery of this metal is due, and all the experiments which we have made on masses of metal such as it would have been impossible to procure when M. Bussy carried on his operation, have confirmed the results which he introduced into science. It has been the same with respect to M. Bunsen, who has had comparatively large quantities of magnesium at his disposal.

There is, however, one physical property of this metal which has not been ascertained and to which we now wish to call the attention of the Academy; it is a new fact which still more nearly approximates zinc and magnesium, and gives them another property in common. Magnesium is volatile, like zinc, and at nearly the same temperature. We have easily distilled thirty grammes in the charcoal apparatus which one of us has already described. When the magnesium is pure it leaves no residue, and the sublimed metal is white, surrounded with a small quantity of oxide. When impure, it leaves in the charcoal boat a certain quantity of a very light and very complex matter, to the nature of which we shall return, and then the distilled magnesium is covered with small colorless and transparent needles, which are very rapidly destroyed, being converted into ammonia and magnesia, which indicates for magnesium the probable existence of a nitrate analogous to those remarkable bodies which M. Wöhler and M. H. Rose have already discovered for a certain number of simple bodies.

Magnesium fuses at very nearly the same temperature as zinc. At a little higher it inflames, producing a brilliant flame, in the midst of which are seen from time to time sprays of indigo blue, especially when jets of oxygen are thrown into the metallic bath. The combustion of magnesium is accompanied by all the phenomena observed with zinc, and which denote a volatile metal, whose oxide is fixed and infusible: a brilliant flame, a deposit of magnesian *pompholix*, and rapid combustion.

We have found that the density of magnesium is equal to 1.75. We are now examining carefully its malleability, its ductility, and the physical properties depending on them. Finally, we shall give the cypher of its electrical conductivity, a very interesting determination because of the excessive lightness of the metal. Magnesium files very well and burnishes beautifully. It alters very little in contact with air, when pure; and when the surface is polished, comparable in this respect also with zinc, which however resists a little better.

To prepare magnesium we use a process already in use for aluminium, but which requires a little modification when applied to mag-

nesium, as this metal is lighter than the scoria in which it is produced.

The chloride of magnesium is most carefully prepared by the ordinary process: 600 grammes are mixed with 100 grammes of fused common salt (or preferably, the mixture of salt and chloride of potassium of M. Wöhler) and 100 grammes of pure fluoride of calcium, all previously pulverised. 100 grammes of sodium in pieces are then added and mixed intimately with the powdered chloride, and the whole is thrown with a small iron shovel into a red hot earthen crucible, which is immediately covered with its lid. After some time the reaction is perceived. When all noise has ceased, the crucible is uncovered, the mixture is stirred with an iron rod until all the fused parts are homogeneous, and the upper parts of the bath well uncovered, the globules of magnesium then show themselves; the crucible is then allowed to cool away from the fire, and when the saline mass is about to set, it is stirred again, and the little metallic masses are collected with the iron rod so as to form only one, and the whole is poured on a shovel or sheet of iron. On breaking the scoria the globules of magnesium are found and removed. The scoria may be fused once or even twice more; a little magnesium may be removed each time; 600 grammes of chloride of magnesium reacting on 100 grammes of sodium gave us 45 grammes of magnesium.

The crude magnesium is introduced into a charcoal boat itself enclosed in a tube of charcoal and heated to bright redness, almost white, while a slow current of hydrogen passes through the apparatus. The tube being slanted very much in a furnace, all the magnesium condenses in front of the boat, and is easily collected when the tube is cold.

It is then fused in a mixture of chloride of magnesium, common salt, and fluoride of calcium. By slightly augmenting the proportion of the latter which is added gradually to the bath in fusion the scoria is rendered less fusible than the magnesium, so that the magnesium can be poured off when the scoria is just becoming a mass.

When the magnesium is distilled and the current of hydrogen is rapid, a little very fine metallic powder is carried a long way and remains in suspension in the hydrogen which issues from the apparatus. If the gas is then inflamed, one of the most beautiful flames imaginable is produced. This forms a charming lecture experiment.

Some experiments were commenced some time ago at the Ecole Normale to operate the reduction of the earthy alkalies by means of charcoal. The results will very shortly be laid before the Academy.

Comptes Rendus.

INDUSTRIAL CHEMISTRY.

REPORT on a MEMOIR entitled—CHEMICAL RESEARCHES on WHEAT, its FLOUR and PANIFICATION. By MÈGE MOURIÈS.

By MM. DUMAS, PELOUZE, PAYEN, PELIGOT, and CHEVREUL, Reporter.

ON the 9th of June, 1856, M. Mège Mouriès presented to the Academy some investigations on panification, which were partly theoretic and partly practical: partly theoretic, because the author describes the real cause of the coloration of brown bread, and gives a means of preventing the effect, even when the bran remains in the paste of the bread; partly practical, because he proposes a new process of panification; and this process is not merely in the state of a simple project, because one of the Colleges of Paris has used the new bread during the last three months, and that since the month of June, 1856, the Orphanage of St. Charles of the twelfth arrondissement has used it uninterruptedly.

The researches of M. Mège Mouriès have two claims upon the attention of the Academy—their scientific object and the importance which the Academy has always accorded to all those subjects which interest the well being of society. The question is, however, one of a purely practical nature—namely, the yield of flour from wheat and the yield of bread from flour.

The Committee appointed by the Academy to examine the plans of M. Mège Mouriès, after having watched the operations performed at a small bakery, considered that it would not be doing its duty by the Academy, the public, and the author who was subjected to their examination, unless they were sure that they had a method of proceeding which should be free from causes of error. In consequence, it requested of the Prefect of the Seine the means of making experiments at the bakery of the Hospices of Paris. This request was instantly granted by the Prefect, and the experiment met with every facilitation from M. Salone, the Director of the Boulangerie de Scipion, where three operations were made in the presence of M. Salone, under the direction of M. Mège Mouriès.

We shall speak first of that part of the investigations of M. Mège Mouriès which belong to science, properly so called; we will then speak of the practical application of these investigations.

I.—FIRST PART.—*The Investigations of M. Mège Mouriès in a Theoretical Point of View.*

Three years ago M. Mège Mouriès published a work on bran bread, in which he mentioned having found under the pericarp, in the internal part of the perisperm of the grain, an active principle or ferment, which he has since called *cerealine*; this principle, although belonging to the corn, properly so called, is found entire, or nearly entire, in the bran, and not in flour of the first quality, which is exclusively used in Paris in the preparation of white bread. M.

Mège Mouriès found that the solution of cerealine prepared with bran and water, at a temperature below 50° C. (122° F.), had the property of liquefying starch in the manner of diastase, and he attributed to the presence of cerealine in the paste of the bran bread the property of giving a less consistent crumb than that of white bread, because so much starch had been modified into a soluble matter under the influence of the cerealine. This action of the cerealine, then, appeared to medical men who prescribed bran bread in certain diseases as explanatory of its being easier of digestion than white bread.

M. Mège Mouriès, by continuing his labors on bran bread and white bread, has arrived at the remarkable results which we are about to describe. It was always supposed that brown bread owed its color to the bran, and on reflection how can we attribute it to any other cause, when we daily see that *brown bread* is made with flour containing the bran, whereas white bread is made from flour which has been deprived of it? Nevertheless this reasoning has led to an erroneous conclusion, as will be shown by the demonstration of the two facts which we shall explain, after having given an idea of the difference existing between the process of panification practised in bakeries for making the Parisian white bread and the new process of M. Mège Mouriès.

Ordinary or Old Process of Panification used in Paris.

The Parisian white bread is made with flour of the first quality. If 100 parts of corn gave 70 of flour of the first quality it was said to be flour bolted to 70. The rest of the corn may be composed of 10 parts large, middling, and small bran; 20 of brown flour containing 8 of small bran; and 17 of white flour.

The following is the process :—

	kilos.
1st. At eight in the evening a paste is made of 8 kilos. of flour and 4 kilos. of water	12'000
It is left alone until six in the morning.	
2nd. To this is added 8 kilogrammes of flour and 4 kilos. of water	12'000
3rd. At two in the afternoon 16 kilos. of flour and 8 kilos. of water are added	24'000
4th. At five o'clock are added 100 kilos. of flour and 52 kilos. of water containing from 200 to 300 grammes of yeast	152'200
Total	200'200
5th. At 7 o'clock 132 kilos. of flour are added and 68 kilogrammes of water containing from 300 to 600 grammes of yeast, and two kilos. of common salt; it is kneaded to form paste	402'000

With this quantity are made five or six batches of bread by operating in the following manner :—

1st Batch.—It is composed of half of the preceding paste which is divided, put into pans to rise; after which it is put into the oven.

The bread of the first batch is sour, slightly brown, and close in texture.

2nd Batch.—Half the dough remaining from the first batch is mixed with 132 kilos. of flour and about 68 kilos. of water containing 300 to 600 grammes of yeast, and 2 kilos. of salt. The second batch is only composed of half this paste. The bread is whiter and better than that of the first batch.

3rd Batch.—Half the paste remaining from the second batch is mixed with 132 kilos. of flour and 68 kilos. of water containing 300 grammes of yeast and 2 kilos. of salt. The third batch is only formed of half this paste.

4th Batch.—It is prepared as above.

5th Batch.—It is prepared as above. It gives the finest kinds of bread.

New Process of Panification of M. Mège Mouriès.

We will suppose that 100 parts of ground flour give

72·720 of the finest flour

15·720 of brown flour

15·560 of bran.

1st. At six o'clock in the evening put 70 grammes of pure yeast, or 700 grammes of common yeast, into 40 litres of water at 22° C. (71° F.), and 100 grammes of glucose. The temperature of the place in which it is kept should be about 22° C. (71° F.)

2nd. At six o'clock in the morning, the liquid is saturated with carbonic acid gas. We shall see the effect of this solution on the cerealine further on.

With this we moisten the 15·720 kilos. of brown flour. Fermentation commences immediately.

3rd. At 2 o'clock in the afternoon, add 30 litres of water, and strain through a silk or silver sieve to separate the medium and small bran contained by the brown flour.

This bran requires, to be deprived of farinaceous water, 30 litres of water and a second straining. This water containing 1·800 kilos. of flour is used in the following operation.

4th. The 70 litres with which the flour has been treated, after passing through the sieve give about 55 litres with which the 72·720 kilos. of white flour is made into paste, after the addition of 700 grammes of common salt.

The dough is put into pans to ferment.

5th. It is put into the oven.

The bread made by M. Mège Mouriès consequently contains exactly

72·720 kilos. of white flour and 12·720 produce of brown flour.

M. Mège Mouriès has not always used glucose. Thus, at one time

he added to 40 litres of water from the Seine 26 grammes of tartaric acid, and to the 30 litres of water which he added afterwards he added 20 more; he recommended the addition of a larger quantity when the water was richer in sub-carbonate than that of the Seine. In this respect, he was guided by litmus paper, which became distinctly reddened. Citric acid and even vinegar produce the same effect.

Many persons having expressed to M. Mège Mouriès that an acid added to bread might be injurious, or if not really so, might occasion prejudices against using it, he was led to modify his process and give up the use of any acid.

We shall return to the differences between the old and new process. We have now to demonstrate that the color of brown bread is not produced by the bran as had always been supposed, but that it is the result of the process commonly used.

1st Fact.—In producing panification by the process of M. Mège Mouriès with flour which contains from 2 to 5 parts of bran which has not been separated from the water with which the flour was washed when straining it through the silk or silver sieve, we have a bread with a very slight orange tinge, which is quite distinct from the color of brown bread obtained by the common process. When examined through a microscope, or even with the naked eye, the crumb of the bread prepared by the new process will be found to contain pellicles of bran disseminated through the white crumb, which gives the orange tinge of which we speak.

2nd Fact.—The counter proof of the preceding experiment may be found in the following fact, mentioned to the writer by Lieutenant Colonel Favé.

A foreigner proposed to His Majesty the Emperor to purchase a process by means of which he said that the whole of the wheat flour might be manufactured into white bread. The process consisted in removing the colored pellicle from the grain—that is to say the pericarp; all the coloring part being thus excluded, it appeared reasonable to suppose that the bread would be white. Now, this opinion was disproved by the experiment made in the presence of MM. Favé and Salone. The bread was brown, much to the astonishment of those gentlemen and of the inventor himself.

Consequently, as M. Mège Mouriès has said, the color of brown bread is not due to the presence of bran in the flour, but to the process of panification, because he makes by his process white bread with flour containing the bran; and by the old process brown bread may be made of flour which contains no bran, as occurred on the above occasion.

We shall now describe how M. Mège Mouriès arrived at this conclusion.

He first made so accurate a study of the structure of the grain of wheat without the assistance of a microscope that his results were fully confirmed by the microscopical examination of the grain made by M. Trécul, who is now in the United States.

The grain of wheat is composed of the *pericarp* and the *grain*, properly so called.

A.—Pericarp.—The pericarp is composed of three parts.

1st. The External Part.—It is colorless and contains no cellulæ; this is the *epicarp* of M. Mège Mouriès and the *cuticle* of M. Trécul.

2nd. The Medium Part.—This is formed of cellulæ, colored yellow; M. Mège Mouriès calls it *sarcocarp*.

3rd. The Internal Part.—Like the preceding it is formed of cellulæ; both these observers call it *endocarp*.

B.—Grain properly so called.—It is composed of two envelopes; the *testa* and the *internal membrane*; the *perisperm* or *albumen* and the *embryo*.

MM. Mège Mouriès and Trécul agree in every respect as to the anatomical composition of the bran.

This proceeds from the rupture or tearing, by bruising or pressure, of the *pericarp*, to which adhere the *two envelopes of the grain*, with the large external cellulæ of the perisperm and some cellulæ underneath containing globules of starch.

Both observers agree that the large external cellulæ of the perisperm do not contain starch. According to M. Mège Mouriès, they contain principally *cerealine* and *vegetable caseine*. The *gluten* and *starch* are below.

M. Payen has observed similar facts to those mentioned by M. Trécul.

This explanation of the anatomical composition of the grain will render clearer how M. Mège Mouriès has regarded panification in a chemical point of view.

He admits the proximate composition of wheat as given by chemists, except that he has been led by his own experiments to ascertain the existence of a principle which he calls *cerealine*, and which, like *vegetable caseine* and *gluten*, acquires the quality of a ferment by a slight modification, perhaps due to contact with the air. These three principles are nitrogenous.

Cerealine.—It is soluble in water and insoluble in alcohol. It acts as a ferment on starch, dextrine, glucose, and cane sugar.

Its aqueous solution loses its activity at a temperature above 60° C. (140° F.); and when precipitated by concentrated alcohol or by acids, even carbonic.

A liquid formed of 9 parts of water and 1 of alcohol precipitates without destroying its activity.

Diastase loses its action at 90° to 100° C. (194 to 212° F.); in this respect it differs from cerealine.

Cerealine changes starch paste into dextrine, dextrine into glucose, and glucose into lactic acid, and even into butyric acid, when the action is much prolonged.

When starch is in globules and in water, the action of the cerealine will not commence below 50° C. (122° F.)

Cerealine, in reacting on starch, not giving carbonic acid gas, is incapable of raising the dough, and it would act alone in panification.

It gives to *milk of bran* the property of becoming sour, and coloring under the influence of the air.

It alters gluten extremely; this, as well as other products, gives ammonia, a matter whose brown color recalls the appearance of those matters called *ulmine*, and a saccharine product capable of changing sugar into lactic acid.

Caseine.—Caseine, like cerealine, is nitrogenous, soluble in water, and insoluble in alcohol; it is precipitable by acids.

Although it may be said to be without action on starch in the circumstances in which cerealine acts, still it would not be correct to say that it is absolutely inert, for with time it would convert it into dextrine, glucose, and lactic acid.

Gluten.—Gluten left for some time becomes a ferment, capable of changing starch into dextrine, this into glucose, and this into alcohol and carbonic acid.

We shall now give M. Mège Mouriès' views of the differences of the action in the old and new processes of panification.

(To be continued.)

On the MANUFACTURE of the CHINESE GREEN DYE, called LO-KAO.

By the Rev. M. HELOT, Missionary of the Company of Jesus.

On the 6th of August, 1856, I received through the Rev. Father Borgniet, Apostolic Pro-vicar of the Mission in Kiang-nan (China) a note addressed by the President of the Central Council for the Propagation of the Faith, of Lyons, on the subject of a certain Green Dye said to be used by the Chinese. A request accompanied it, that I would omit no steps in order to obtain a prompt and exact reply to the inquiries it contained.

Setting aside, therefore, all other engagements, I commenced my investigation. At Shanghai I could obtain no positive information; not only is the dye in question not prepared there, but it is not even found there in commerce; when Europeans inquire for it through their *campradors*, the latter obtain it from Soo-chow, and sell it at an exorbitant price.

I was, however, fortunate in discovering that the Green Dye was prepared on a considerable scale at Azé, a town of from 6000 to 8000 souls, situated eight leagues to the south of Kia-hin-fou, in the province of Tche-kiang. The Reverend Pro-vicar at once determined that I should proceed thither. A large Christian community of Tche-kiang located near Azé, facilitated my expedition, which would otherwise have been rather hazardous. M. Edan, French Consul at Shanghai, had the kindness to give me for a companion one of his most intelligent *employés*, one who had resided not far from Azé, and who had been formerly a pupil of the Reverend Fathers, the Lazarists of Tche-kiang. The worthy Christians received me with the utmost cordiality, and omitted nothing to put me in the way of obtaining the desired information. They would, in fact, at once have conducted me to the

workshops where the manufacture was carried on, but at that season the process was not in operation. They therefore sent for the chief workmen engaged in the manufacture of the dye, and I interrogated them separately, and thus obtained an acquaintance, at least tolerably definite, as to the preparation of this valuable color. This I hasten to lay before the President of the Central Council; if it considers it as still uncertain and insufficient, it will be desirable to pay another visit to Azé during the month of November;—or it would perhaps be still better to cause two or three workmen to come to Shanghai, and there and then to prepare the dye; some workmen of Azé, friends of the Christians there, have in fact offered to go.

The following is the note which was received from the Central Council of Lyons:—

Note to be transmitted to the Missionaries in China.

"There occurs in trade at Shanghai, in China, a green substance resembling Javanese indigo, but not really indigo. This substance, which is called *Lo-kao*, is not prepared at Shanghai, but in some other part of China, which we know not. It is said that this green coloring matter is in China very scarce and dear. It is employed, nevertheless, in dyeing the coarsest cotton cloths, which appears a striking contradiction.

"It would be very important for the manufacturers of Lyons to obtain some precise information on the following questions:—

"1. What is the price of the *Lo-kao* at Shanghai?

"2. What is the plant, and what is the part of the plant that yields this green dye?

"3. What are the processes employed by the Chinese for extracting the color from the plant, and for employing it in dyeing?

"4. Could one obtain with certainty the seeds of the plant whence the *Lo-kao* is extracted, as well as information regarding its culture and the situation most favorable for it?

"We are aware that the points upon which we request information are numerous, and that they may be obtained with difficulty; but it being a question of obtaining a substance which would be of great service to our manufacturers, a small amount of information even would be useful.

"The seed of a shrub, said to be that which furnishes the *Lo-kao*, has been received, and the shrub itself is cultivated; but as there is still some uncertainty as to its origin, it is very important to obtain it afresh, so that we may more confidently persevere in those labors which will lead to the desired result."

To commence my history:—About the years 1848 to 1850, specimens of some Chinese productions were sent to the Minister of Commerce; among them some cloth of a sea-green color attracted attention. It was placed in the hands of some skilful chemists, who

were astonished to find that they could not detect in it by reagents, either a blue or a yellow color. Hence they inferred that its color was due to some green coloring principle unknown in Europe. Traces of alumina and of iron were all that could be detected. As at this period we had no consul in China, the American Consul at Canton was addressed for more ample information. He could procure only about fifteen grains of the desired material, but the experiments made upon this small quantity gave the most satisfactory results. Recourse was again had to the American Consul, who succeeded this time in sending a pound of the dye at a price of 85 dollars, promising also to write to Shanghai for information about it, since it appeared not to be prepared in the south of China. M. de Montigny, who had just been appointed French Consul at Shanghai, now received from the Minister instructions to pay attention to the matter. Assisted by the Missionaries both of Kiang-nan and Tche-kiang, he succeeded in procuring some ounces of the Green Dye; but as it was necessary, in default of other information, to obtain it from Soo-chow-foo, its rarity and dearness were exaggerated by the dealers, and it had to be paid for at the rate of 10 dollars per oz.

About the year 1854, M. de Montigny sent seeds of the trees which furnish the *Lo-kao*, and two glass cases containing from two to three hundred plants, the whole obtained from authentic localities, through the intervention of the Lazarists of Tche-kiang.

Such was the position of the question, when the Reverend Apostolic Pro-vicar commissioned me to investigate the subject, and the following is the information I have obtained:—

1. The trees which yield the *Lo-kao* belong to two species. At the period of the fall of the leaf, it is collected by the peasants in the districts where the dye is manufactured, and consists of fagots of small wood, known by the name of *Lo-za* (green-branch). The *Lo-za* is of two species, or it may be but two varieties of the same species: the one, which comes from the sterile mountains in the S.W. of Tche-kiang and Shantung, is called *Pa-bi Lo-za*; the other, which forms a bush, also uncultivated, occurs in the fertile plains in the neighbourhood of Azé in Tche-kiang, and is named *Hom-bi Lo-za*. At Azé the fagot of 100 pounds of *Hom-bi Lo-za* costs 1000 *sapercs*, that is, about 5 francs; while a similar fagot of *Pa-bi Lo-za* costs about three times that sum. I may remark that the name *Lo-za*, which is given to the shrub in Tche-kiang, is not its correct appellation, but only that of the branches made into fagots, and found as an article of trade for preparing the dye; for the word *za*, which signifies *fagot* or *small wood*, is employed to express any wood used for fuel. The people of Shantung, who also have this shrub on their mountains, say that it is called *Lieu-lo-chou* (*Lieu-green-tree*), and that at the commencement of winter it is carried about for sale in small fagots under the name *Lieu-lo-tche*.

I have had made by a Chinese artist a drawing from nature of a branch of the *Hom-bi Lo-za* loaded with seeds, still green, but which towards winter become black. The mail of December will, I hope, bring you about 50 lbs. of these seeds:—at least I have taken measures

that it shall do so. If you desire plants, a case of them can be sent, but I am assured that the seeds will answer quite as well. If I can do so, I shall send you seeds of the *Pa-bi Lo-za* also.

The seeds and shrubs which M. de Montigny sent are certainly authentic. Both the *Hom-bi* and the *Pa-bi Lo-za* ought to be found among them; they may be distinguished, if by no other method, at least by the color and preparation, which is still to be described.

2. The preparation of the green dye with the bark of the *Hom-bi Lo-za*, as carried on at Azé, is as follows:—

The *Loza* is taken, as before mentioned, in the form of fagots of small wood, which are brought in by the peasants. Its bark is then roughly removed with a knife; it should not be quite dry, as the dry bark is no longer rich in coloring matter; this is why the branches are not stripped until the time for using the bark. The smaller branches are crushed with a hammer; in fact, in removing the bark a good deal of the wood adheres to it, so that a fagot of 100 lbs. is reckoned to give half its weight of bark. Of this bark, 12 lbs. are taken to 150 lbs. of water, and a decoction is made: a froth appears on the surface, which froth is at first white, and then passes to rose color. It is this peculiarity which has occasioned the name *Hom-bi* (red-bark) to be applied to this sort of *Loza*. When the froth or scum has assumed a rose color, the fire is removed, and the contents of the boiler are poured into a large vessel and suffered to macerate for at least two days and two nights, after which it is ready for use. In very cold weather this maceration may be carried on for twenty days; if left longer the materials would spoil.

The preparation of the dye of the *Pa-bi Lo-za* is precisely the same, only that continued ebullition has not the effect of causing the froth to assume a rose color, whence the name *Pa-bi* (white bark). Moreover, the maceration must be continued for at least ten days, whilst two days are sufficient in the case of the *Hom-bi*.

To insure success in the process of dyeing, clear, settled weather must be chosen, for the sun's action is absolutely necessary.

Placed ready for use, it is customary to have a vessel of lime water, which the Chinese prepare by putting the lime into a little bag and agitating it in a large quantity of water.

Dyeing Cotton Cloth Green.—The dyeing-liquors both of the *Hom-bi* or *Pa-bi Lo-za*, being ready as well as the lime water, the coloring is communicated to the cloth by dipping it from seven to ten times in the decoction of *Hom-bi*, concluding by three dippings in that of the *Pa-bi*, care being taken to allow the cloth to dry between each immersion. The details of the operation are as follow:—

We have already mentioned that the bark of the *Lo-za* is allowed to macerate two days in the case of the *Hom-bi Lo-za*, and ten in that of the *Pa-bi Lo-za*. The maceration being then complete, and the weather fine enough to admit of the dyeing process being commenced, the bark is removed from the liquor, and a small quantity of lime water added; the cloth is then plunged into the cold solution, wrung out so as to avoid any waste, and then, without rinsing, spread upon

the ground to dry. It is important that the drying be not too rapid. For this reason, the dipping is not commenced till the latter part of the day, so that the cloth may be exposed all night and receive the benefit of the following day's sunshine. But even sunshine may be in excess. This operation having been repeated from seven to ten times with the liquor of the *Hom-bi Lo-za*, and afterwards with that of the *Pa-bi Lo-za* (lime-water being added as in the former case), the cloth is dyed, but on one side only, viz., that which has been exposed to the sun. Such cloth is called here in commerce *So-lo-pou* (colored-green-cloth).

If after the dyeing operation is over, there still remains some of the solution unused, it may be employed the following day, a little more of lime water being added to it. If there is still some over for a third operation (which ought to be avoided), it may be used, provided a still larger quantity of the alkaline solution be added to it, and so on for any further residue.

I have asked many questions as to the use and properties of the two dyes *Hom-bi* and *Pa-bi*, and, so far as I can find, it seems clear that the solution of *Hom-bi Lo-za* gives a deeper, stronger shade, but without lustre or reflection; while the solution of *Pa-bi Lo-za*, though affording a feebler hue, imparts a magnificent lustre [*reflet magnifique*]. The ground of the color is therefore produced with the *Hom-bi*, and it is completed with the *Pa-bi*; and thus is obtained the green color that is most esteemed by the Chinese. I send you a small sample of cloth purchased at Shanghai for a sum equivalent to 25 French *centimes*. A similar quantity of uncolored cloth is worth two-thirds the price. From data like these I estimate that the dyeing of a square *mètre* of cotton cloth costs from 8 to 9 *sous*.

I have put many questions, in order to discover if any of the mordants used by the Chinese for other dyes—such, for instance, as alum, green vitriol, potash, &c.—is ever employed with this green; but the constant reply has been, that no other than lime-water is used. I have a suspicion, however, that in the north, the peasants do make use of a solution of alum.

(To be continued.)

AGRICULTURAL CHEMISTRY.

INVESTIGATIONS as to the QUANTITIES of NITRATES contained in the SOIL and in WATER.

By M. ROUSSINGAULT.

IN a former memoir, I have endeavored to show that saltpetre acts directly on the development of plants; I have mentioned the experiments on the use of Peruvian nitrate of soda on cultivation on a large scale, and I observed that the nitrates had been long known in the arable land endowed with a great degree of fertility, by Bowles, Proust, and Einhoff; in the water of torrents, rivers, and springs; in meteoric waters by Bergmann, Berzelius, and more recently by the

remarkable works of MM. Bineau, Henri Sainte-Claire Deville, Brandes, Liebig, Bence Jones, and Barral.

In the investigations which I am now about to lay before the Academy, I propose to extend the researches of my forerunners, by determining how much nitrate is contained, *at a given moment*, by 1 hectare of arable land, 1 hectare of meadow, 1 hectare of forest earth, and 1 cubic metre of river or spring water.

The nitrates were estimated in forty samples of earth; but before presenting the result of these estimations, I should first mention the circumstance which decided me to undertake the work.

I had occasion to remark that the plants grown in the kitchen garden of the old monastery of Liebfrauenberg contained very considerable quantities of nitrates; the beetroots which I cultivated in 1854, at the request of M. Peligot, containing so large a proportion as to render it almost impossible to extract the sugar from them.

In the autumn of each year, the garden received a very thorough manuring of very strong farm-yard manure. The soil is light; it is a disaggregation of free-stone; water passes through it, because the soil extends to a great depth.

On the 9th August, 1856, after a fortnight of great heat and dryness, some of this vegetable earth was taken out. In 1 kilogramme of this earth dried in the sun was found the equivalent of 0.211 gr. of nitrate of potassa. The litre of dry earth weighing 1.500 kilogrammes, we have 316.5 gr. of nitrate per cubic metre. So that, on the 9th August, we may consider that there were 1055 kilogrammes of saltpetre in 1 hectare of garden, taking 33 centimetres as the average depth of the vegetable earth.

Such a proportion of nitre in an abundantly manured soil is not surprising. In fact, if we incorporate in a well worked soil, stable manure in an advanced state of decomposition; then put in ashes or marl; work it well, to mix and give access to the air; and then drain it well, to prevent the stagnation of water; this is to prepare the ground to give abundant harvests. Well, when we reflect, this is exactly the plan pursued when we wish to establish an artificial nitre bed. The only difference is, that, in a rainy climate, the nitre bed should be sheltered so as to preserve in the earth salts so soluble as the nitrates; and which, even if it did not continue long, the rain would remove or at least carry into the subjacent soil. Thus, from the 9th to the 29th August, it rained every day at Liebfrauenberg; we measured, in the udometer, 58 millimetres of water. The 29th August, after it had ceased raining, the earth was taken up into the same vessels into which it was put on the 9th. After drying, 1 kilogramme of this earth gave 0.0087 of nitrate; consequently, in 1 cubic metre, the equivalent of 13 grammes of nitrate of potassa, or 43 kilogrammes per hectare. The greater part of the saltpetre had therefore disappeared from the surface of the soil.

In the month of September it rained fifteen times, and 108 millimetres of water fell. On the 10th October, after fourteen dry days, the soil of this garden, under the influence of a steady wind, had lost

its excess of humidity—it had become sufficiently dry to require watering. Earth taken from the foot of a wall, gave, after drying, 0.298 gr. of nitre per kilogramme, or 447 grammes per cubic metre, or 1490 kilogrammes per hectare; a number which approximates and exceeds that obtained in the estimation of the 9th August. The alternations of dryness and humidity which the soil had undergone explain the enormous variations ascertained in the proportions of the nitrates; as for the large quantities of these salts, it proceeds, undoubtedly, from the profuse manner in which the garden had been manured—true type of an intense culture. It therefore appeared advisable to estimate the saltpetre in soils which are not manured, such as the soil of forests, or which receives but small proportions, such as common ploughed land.

I analysed seven samples of forest soil. The earth taken on the 27th October, in a fine forest near Ferrette, in the Haut Rhin, only furnished traces of nitrates.

The earth of a pine forest on the top of one of the Vosges mountains, and in such a situation that it was only moistened by rain water, contained, on the 4th September, the equivalent of 0.7 gr. of nitrate of potassa per cubic metre.

Some sand taken on the 15th October, in the forest of Fontainebleau, contained, per cubic metre, the equivalent of 3.27 gr. of nitrate of potassa.

In the earth of a heath, taken on the 15th August, in the forest of Hatten, a short distance from the Rhine, we found, per cubic metre, the equivalent of 12 grammes of nitrate.

In meadow soil, taken in September and October, on the banks of the Saïler, in a valley of the Vosges, and in a pasture situated near Roedersdorff (Haut Rhin), the equivalent of nitrate of potassa varied, per cubic metre of earth, from 1 to 11 grammes.

Of nineteen samples of arable earth, of good quality, taken in September and October, in the valleys of the Rhine, Loire, Marne, and Seine, four gave no nitre. The earth which contained the least proceeded from a field of maize, at Hoerd (Bas Rhin), from the vineyards of Liebfrauenberg, and from a field of beetroot on the bank of the Saïler; the cubic metre of earth only contained 0.8, 1.28 and 1.33 as the equivalent of nitrate of potassa.

The earths which were poorest in saltpetre were collected in a corn field in the environs of Reims, and from an arable land in Touraine; the cubic metre contained 10.4 and 14.4 gr. of the equivalent of nitrate of potassa. Some earth from Touraine, which had been manured with debris of shells five years before, showed a most exceptional richness; in one cubic metre there was the equivalent of 108 grammes of nitre.

I had not waited for this last result to seek nitrates in the calcareous manures which are added in such great quantities to the soil.

The debris of shells had been added to this soil in the proportion of 70 cubic metres per hectare. In 1 kilogramme of these debris, lately removed from the bed, I found no trace of nitre.

A very white marl, from Chaise pres Louzouer (Loiret), examined

immediately after its extraction, contained the equivalent of 7.2 gr. of nitrate of potassa per cubic metre. In marl from the same spot, removed in 1853, and which had remained heaped up near the quarry, 19 grammes of nitrate were found in the same volume. A very argillaceous marl, from the Chaumont banks, contained 25 grammes.

The chalk, at Meudon, is extracted in three superposed works. The upper part, which is much worked, in a part which was being carried, contained, per cubic metre, the equivalent of 16 grammes of nitrate. One fact, worthy of remark, is, that no nitre was found in the lower portions of the mass of chalk. Knowing what quantities of calcareous earth are used, it will be evident that nitrates should be sought in them, notwithstanding their small proportions, because they may form a part of those substances which marl contains in very small quantities, but which are nevertheless very efficacious, such as phosphate of lime and the alkaline carbonates.

With some few exceptions, saltpetre was found in the soils examined, and generally in very small proportions. But we must not forget that these estimations were made in a very rainy autumn, and that rain is apt to remove or at any rate to displace the nitrates. We found, in fact, that the nitre of 1 cubic metre of garden earth varied from 316 to 13 grammes, according to whether it was estimated before or after rain. One thing to be especially remarked in these results is the frequency of saltpetre in vegetable earth, whether taken from forest land at a great height above the valleys, where it receives no manure but rain, or whether taken from a cultivated soil, to which the most powerful manure has been applied.

As water dissolves the nitrates, we should expect to find the largest proportion of these salts in well manured ground, sheltered from the rain. I have found very large quantities of saltpetre in hothouse earth, which has the greatest analogy to artificial nitre beds.

In 1 kilogramme of earth from a greenhouse in the Jardin des Plantes, I found the equivalent of 6 centigrammes of nitrate of potassa; 89 grammes per cubic metre: 1 kilogramme of earth, taken from another greenhouse at the same establishment, gave the equivalent of 6 decigrammes of nitrate of potassa, or 804 grammes per cubic metre. These earths were not of the same density.

M. Moquin Tandon having kindly allowed me to take some earth from the hothouse of the botanic garden of the Ecole de Médecine, I found, in the light black earth, the equivalent of 0.121 gr. of nitrate of potassa, or 161 grammes per cubic metre.

In 1 kilogramme of strong earth, taken at a depth of 30 centimetres below the light earth, I found the equivalent of 0.107 gr. of nitrate of potassa; 185 per cubic metre. It is perhaps interesting to mention that it was in this same hothouse that M. de Luca executed his beautiful experiments on the nitrification of potassa by the elements of the air.

Whether the nitrates which I have found in such large quantities in the soil of hothouses have their origin in the atmosphere, or whether they are found in consequence of the gradual modification of the or-

ganic matters of the manure in the presence of alkaline or earthy bases; or whether again they are the simple result of the successive accumulation of nitrates brought by the water employed in watering; or from all these causes united; still, in any case, their remaining in the earth depends essentially on the fact that the rain cannot remove them: everything likewise leads us to the opinion, setting aside the favorable influence of heat and moisture, that it is in a hothouse that a manure will produce its maximum effect. This subject induces some reflections.

In the present state of knowledge, it is natural to attribute the nitrogenous principles of vegetables either to ammonia or to nitric acid; reserving, of course, the question as to whether the nitrogen of the acid may not pass into the state of ammonia, under the influence of the vegetable organism. The nitrogen of the albumen, caseine and fibrine of plants, has probably formed a part of an ammoniacal salt, or of a nitrate. To these two salts may perhaps be added a brown matter; but, even with the addition of this little known matter, it remains established that all the immediately active elements of manure are soluble; and, consequently, a manured soil, when exposed to continued rains, loses a more or less large portion of the fertilising agents put into it; thus we constantly find in drain water, the true ley of a soil, nitrates and ammoniacal salts: and, if it is true that the summit of mountains and elevated table lands have no other manure than the mineral substances derived from the rocks which form them and meteoric water, it is not less so that, in the most ordinary conditions of cultivation, a strongly manured land yields to the rain water which passes through it more fertilising principles than it receives from it. By giving to the earth manure in a but little advanced state of decomposition, containing consequently rather the elements of the ammoniacal products and nitrates than these salts themselves, the ill effects of prolonged rain will be much less than if we apply a manure in which the soluble salts are already in large proportion. Moreover, among the advantages incontestably attending the application of *liquid manure*, I believe that the greatest are the giving to the plants matters modified so as to be fit for absorption, and only giving them in the proportions required; which estimation has a certain resemblance to the most delicate processes of experimental physiology, and which removes the manure from the dissolving action of rain water.

(To be continued)

APPLIED PHYSICS.

On INSTANTANEOUS, ELECTRICAL, and HYDROTHERMIC IMAGES.

By M. MORREN.

To obtain beautiful, electrical prints, by the process which I published in 1845, the following is the most certain method; it succeeds equally well for small proofs, as for plates of considerable dimensions.

A plate of glass is taken, of about 1 millimetre in thickness and of considerably larger dimensions than the medal or print which it is desired to reproduce. On one side is fixed a metallic armature—a sheet of tin, for instance, so that some centimetres shall be uncovered all round beyond the sheet of glass. The free side is dried, but not electrified. This plate is placed upon a table; the metallic armature which is beneath communicates with the earth. On the glass is placed a small sheet of paper, one side of which has been covered with a layer of dextrine: the first should be quite dry, and a bad conductor of electricity. On the paper is placed the piece of money, medal, or plate which it is desired to reproduce; but previously it must be covered, by rubbing with the finger, with a thin layer of some conductor of electricity reduced into fine powder and adhering to all the furrowed parts of the coin; plumbago answers very well. Then rub it with the clean finger until all the parts in relief are clean and uncovered. Then, before putting the medal on the paper, it is turned and tapped slightly, to throw off any non-adherent plumbago. The medal, deposited gently on the paper, only requires to be put near a charged Leyden phial; contact is not necessary, and the print is formed with great distinctness and power. The expulsion of the conducting body is so powerful that, if silk is used instead of paper, the image passes through and is visible on both sides. To fix it solidly, it is only necessary to put the sheet of paper over a vessel containing steam; the dextrine becomes humid, and the plumbago thus fixed adheres to it.

If, before the fixing, several plates are put one above another, separated by sheets of white paper, and put these upon the plate of glass, after having deposited upon them a thin layer of metal, and then place them all near the Leyden phial, the prints will be found copied upon the white leaves.

One curious peculiarity is, that the proofs are much less beautiful if we take the precaution of cleaning the medal with any powder, such as pounce, tripoli, Spanish white, etc. In the first place, it is evident that the lustre thus produced renders the adherence of the plumbago to the coin less easy; then we remove almost completely the peculiar substance, which I believe to be organic, which I mentioned in 1845, and which covers all bodies exposed to the air. This, in the above experiment, is partly projected on the paper with the plumbago, and produces this singular circumstance, that the first proofs produced with a medal are always the most beautiful, and that afterwards their distinctness diminishes. Now, according to this fact it is probable that, in the experiments of Mr. Grove, the print produced by the electrical tension, when a sheet of paper with characters on is placed upon a sheet of glass, is because this peculiar substance, unequally spread on bodies according to the modifications of their surface, is projected on the glass, on which the vapor of breath causes it to appear distinctly.

The hydrothermic prints, of which I spoke in 1845, are produced in the same manner, but by employing moisture instead of plumbago, and heat as a repulsive force instead of electricity (it probably suffices

to heat the metal), prove, by the distinctness of the image which appears when the steam reaches it, that the radiation of the heat, similar to electric radiation, is made normally on the slightest asperities of the surface of the bodies put in equilibrium.

Comptes Rendus.

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS.

VALERIANATE of AMMONIA.

THIS substance appears to attract considerable attention, as a remedy for neuralgia, and has been the subject of a somewhat unseemly squabble among certain members of the Parisian faculty; but with this we have no concern. When we first saw a notice of it in the periodicals, we could find no mention of its mode of preparation; and as the dose spoken of was by spoonfuls, manifestly of a solution of unknown strength, we were thrown on our own resources to prepare it. Accordingly, we mixed equivalent solutions of valerianate of zinc and carbonate of ammonia, and removing the carbonate of zinc thus formed by filtration, evaporated the filtered liquid, and finding that it would not crystallise, dried and powdered the residue. We found this to be a very expensive process, the product being considerably less than what theory would lead one to expect. M. Laboureur being, like ourselves, without a guide, has also been experimenting; but the process which he adopted, was to pass dry ammoniacal gas through mono-hydrated valerianic acid, when he obtained a product perfectly white, and confusedly crystallised. Its composition is, one equivalent of valerianic acid, one of water, and one of ammonia; or, one equivalent of valerianic acid, and one of oxide of ammonium, according to the theory you adopt. It is very deliquescent; when placed on water or alcohol it gyrates rapidly, according to the custom of the valerianates. It has a mixed odor of valerianic acid and of ammonia, but soon loses the latter when put in an exhausted receiver. Its reaction is slightly acid, even when dissolved in water or alcohol. Ether dissolves it, forming an oily liquid; so do the oils, although more slowly. Oil of turpentine gives it the appearance of transparent plastic fat. Heat partially decomposes it, and what remains re-crystallises by cooling. The acids decompose it, liberating the valerianic acid, which swims on the surface of the liquid.

It appears now, however, that the medicine first introduced to the notice of the profession by M. Déclat, is a solution of valerianate of ammonia of a fixed strength, which has long been prepared by M. Pierlot, a pharmacien in Paris, and which has been extensively exhibited to the epileptics, both at the Salpêtrière and the Bicêtre. M. Pierlot has at length published his formula, which is as follows:—

Distilled water, 32 drachms,
Valerianic acid, 1 drachm,
Sub-carbonate of ammonia, q. s.

To neutralise the acid, add—

Alcoholic extract of valerian, 2 scruples.

His object in the construction of this formula, he says, was to obtain a concentrated solution of all the constituents of valerian root, in a condition as little disagreeable as possible. He maintains that valerianic acid pre-exists in the root, and is an educt, not a product, as hitherto presumed.

However that may be, there seems to be some virtue in the medicine. Dr. Desmarres describes a case of intense choroiditis, in which, after considerable depletion and low diet, severe paroxysms of neuralgia supervened. Doses of a grain and a half of sulphate of quinia seemed merely to exasperate the pains. He then tried Pierlot's solution of valerianate of ammonia, in doses of three coffee-spoonfuls per diem; and the first day the pains so far remitted, that the patient obtained a tranquil sleep, and in a few days more he quite recovered his appetite. Dr. Tufnell, Professor of Military Surgery in this city has also tried it in some cases, and found it eminently successful.

Dublin Hospital Gazette.

On the ECONOMICAL USE of QUINIA in INTERMITTENT and REMITTENT FEVER.

By O. C. GIBBS, M.D., Chatauque Co., N. Y.

THERE has been much time spent, and many experiments made, with the view of finding a remedy that might serve as a substitute for quinia, in those diseases over which that agent exerts almost a specific control. The high price of the article, the frequent and ever-recurring necessity for its administration in some localities, and the probable failure of the cinchona tree at no very distant day in the future, have all acted as so many stimulants to the search for the much-desired substitute. Considering the frequency of the so-called miasmatic diseases in many situations, and the high price and unpalatableness of the agent most successful in their cure, a substitute for the latter remains a much desired desideratum. Whether or not such a substitute exists in nature is a question yet to be decided. Until such a discovery is made, it may not be inappropriate to study economy in the use of a remedial agent, of which the supply may not always equal the demand, and the cost of which must ever in some measure militate against its liberal administration or prescription, in miasmatic diseases occurring among the poor.

There is no small amount of diversity of opinion among medical authors and practitioners, as to the amount of quinia necessary to arrest and cure a case of miasmatic fever; and the same diversity exists in regard to the amount of preliminary treatment, and the appropriate time for the commencement of the curative agent. There are many physicians now, and a few years ago there were more, who make no attempt to arrest an intermittent, until after the patient has suffered the recurrence of several paroxysms; and many, also, who

never administer quinia in a case of *remittent* fever until it subsides into a distinct intermittent. Many physicians suppose that venesection, emetics, cathartics, the anti-bilious and alterative influence of calomel, and the sudorific effect of Dover's powders, are indispensable preliminaries and preparatories for the safe administration of the anti-periodic. This opinion, where miasmatic diseases are most prevalent, is gradually wearing away. For myself, I have believed, and acted upon that belief for years, that a cathartic, of which calomel formed an ingredient, and, occasionally, bloodletting, in nine-tenths of the cases, as they have fallen under my observation, are the only preparatory measures demanded. This treatment involves little or no loss of time, as the anti-periodic can usually be brought to bear upon the case in sufficient time to prevent the recurrence of the succeeding paroxysm. I do not now remember to have met with more than two instances, in the last five years, in which a paroxysm of fever has succeeded the commencement of medication, excepting in cases of relapse after a temporary arrest of the disease for two or more weeks. In each of these two cases, a single paroxysm followed the commencement of treatment, when the disease was promptly arrested. And my experience proves that periodical fevers are much less liable to complications and relapses, where the disease is promptly met and arrested at the commencement, than when delay is allowed for the purpose of preparatory treatment, or with a view of waiting for a distinct remission or intermission, in cases of remittent forms of disease.

I do not propose here to argue the propriety of the early administration of quinia in intermittent and remittent fevers, however confident I may be of the judiciousness of such a procedure; the object of the present paper being simply to suggest a plan of meeting these diseases, with perhaps more than average promptness and permanency of success, and, at the same time, with more than ordinary economy in the use of the sulphate of quinia.

Some practitioners are in the habit of prescribing from two to four grains of quinia once in three or four hours during the intermission of the febrile paroxysm; while others advise from eight to ten grains, to be repeated every four or six hours during the same period of intermission. Under the former plan, the paroxysms will frequently continue to recur for several days; under the latter, though a prompt arrest is usually made, the medication is unnecessarily large; and as both advise a continuance of the remedy in order to secure a degree of permanency in the cure, the amount of the sulphate administered is, in the aggregate, much above what I have reasons for believing the necessities of the case demand.

In the year 1851, while I was practising medicine in Perry, Lake County, Ohio, malarious fevers were exceedingly common among the Irish laborers upon the Cleveland and Erie Railroad. As they were mostly poor and transient, and, as a physician was expected to furnish his own medicines, it became a matter of policy with me to reduce professional services and cost of medicines to the lowest minimum, compatible with the safety and speedy recovery of the patient.

Because many of them had been previously ineffectually treated with quinia, and because of their ignorant and unreasonable prejudices against that article. I was obliged to give it an unusual form, thus avoiding their prejudices, and giving them the impression that the treatment was not a repetition of that which had previously proved ineffectual. After a little reflection, a mixture was prepared after the following formula, which, in all my subsequent experience, has proved uniformly adequate, with such adjuncts as the peculiarities of some cases seemed to require.

R—Sulphate of Quinia,
Tartaric Acid, *aa* ʒij.
Tincture of Opium, ʒij to ʒiij.
" Capsicum,
" Camphor. *aa* ʒij.
Simple Syrup, ʒx.—Mix.

The tartaric acid converts the sulphate into a tartrate of quinia, which is readily soluble in the simple syrup. Opium is probably the best known adjuvant to the quinia, and the camphor and capsicum my experience proves to be no unimportant additions. My usual directions for an adult have been as follows:—Take three compound cathartic pills at evening; and, in the morning, two or three hours before the paroxysm is expected, take a tablespoonful of the mixture; and if the type of the disease is quotidian, take two teaspoonfuls of the mixture every morning, for three succeeding mornings; then one teaspoonful every morning thereafter, until the two ounces of the mixture are taken. If the type of the disease is tertian, the mixture is to be given in the same doses only every second morning. In 99 cases in 100 of intermittent fever, as they have fallen into my hands, the first dose of the above mixture will arrest the paroxysms; and the repetition of the remedy, at intervals corresponding with the type of periodicity, is usually sufficient to effect a cure, more permanent than that following any other plan that I have ever pursued.

So much for the quantity, dose, and time of administration of the anti-periodic remedy, simultaneous with which any other remedies may be administered, which the circumstances of individual cases may indicate. It is but just to observe here, however, that in those cases among the indigent Irish, where the abovementioned remedies were first prescribed, no additional medication was employed. And yet, from these cases, the reputation of the mixture so rapidly extended, that I was soon consulted in miasmatic diseases, much beyond my usual limits of practice; and, before the first autumn had passed, had been many times consulted, in behalf of patients living 50 and 100 miles away. I make mention of this fact in no spirit of egotism, but simply in confirmation of my belief, that the above plan of treatment is more speedily and permanently effectual than that usually employed by physicians in the vicinity of where I then resided.

The difference between an intermittent and a remittent fever, seems to me to consist in a higher febrile excitement in the latter, with a

protracted paroxysm, and consequently less complete intermission, and greater tendency to visceral congestion and inflammation. Hence, in cases of remittent fever, if seen before inflammation of any organ has been established, I usually bleed as liberally as the attending circumstances will warrant, and then resort to the curative measures above specified without delay; and the happy results that I have thus far met with, prove the safety and propriety of the procedure.

According to my observation, 40 grains of the sulphate of quinia are usually sufficient for the speedy cure of most cases of miasmatic fever. What success would follow such limited medication further south, where these fevers assume a greater degree of malignancy, I have no means of determining.

North American Medico-Churgical Review, January, 1857.

SANTONIN as an ANTHELMINTIC. By G. G. PERRY, Esq., Droxford.

[The very satisfactory effects of Santonin in expelling round and thread intestinal worms are not generally known. Mr. Perry, who has tried its effects in many cases, says:]

Among the first cases treated, was that of a child of two years of age, to whom I gave three grains of santonin, followed in two hours by an aperient powder; this child voided the next morning, at one time, thirty-seven worms, some of them a foot in length, of the lumbricoid ascaris kind. Two children in another family were similarly treated, and between forty and fifty worms came from each; again, in a family of four, 124 worms at one time, and many more afterwards, followed the aperient, each child having taken one dose of santonin. I could instance many more cases which have been relieved by this medicine. After the expulsion of the worms, I give a tonic mixture, containing the muriated tincture of iron and muriatic acid, and change the diet from a vegetable one to that of meat and bread. My cases all do very well.

I should state that santonin is a medicine that may be administered with perfect safety. I give it in its crystallised form, between bread and butter, and two hours after it a dose of calomel and jalap; in some cases an interval of twenty-four hours occurs before the worms are voided.

The persons who reside in the locality in which I have met with these cases, are very poor, and from the high price of bread this winter, have had recourse to vegetables of the commonest description, as an article of food, which will account for the presence of worms in the alimentary canal.—*Medical Times and Gazette*.

TREATMENT of TAPEWORM by the OIL of MALE FERN.

By Dr. WILLIAM JENNER, Physician to University College Hospital.
In the treatment of tapeworm, we have to keep three objects in view, viz.—

1. The expulsion of the entozoon ;
2. The prevention of the entrance of another scolex of *tænia* into the patient ;
3. The improvement of the health of the patient, so that his intestines may no longer form a favorable nidus for the development of the scolex into a *tænia*.

The two last objects are to be attained by directing the patient to live well, but to avoid pork and imperfectly cooked meats of all kinds. Spices, onions, and garlic, should be used with the food. Spirits and wine are to be preferred to beer. Beer, especially if it contains but little hop, is thought by some most experienced German physicians to be highly favorable to the development of tapeworm. Mild aperients, vegetable bitters, steel, and zinc, are the medicines in which most confidence is to be placed. Out-door exercise is essential.

For the expulsion of the worm, various agents have been employed with success. As the animal increases in length, by the formation of new joints, at the neck close to the head, it is no matter how many yards are removed, provided the head remains; for, in that case, the worm quickly grows to its original length. No remedy, then, is successful which does not expel the head. But, although this is true, and tapeworm is a common enough disease, many practitioners have never seen the head of a tapeworm.

The anthelmintics chiefly employed in cases of tapeworm in this country are turpentine, kousso, pomegranate, and male fern. The objection to turpentine is its horribly nauseous flavor, and its very unpleasant effects on the head, and occasionally on the kidneys. It is a remedy which should be used only as a last resource. Kousso is expensive and bulky. Pomegranate is bulky and nauseous, and, as ordinarily obtained in this country, not very certain in its action. Male fern has the advantages of being inexpensive, only moderately disagreeable in flavor, so that children take it readily, of small bulk, perfectly innocuous to the patient, and more certain than the other agents in its action on the parasite. It is one of the oldest of the remedies for tapeworm, and one of the very best. The preparation I have used is the ethereal oil. An aperient was given in the morning, the patient was kept without food for sixteen or eighteen hours, and then one or two drachms of the oil of male fern were administered on a little cinnamon water.

I have notes of twenty-four cases to which the oil of male fern was given. Sixteen of these cases were cured by a single dose. In three of these sixteen cases the head was found, three of the remaining thirteen were ascertained to be well two years after the administration of the oil, one a year after, one seven months, two six months, three four months, one three months; and before the other two ceased to be

under observation, a second dose was given by way of precaution, as it was to all the patients when the head was not found, without any *tænia* coming away with the stool.

Three required two doses of the drug; in one of these three some yards of *tænia* were expelled by the first dose; for two months after this no joints were found in the stools, then a few appeared, and a second dose was given, and was followed by the expulsion of nine yards of *tænia*; the patient continued well two years after this. In the second case, three yards were expelled by the first dose, and a month after, five feet by a second dose; at the expiration of four months and a-half the patient continued well; in the third case five and a-half yards of *tænia* were expelled by the first dose, and seven yards by the second, given two months after the first.

These doses were required in two cases. The first dose of the oil, however, given to one of these cases was not of good quality. In one of the two, three days elapsed between the first and second dose, and four hours between the second and third. In the other, two days elapsed between the first and second dose, and one between the second and third. In both cases the head was obtained.

In one case, viz., that of a child five years and six months old, between the 15th of July and the 4th of August inclusive, five doses of castor oil, and as many of oil of male fern, were administered without a decided effect—a few joints of *tænia* only being expelled. On the 17th of August, twenty grains of the extract of male fern, obtained from Duncan and Flockhart, of Edinburgh, were given without effect. On August 23rd, one point of infusion of pumpkin seeds; on September 1st, decoction of pomegranate; and on September 5th, infusion of kousso; all produced copious evacuations, but no tapeworm. The child now left the hospital. In November he was readmitted, and during my absence was treated with success by my friend Dr. Ballard with the oil of male fern. This time the child was kept for forty-eight hours with little if any food, before the oil was given. The child was free from tapeworm some months after he left the hospital.

One man took the oil two or three times without any good effect, but then large quantities of solid *fæces* were discharged from its action; and before it could be administered in a more effectual manner, the patient escaped observation.

Among those cured by a single dose, and well two years afterwards was one man who had taken kousso three times, and oil of turpentine twice. Several of the others had taken turpentine and other remedies with permanent good effect. In three cases (children) the patients rejected the oil by vomiting; with one exception, all admitted that it was much less nauseous than castor oil. In no case did it cause griping or other unpleasant symptom. The shortest time after taking the oil in which the worm was expelled was half an hour; the longest twelve hours; the ordinary time four hours. A large quantity of tenacious yellow mucus was usually expelled either with or before the worm, and often, also, when no worm was present, as when the oil

was given to ascertain that no worm remained, the head not having been found.

In no case was the worm alive when expelled, and in no case was it expelled entire.

The mode of administering the oil of male fern, I would recommend after the experience I have had of it, is as follows:—

For an adult, two pills may be taken at bedtime, containing three grains of calomel and eight of compound colocynth pill—the following morning a dose of castor oil. A little broth only should be given till the bowels have been thoroughly cleared out. As soon as that object is effected, one drachm and a-half of oil of male fern is to be given on an ounce of some aromatic water; and the dose of oil of male fern is to be repeated in six hours, if the first dose has not proved effectual before the expiration of that time.

For a child, calomel and jalap may be substituted for the colocynth and calomel. The dose of the oil of male fern must be as large for the child as for the adult, seeing that its action is on the parasite and not on the patient. I have never seen any unpleasant results follow its employment in the child.

Association Medical Journal, August 23rd, 1856.

On a NEW SYSTEM of TREATMENT SUCCESSFULLY EMPLOYED in DIABETES. By M. Piorry.

A PATIENT under me at the Hôpital de la Charité, Salle Sainte Anne, No. 19, was attacked with diabetes, with a very abundant production of sugar. From the 2nd to the 12th of January, she passed 10 quarts of urine per day. The kidneys, the liver and all the organs, with the exception of the spleen, which was slightly hypertrophied, were of normal size. They occasioned no symptom of disease. Febrile attacks, coming on in the evening, yielded rapidly to one gramme of quinine dissolved in alcohol diluted with water.

On the 12th of January, the patient was subjected to almost entire abstinence from drinks and liquid nourishment; 125 grammes of sugar candy and two portions of meal were given to her. On the 13th, the urine secreted was 10 quarts, and contained 58 per 1000 of sugar, as on the preceding days. On the following days, the same treatment was continued, and the quantity of urine secreted was only 2½ to 3 quarts a day, and did not contain more sugar than 2½ quarts of this fluid formed before the treatment.

On the second of January the loss of sugar was nearly 700 grammes in 24 hours; from the 12th to the 24th of January, and although the patient had taken sugar candy, it was only 136 grammes per day. The density of the liquid remained at 1060, as it was before the treatment was commenced.

The idea which led me to employ this treatment, which is very different from all the means hitherto employed, was this:—It is now

ascertained that the presence of sugar in the system is indispensable to the maintenance of life. Now, in the morbid state called diabetes mellitus, the patients lose enormous quantities of sugar. To deprive them in these conditions of sugar and fecula, is to keep from them the means of repairing the losses which they experience of an useful principle. It seems thus far rational to restore it by the food. The fact which I have just narrated tends on one hand to confirm the accuracy of this notion, and on the other to demonstrate that abstinence from drinks is of extreme importance in diabetes mellitus.

I had already profited by the privation of liquids in a patient who passed in one day 30 quarts of urine (not containing sugar), and I had thus reduced the quantity to one quart a day. I was also led to have recourse to this abstinence from drinks in diabetes by the very beneficial results which I had frequently obtained by this means in case of albuminous urine.

I am far from believing that the fact now brought before the Academy decides the question as to the treatment of diabetes: I say only that it is desirable to make experiments in the new direction towards which this fact leads.

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THE MONOGENESIS OF PHYSICAL FORCES: a Lecture delivered at the London Institution, February 18, 1857. By ALFRED SMEE, F.R.S., F.C.S., Surgeon to the Bank of England, &c. London: Longmans & Co. 1857, pp. 24.

THIS is an attempt to trace all the physical forces to one common origin, into which we shall not attempt to follow the learned and ingenious author. The subject is one, however, to which we wish to call the attention of such of our readers as are fond of speculation.

Mr. Smee states "that attraction, acting on attracted matter, is the source of all force, and that, therefore, every physical force has a monogenetic origin, and, when generated, a truly equivalent power."

CHEMICAL AND PHARMACEUTICAL MANIPULATIONS: a Manual of the Mechanical and Chemico-Mechanical Operations of the Laboratory. For the Use of Chemists, Druggists, Manufacturers, Teachers and Students. Second and enlarged Edition. By CAMPBELL MORFITT, Professor of Analytic and Applied Chemistry in the University of Maryland; and CLARENCE MORFITT, Assistant Melter and Refiner in United States Assay Office. With 537 Illustrations. Philadelphia: Lindsay and Blackiston. 1857, pp. 626.

THIS is a very valuable manual; well arranged, and most usefully illustrated. It commences with copious directions for the construction and fitting up of the laboratory, and then proceeds to treat of the apparatus, instruments, and operations of the laboratory in a very full and clear manner.

The practical utility of a work like this—partly written and partly compiled by competent persons like the authors—cannot be too highly estimated; and we doubt not that the chemical practitioners and students of the United States find this book a great boon. We do not know whether its sale in this country can be interfered with

owing to the present position of the copyright laws between the two countries; but we know that our readers will find this manual highly suited to their wants, if they can procure it, as probably they can.

We find that the authors have availed themselves of the valuable tables of specific gravities and table for testing by the blow-pipe, originally contributed by our talented friend, Mr. Thornton J. Herapath, to *THE CHEMIST*.

TESTIMONIAL to MR. SCOTT ARCHER.

SIX years have elapsed since Mr. Archer made known to the world, in a communication to *THE CHEMIST*, his discovery of the now universal "Collodion Process." Since March 1857, when the process was first described in our journal, how many thousands have in one way or another, been made acquainted with the immense advantages which this process offered to those who desired to copy the works of Nature or of Art by the agency of light!

When admiring the magnificent photographic prints, which are now to be seen in almost every part of the civilized world, an involuntary feeling of gratitude towards the discoverer of the collodion process creeps over us, and we wish that we could tell him personally how much the world is indebted to him for having placed at its command the means by which such beautiful objects are presented to us. And how many thousands—many of whom owe their means of subsistence to this process—must have experienced this feeling of gratitude! Had Mr. Archer patented his process, instead of generously giving it to the public, what a large fortune he would have realized by the sale of licences alone!

It is with such feelings as these that we invite the readers of *THE CHEMIST*,—and they are many—to assist us in securing for Mr. Archer some reward for the service which he has rendered to science—art—his country, nay, to the world!

We propose that an immediate subscription should be raised for this desirable purpose, and we shall be happy to receive the co-operation, and to attend to the suggestions of our readers, and we shall with pleasure receive subscriptions from any of them who may feel with us, that Mr. Archer deserves an acknowledgement for the service which he has rendered us all.

We trust, moreover, that the Government will take into its consideration the vast service which it has derived from the use of the collodion process, at home and abroad, and that it may reward him as he so richly deserves.

It is to be hoped that our scientific brethren, and all professional photographers, and amateurs, who have derived pleasure or gain from the collodion process, will at once come forward and aid us in this undertaking, and we feel sure that they will do so with pleasure. We shall be happy to receive communications from all who may be willing to co-operate with us, and trust that a committee will at once be formed to carry out our suggestion.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the SOLUBILITY of SULPHATE of BARYTA in ACID SOLUTIONS.

By F. CRACE CALVERT, F.C.S.

(Concluded from page 451.)

Employment of Sulphuric Acid instead of Sulphate of Potash.

I THOUGHT it right to make the following series of experiments, in order to ascertain that the non-formation of sulphate of baryta, in a liquor rendered acid by nitric acid, was not owing to a combination being formed between the sulphate of potash and the nitric acid; to attain this object, I poured into each multiple volume of nitric acid a quantity of sulphuric acid, equivalent to that contained in 3·34 grs. of sulphate of potash, and the results obtained were identical with those observed in Table No. 2; consequently no combination was formed between the nitric acid and the sulphate of potash.

TABLE No. 8.

No. of Jars.	Number of Divisions of the Alkali-meter.	Corresponding Weight of Acid. Sp.gr. 1·167.	Sulphuric Acid equal to 1·533, capable of producing 4·46 Ba O S O ₄	Weight of Nitrate of Baryta.	Time when a precipitate appeared.	Weight of Nitrate of Baryta.	Time when a precipitate appeared.
1	40	466·8	1·533	5·00	instantly.	10·00	instantly.
2	80	933·6	..	..	13 minutes.	..	6 minutes.
3	120	1400·4	..	..	2 hrs. 30 min.	..	30 minutes.
4	160	1867·2	..	..	7 hours.	..	1 h. 15 min.
5	200	2334·0	..	..	24 hours.	..	5 h. 30 min.
6	240	2800·8	..	..	no precipitate	..	7 h. 30 min.
7	280	3267·6	..	..	ditto	..	24 hours.
8	320	3734·4	..	..	ditto	..	no precipitate
9	360	4201·2	..	..	ditto	..	ditto
10	400	4668·0	..	..	ditto	..	ditto

I also made several series of experiments, dissolving first the nitrate of baryta in the nitric acid, and then pouring into it the sulphate of potash; but this mode of operating did not produce any alteration in the results obtained. During this series of experiments, I determined the solubility of nitrate of baryta in nitric acid sp. gr. 1·167 at a temperature of 60°.

1	grain of nitrate of baryta requires	250·2
2·5	"	446·8
5·0	"	933·8
6·5	"	1334
8·0	"	2800
10·0	"	3267

On the influence exercised by different degrees of concentration of Nitric Acid on the formation of Sulphate of Baryta.

In the following series of experiments I operated in an entirely different manner; the same quantity of nitric acid, sp. gr. 1·305, was put into each jar, and to each was added multiple volumes of water; therefore, each solution decreased in density; and after having dissolved the quantity of sulphate of potash which I intended to employ, I poured in the solution of nitrate of baryta.

On comparing the experiments contained in the following table, it will be seen that I have obtained results very different from the preceding, for instead of 4·46 of sulphate of baryta remaining in solution in the last five jars, as was the case when I employed the same quantities of salt in multiple volumes of the same acid, precipitates were found in all the jars, and that in the space of three minutes; therefore, multiple volumes of acid, decreasing in strength, do not exert the same influence as the increase of volume of the same acid. This table also presents a fact important for chemical analysis, viz., that the solubility of sulphate of baryta is affected even by the weakest acid, since 2,200 grs. of nitric acid, sp. gr. 1·032, are capable of dissolving 0·08 of sulphate of baryta; this solubility, though slight, is still five times greater than it would be in distilled water.

TABLE No. 9.

Order of Jars.	Number of Divisions of the Alkali-meter of Nitric Acid Sp. gr. 1·305.	Number of Divisions of the Alkali-meter of Water added to the Acid.	Sp. gr. of the Acid.	Sulph. of Potash dissolved in the Acid.	Nitrate of Baryta.	Time when a precipitate appeared.	Quantity of Sulphate of Baryta precipitated instead of 4·46.	Quantity of Sulphate of Baryta dissolved.
1	20	20	1·167	3·34	5·00	3 minutes.	4·28	Average quantity dissolved equal to 0·10
2	20	40	1·120	..	..	..	4·34	
3	20	60	1·085	..	..	..	..	
4	20	80	1·067	..	..	..	..	
5	20	100	1·057	..	..	..	4·35	
6	20	120	1·050	..	..	..	4·35	
7	20	140	1·044	..	..	..	4·36	
8	20	160	1·039	..	..	..	..	
9	20	180	1·035	..	..	..	..	
10	20	200	1·032	..	..	..	4·38	

The series of experiments which follows, was made with the view of knowing what would be the influence of a less quantity of nitrate of baryta for the same quantity of sulphate of potash, and it will be seen, in perusing the table, that instead of obtaining a precipitate in all the jars in three minutes, the time required was from 2 hours 30 minutes to 24 hours, although, with the exception of the quantity of nitrate of baryta, everything was equal.

TABLE No. 10.

Order of Jars.	Number of Divisions of the Alkali-meter of Nitric Acid Sp. gr. 1.305.	Number of Divisions of the Alkali-meter of Water added to the Acid.	Sp. gr. of the Acid.	Sulphate of Potash Dissolved in the Acid.	Nitrate of Baryta poured in.	Time required for a precipitate to appear.
1	20	20	1.867	3.34	0.25	2 hours 39 minutes
2	20	40	1.120	..	..	2 hours 45 minutes
3	20	60	1.085	..	..	2 hours 45 minutes
4	20	80	1.067	..	..	6 hours.
5	20	100	1.057	..	..	6 hours.
6	20	120	1.050	..	..	24 hours.
7	20	140	1.044	..	..	24 hours.
8	20	160	1.039	..	..	24 hours.
9	20	180	1.035	..	..	24 hours.
10	20	200	1.032	..	..	24 hours.

I thought that it would be interesting to know what would be the *minimum* quantity of nitrate of baryta necessary, in order that no precipitate should be formed in any of the jars of the series, when 3.34 of sulphate of potash was in solution, and the following table gives the results obtained.

TABLE No. 11.

Order of Jars.	Number of Divisions of the Alkali-meter of Nitric Acid Sp. gr. 1.306	Number of Divisions of the Alkali-meter of Water added to the Acid.	Sp. gr. of the Acid.	Sulphate of Potash Dissolved in the Acid.	Nitrate of Baryta poured in.	Time required for a precipitate to appear.
1	20	20	1.167	3.34	0.062	No precipitate after 24 hours in any of the jars.
2	20	40	1.120	..	..	
3	20	60	1.085	..	..	
4	20	80	1.067	..	..	
5	20	100	1.057	..	..	
6	20	120	1.050	..	..	
7	20	140	1.044	..	..	
8	20	160	1.039	..	..	
9	20	180	1.035	..	..	
10	20	200	1.032	..	..	

Has the formation of a Precipitate an influence on the Solubility of Sulphate of Baryta.

It is generally believed that the formation of a precipitate tends to increase its quantity, but if this opinion be correct with regard to crystalline precipitates, such as those of bitartrate of potash, &c., it is *not* so with sulphate of baryta; for it will be seen, in the following table, that the quantity of sulphate of baryta which remains dissolved in 1,000 grs. of nitric acid, sp. gr. 1.167, at 60° Fah., is the same, whether there is or is not a precipitate formed in the liquors.

Order of Table.	No. of Jar.	Quantity of Acid Sp. gr. 1.187 employed.	Weight of Sulphate of Baryta dissolved.	Quantity of Acid.	Weight of Sulphate of Baryta dissolved per 1000 grs.	Remarks.
2	5	2334.0	4.46	1000	1.957	With no precipitate.
3	4	1867.2	3.66	..	1.911	With a precipitate.
4	8	8734.0	7.186	..	1.919	With no precipitate.
5	5	2334.0	4.806	..	2.059	With a precipitate.
Results with 16 } grs. Ba O S O ₃ }	9	4201.2	9.180	..	2.066	ditto
	10	4668.0	10.470	..	2.149	ditto

On the influence of Mass on Chemical Affinity.

When the results contained in the preceding tables are compared, there is observed a certain number of facts which demonstrate the influence of masses of matter "put in presence;" and in order to corroborate this assertion, I add here a certain number of results, extracted from the preceding tables.

1. If we compare the results given by the two first jars in Tables Nos. 1, 2, and 4, it will be seen that, although the same quantity of acid was employed, the time necessary for the formation of a precipitate varied considerably.

Order of Table.	Quantity of Acid employed.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta	Weight of Sulphate of Baryta.	Time when a precipitate appeared.
1	466.8	0.753	1.121	1.00	12 hours.
	933.6	0.753	1.121	1.00	None.
2	466.8	3.84	5.00	4.46	Instantly.
	933.6	3.84	5.00	4.46	2 hours.
3	466.8	5.12	8.00	7.18	Instantly.
	933.6	5.12	8.00	7.18	2 minutes.

The differences observed above can only be owing to the fact that, for the same quantity of fluid, increasing proportions of nitrate of baryta and sulphate of potash were employed; for it cannot be explained by admitting that it is owing to there not being a sufficient quantity of nitric acid to hold a greater quantity of sulphate of barytes in solution; for if we calculate how much such sulphate of baryta remains dissolved in 1,000 grs. of acid, it will be seen that the quantities are the same in each of the three tables.

Order of Table.	Order of Jar.	Quantities of Acid employed.	Quantities of fluid represented by	Weight of Sulphate of Baryta dissolved.
1	1	466.8	1000	2.142
	4	1867.2	..	2.389
2	5	2334.0	..	1.911
	7	8267.6	..	2.182
4	8	8734.4	..	1.909

Neither can it be admitted that the difference observed is owing to the formation of a precipitate, for I have shown above that the presence of a precipitate has not any influence on the degree of solubility or non-formation of sulphate of baryta.

2. The increased solubility of sulphate of baryta, arising from the presence of quantities of matter, can only be explained by admitting that this solubility is influenced by the increased quantities of salt employed, since the volume of acid is the same in every case, and all other circumstances are equal.

Order of Table.	Order of Jar.	Quantity of Acid employed.	Quantity of Sulphate of Baryta Dissolved.	Quantity of Sulphate of Baryta dissolved in 1000 grains.	Increased ratio of solubility per 1000 gra. of Acid.
1	2	933.6	1.00	1.072	
2	6	2800.8	4.46	1.522	.450
3	9	4201.2	7.13	1.912	.890

I will again introduce the figures given above, as they show in a striking manner the rapid increase of the solubility of sulphate of baryta, for 1,000 grs. of acid, in three successive jars of the same series.

Order of Table.	Order of Jar.	Number of Divisions of the Alkallimeter.	Corresponding Weights of Acid. Sp. gr. 1.167.	Weight of Sulphate of Baryta Precipitated.	Weight of Sulphate of Baryta dissolved.	Weight of Sulphate of Baryta dissolved per 1000 gra.
1	1	40	466.8	4.44	0.02	0.048
	2	80	933.6	3.17	1.29	1.382
	3	120	1400.4	2.02	2.24	1.670
	4	160	1867.2	0.80	3.66	1.957

Therefore it appears to me that we must admit that the difference observed is due to the quantities of matter "put in presence."

I would also, in conclusion, call attention to the enormous differences between the effects of multiple volumes of an acid, compared with volumes of the same acid decreasing in density.

On the Solubility of Sulphate of Baryta in Hydrochloric Acid.

I thought it advisable to repeat some of the above series, substituting hydrochloric acid for nitric acid, and it will be seen that although sulphate of baryta is less soluble in this acid than in nitric acid, still it is sufficiently so, to cause chemists to avoid its use in excess in delicate analyses.

The following table shows the results obtained by employing multiple volumes of hydrochloric acid, of sp. gr. 1.0775, with quantities of nitrate of baryta and sulphate of potash, capable of producing 1 gr. of sulphate of baryta.

TABLE No. 12.

Order of Jar.	Divisions of the Alkali-meter of Acid Sp. gr. 1·0775.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Weight of Sulphate of Baryta obtained instead of 100.	Weight of Sulphate of Baryta dissolved.	Time when a Precipitate appeared.
1	40	0·735	1·121	1·00	·96	·04	1½ minute.
2	80	..	..	..	·85	·15	9 minutes.
3	120	..	..	..	·83	·17	50 minutes.
4	160	..	..	..	·66	·34	8½ hours.
5	200	..	..	..	·48	·52	10 hours.
6	240	..	..	..	·22	·78	24 hours.
7	280	..	..	..	no precip.	1.00	no precipitate
8	320	..	..	..	..	..	..
9	360	..	..	..	..	..	..
10	400	..	..	..	..	..	..

On comparing the results contained in this table with those of No. 1 Table of the nitric acid series, it will be seen that instead of the non-formation of a precipitate in jar No. 2, one was produced in the first six jars; sulphate of baryta, therefore, is less soluble in hydrochloric acid than in nitric acid, and still, if we calculate, taking for term of comparison the results obtained in jar No. 7 of hydrochloric acid, it will be found that its solubility is equal to one part of sulphate of baryta in 2,800 parts of hydrochloric acid; and consequently the solubility of sulphate of baryta in hydrochloric acid, sp. gr. 1·0775, is fifty times greater than in distilled water.

I have also made a series, employing the same quantities of salt of potash and of baryta, as in series No. 2 of nitric acid.

TABLE No. 13.

Order of Jar.	Divisions of the Alkali-meter of Acid Sp. gr. 1·0775.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Weight of Sulphate of Baryta obtained instead of 4·46.	Weight of Sulphate of Baryta dissolved.	Time when a Precipitate appeared.
1	40	3·34	5·00	4·46	4·85	·11	instantly.
2	80	..	..	..	4·84	·12	instantly.
3	120	..	..	..	4·24	·22	20 seconds.
4	160	..	..	..	4·20	·26	1½ minute.
5	200	..	..	..	4·14	·32	2 minutes.
6	240	..	..	..	4·14	·32	3½ minutes.
7	280	..	..	..	4·17	·29	6 minutes.
8	320	..	..	..	3·94	·52	9 minutes.
9	360	..	..	..	3·98	·48	18 minutes.
10	400	..	..	..	3·96	·50	18 minutes.

I have also made a series of experiments, employing no longer multiple volumes of the same acid of a constant strength, but multiple volumes of acid decreasing in strength, and it will be observed that the results in this last table are comparable with those of Table No. 7 of the nitric acid series.

TABLE No. 14.

No. of Jar.	Divisions of the Alkali-meter of Acid Sp. gr. 1.1425.	Divisions of Water added.	Weight of Sulphate of Potash.	Weight of Nitrate of Baryta.	Weight of Sulphate of Baryta.	Weight of Sulphate of Baryta obtained instead of 4.46.	Weight of Sulphate of Baryta dissolved.	Time when a precipitate appeared.
1	20	20	3.84	5.00	4.46	4.81	Average quantity dissolved equal to 0.15	instantly.
2	20	40	..	..	..			instantly.
3	20	60	..	..	..			instantly.
4	20	80	..	..	..	4.81		15 seconds.
5	20	100	..	..	..			ditto
6	20	120	..	..	..	4.82		ditto
7	20	140	..	..	..			20 seconds.
8	20	160	..	..	..			ditto
9	20	180	..	..	..			ditto
10	20	200	..	..	..	4.85		ditto

TRANSLATIONS AND ABSTRACTS.

MINERALOGY.

REPORT on several MEMOIRS by M. DELESSE, describing some CHEMICAL and MINERALOGICAL INVESTIGATIONS on CRYSTALLINE ROCKS, and ESPECIALLY on GRANITE.

By MM. ELIE DE BEAUMONT, DE SENARMONT, and DUFRESNOY.

THE study of crystalline soils has for a long time been a favorite subject for the investigations of mineralogists and geologists; but since the valuable labors of M. Cuvier and M. Brongniart on the neighborhood of Paris, geology has in some degree changed its direction. The fossils which exist in the Neptunian strata, furnish an easy method of classing them; the laws which regulate their arrangement in the earth, and their comparison with the animals of the present epoch, are so very interesting, that the majority of geologists have, for the last 30 years, preferred paleontological researches. The study of crystallised soils has therefore remained stationary; it is even now behind that of the Neptunian strata.

Our knowledge of the nature of crystalline rocks is chiefly derived from the memoir which M. Cordier presented to the Academy in 1815, on the mechanical analysis of rocks; although many new classifications have been made since that time, very little attention has been paid to their nature. What has just been said as to the mineralogy of rocks, does not apply to their mode of formation; the phenomena which produced them, the successive epochs in which they have appeared, have been the object of the investigations of M. Elie de Beaumont and others. We have now a certain knowledge of the different ages of many granites, porphyries and other crystalline rocks. Nevertheless, we are not afraid to assert, that this portion of geology has progressed less than that of modern soils, and that it would be a useful subject to which to direct the attention of science. One probable

reason of this state of things is, that this study is much more difficult, and requires on the part of those who devote themselves to it, a more various and profound course of study. Isolated observations, undertaken at one single point, or the discovery of certain fossils frequently suffice to determine the age of a stratified soil, or at least we are satisfied with this determination. The study of crystallised soils requires numerous and delicate investigations; we no longer see the successive layers which guide the observer; the characters which enable us to establish the differences between these soils, and to ascertain the period of their arrival at the surface, must be determined by numerous observations; it is even necessary very often to complete them by chemical experiments, which teach us the nature of the minerals: in many of them, indeed, especially with those which are termed *feldspathic group*, the external characters are frequently insufficient to characterise them, their color, their brilliancy, their hardness, and even their crystalline forms approximate very nearly, their composition alone is different; we must therefore have recourse to chemical analysis, if we wish to give a correct foundation for works on the crystalline rocks. After the division made by M. Lévy and M. G. Rose of the old feldspath into three species, under the names of *orthose*, *albite* and *labrador*, orthose has been supposed to be non-existent in volcanic rocks, and it has been supposed, on the contrary, that labrador was almost exclusively in these rocks; but this law which has been too hastily established, has not been verified by experiment, and the chemical study of these rocks has shown that many trachyles exist with a base of orthose.

The investigations of M. Delesse, on which we now have to report, are on this difficult part of geology; he has devoted peculiar attention to the group of the Vosges Mountains, which is especially curious, in consequence of the diversity and peculiar nature of the porphyries existing in it. M. Delesse has studied them in the order of their emission, so as to throw some light on their relative antiquity, and their connection with the stratified soils, raised and modified by their approximation. He has shown by the exact determination of the porphyry rocks, that if the greater part are the direct result of emission and consequently constitute porphyries, properly so called, some have become porphyric by the action of ignited rocks; he has shown that the granites, which form the axis of the Vosges, although possessing extremely varied characteristics, may be formed into two distinct categories, from their composition as well as from the phenomena which attended their emission.

1st. The Balloon Granites.—These are characterised by the presence of a single species of mica, remarkable for its deep color, which is generally black, sometimes of a pinchbeck brown, and sometimes of a blackish brown.

2nd. The Granite of the Vosges.—This contains two distinct species of mica, the one dark colored, analogous to that contained in the balloon granite, the second of a silvery whiteness, or of a pale violet grey color.

This mixture of two micas of different colors, forms the characteristic mark of the granite of the Vosges; so much so that these two rocks, although of the same nature, present such marked differences that the observer is in no danger of confounding them.

The balloon granite has a distinct granitoid structure, the elements are distributed in an almost homogeneous manner, whether considered by numbers or by dimensions; the Vosges granite is associated in some respects with Septinite, and in some with Gneiss, and although these three rocks may have distinct mineralogical characteristics, when considered separately, they only differ by the disposition of certain minerals, or by some modifications in their structure, so that they pass insensibly from one into the other, they constitute together a complex formation, which is very different to the simple formation of the balloon granite.

The manner in which these granites exist and their period of formation, present differences analogous to their structure; M. Delesse tells us, that the balloon granite constitutes immense dome-shaped masses, which predominate all over this part of the country, and which are not covered by any stratified rock.

The mountains formed by the Vosges granite are smaller; they surround these balloons which appear to have risen in one single piece, in the midst of the masses of ancient granite; the balloons are of more modern formation; the granite of the balloons has been violently projected through the Vosges granite; it has formed more or less extensive ranges through it; in some localities, especially on the route from Tendon to Tholy, it has even covered it. We must add, as a characteristic of these two granitic periods, that the Vosges mountains are covered by stratified rocks, which were penetrated by the granite balloons at the same time that this rock passed through the Vosges granite.

The difference of the phenomena which we have just mentioned, corresponds to the differences in the composition of the granites themselves, as well as in the numerous minerals which accompany them; these differences form the subject of the long and careful investigations of M. Delesse; they have required more than 80 extremely complicated analyses. The majority of these minerals contain potassa and soda mixed with silica, alumina, lime and magnesia; in many, oxide of iron and oxide of manganese are combined with oxide of titanium. The minerals disseminated in these granites do not line the cavities and are not isolated, as in veins; but are, on the contrary, impacted in the mass of the rock itself, so that the mechanical separation of them is difficult, which adds materially to the difficulties of the analysis; to this we must add, that to be sure of the results which he obtains, the chemist who examines these delicate questions should verify his analyses by trying them again on different parts, so as to ascertain the exact mixture of rocks with which the minerals are impregnated.

We have shown the circumstances under which M. Delesse undertook his investigations, and the difficulties which he had to conquer;

it is easy to show the progress which they have led to: he has demonstrated that the mountains of the Vosges are the result of two phenomena distinct both in time and in the nature of their products, which, although both known by the name of *granite*, have very different characters; he has likewise proved that the characters of the minerals which form the balloons and the mountains of the Vosges are very different. We shall now make some very summary remarks on these minerals.

Orthose is the preponderating mineral in the composition of the granite of the balloons; it is found in crystals which are sometimes white, and sometimes of a reddish fawn color; it is accompanied by another species of feldspath, which belongs to the sixth system. But it is particularly the presence of hornblende which characterises this granite by transforming it into syenite. Oxidulated iron, oligist iron, titaniuretted iron, zirconium and chlorite, which in other mountainous countries exist in amphibolic granites, are diffused in considerable abundance in the granite of the balloons.

Orthose is also the predominating feldspath in the granite which M. Delesse calls the *Vosges granite*; its general characteristics are different from that which constitutes the granite of the balloons; it is white, and very rarely with any pink tinge; it has a pearly lustre, and its crystals, which are frequently several centimetres in length, are very distinctly marked, so that this granite passes to the porphyroid structure. We have already remarked, that the presence of two micas, distinct both in composition and color, communicates to it a peculiar aspect. The accidental minerals are likewise different; garnets are very abundant; their crystals, whether red or reddish brown, are transparent; disseminated in all the varieties of granite of the ancient period, they are always peculiarly numerous in those varieties which pass into leptinites, or which effect a schistous structure.

Cordierite, which is usually a rare mineral, is frequently found in the Vosges granite; it is sometimes in crystals, sometimes in irregular lumps, as well as crystalline; these latter, sometimes decomposed on their surface, show a passage to pinite, which, according to the observations of many mineralogists, confirmed by those of M. Delesse, should be considered as altered cordierite. This interesting observation gives a key to the various opinions held respecting pinite; the differences found in it depend upon the more or less advanced stage of composition to which the cordierite has attained.

Chlorite, crystallised graphite and febrilite, are likewise minerals found in the Vosges granite.

The investigations of M. Delesse, although chiefly respecting the chain of the Vosges, are directly applicable to other groups of ancient mountains, which circumstance augments the interest attaching to them; in the south of France and in Bretagne two classes of granite have been found, analogous to those of the Vosges. M. Delesse has himself shown, that an analogous division exists in Normandy; and on the right bank of the Rhine, he finds evidence of two distinct periods of granitic formation; they are characterised, like those of the Vosges,

by the presence of a black or very dark-green mica in one, and in the other by the co-existence of two micas, differing essentially in all their characters.

Comptes Rendus.

ORGANIC CHEMISTRY.

On the AMIDIC ACIDS of the MONOBASIC ACIDS.

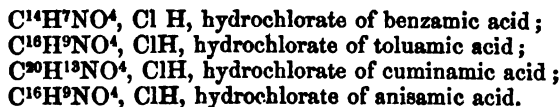
By M. AUGUSTE CAHOURS.

THE amidic acids derived from benzoic, toluic, anisic acids, &c. by the reduction of nitrobenzoic, nitrotoluic, nitranisic acids, &c., whether by means of hydrosulphate of ammonia, or by means of acetate of protoxide of iron, and which are known by the names benzamic, toluamic, anisamic acids, &c., behave, as far as we can judge from the following facts, like true alkaloids, and may be ranked next to glycocolle, alanine, and leucine.

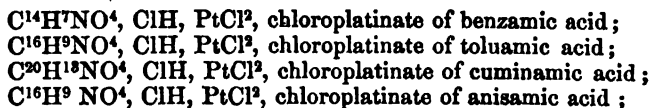
The study of benzamic acid, lately made by M. Gerland, having shown that this substance possesses the property of forming crystallisable compounds with nitric and sulphuric acids, I thought that it might be possible to produce similar compounds with various acids in the same manner as glycocolle, and that the other acids which are analogous in their formation and composition might probably behave in the same manner.

I ascertained that benzamic acid forms definite compounds with phosphoric, oxalic, hydrobromic, hydrochloric acids, etc. I have obtained similar combinations between these various mineral acids and toluamic, cuminamic, and anisamic acids, which crystallise with great facility.

The hydrochlorates formed by these various amidic acids present a composition analogous to that of the hydrochlorates of the alkaloids; it is expressed by the formulæ:—



These various hydrochlorates combine with bichloride of platinum and form very clearly crystallised products whose composition is represented by the formulæ:—



The first two crystallise in fine needles of a golden yellow, the third affects the form of orange colored needles; finally, the fourth crystallises in prisms of a brownish red, which sometimes become very voluminous.

These compounds are most easily obtained by treating the amidic acid with a slight excess of concentrated hydrochloric acid, adding sufficient alcohol to entirely redissolve the hydrochlorate at the boiling temperature, then pouring in an excess of bichloride of platinum; by evaporation the double salt separates entirely under the form of crystals.

An experiment of the same description made with hydrochlorate of glyocolle gave me similar results. We obtain a chloroplatinate under the form of brilliant prisms, of the most beautiful orange, containing 35 per cent. of platinum, and whose composition, expressed by the formula



corresponds perfectly with that of the preceding compounds.

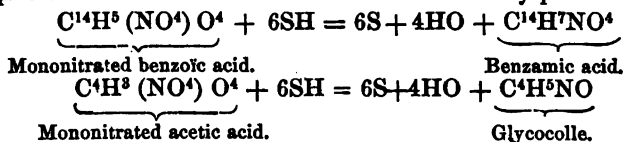
To these characters, which manifestly approximate these various combinations, we will add the following observation; their sulphates have a characteristic sugary taste.

In the same manner as glyocolle, which presents numerous isomers, benzoic acid gives us two cases of very distinct similarity; anthranilic acid and salicylamide.

Benzoic acid showing with regard to salicylamide the same relations as those observed between glyocolle and glycollamide, between alanine and lactamide, we might ask what causes the differences which are remarked between bodies possessing at once the same centesimal composition and the same chemical equivalent.

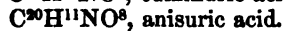
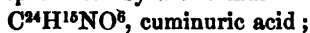
Now salicylic acid in being transformed into salicylamide loses O^2 , which it exchanges for NH , whereas benzoic acid, in being transformed into benzoic acid, gains NH , without losing the oxygen of its molecule. Hence it will be seen that the relative position of the atoms would be very different in these two compounds which sufficiently accounts for the different properties which they exhibit.

Glycollamide being produced in the same manner as salicylamide, by the reduction of glycollic acid by means of ammonia, might we not hope to obtain glyocolle by a process similar to that which gives benzoic acid—namely, by reducing mononitrated acetic acid by hydrosulphuric acid. The two reactions should be entirely parallel:—



Chloride of benzoïle engenders, in reacting on zinc glyocolle, according to the curious observations of M. Dessaignes, the acid found in the urine of herbivorous animals, and which has long been known by the name of hippuric acid. I have ascertained that the chlorides of cumyle and anisyle furnish analogous products by their reaction on glyocolle of silver. We thus obtain distinctly crystallised acids capable of forming crystallisable salts, and decomposing in the manner of hippuric acid, under the simultaneous influence of acids

and heat into glyocolle and cuminic or anisic acid. The composition of these products is represented by the formulæ :—



There still remained to ascertain that the chlorides of the same radicals in reacting on a benzamate, a cuminate, an anisamate, would give analogous results.

Experiment has fully proved these facts.

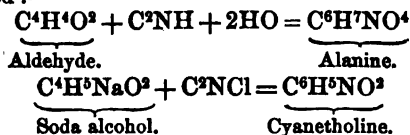
If we cause chloride of benzoïle to act upon benzamate of silver, the mixture heats strongly, in consequence of the reciprocal decomposition which takes place in these two substances; we thus obtain chloride of silver, as well as a new acid which may be isolated by taking up the pure residue with concentrated alcohol, which transforms into benzoïc ether the excess of chloride of benzoïle employed. The liquor resulting from the filtration is evaporated, then treated with caustic ammonia, which dissolves the acid without touching the benzoïc ether. The solution is finally treated by an excess of hydrochloric acid, which causes the acid to be deposited, which is then purified by washing and crystallisations in alcohol.

The result of the facts described in this work is, that between glyocolle, alanine, leucine, and the amidic acids formed by the monobasic acids, there are strong links of connection, whatever may be besides the rational constitution of these compounds, which it would be impossible to tell at present.

We have on both sides limited bodies which, in the manner of hydrate of phenyle, combine indifferently with the acids and bases, and will even unite with salts.

It is probable that we could form these compounds by very various processes. It would be curious, among other methods, to examine whether the new isomer of cyanic ether recently discovered by M. Cloëz, by means of the reciprocal action of chloride of cyanogen and soda alcohol, might not be transformed into alanine by the simple fixation of water, then to reproduce with its homologues glyocolle, leucine, &c. The method of production of cyanetholine and alanine presents, undoubtedly, the most evident analogies.

We have indeed :—



If we can only succeed in fixing on the molecule of aldehyde the molecule of hydrocyanic acid without the intervention of water, everything leads us to believe that this would produce cyanetholine; the connection between these two substances being in every respect comparable to that observed between an amide and its nitrite—between an alcohol and the carburet of hydrogen which corresponds to it.

Comptes Rendus.

CHEMICAL HISTORY of COLCHICUM. By M. L. OBERLIN.

IN 1820, in continuation of the chemical examination of several vegetables of the family of colchicaceæ, MM. Pelletier and Caventou assimilated the principle found in *colchicum autumnale* with that discovered by them in other plants of this family, indicating the presence of veratrine in this plant.

In 1833, MM. Hess and Geiger showed the difference which separates the active principle of colchicum from that which hellebore and sabadilla contain, giving the physical and chemical characters of the alkaloid which they called colchicine. I have resumed this study. But although I have carefully followed the process of extraction which they employed, I have not obtained a crystallisable principle, even by trying all the solvents for facilitating crystallisation, and two chemists, who have been kind enough, at my solicitation, to repeat the experiment, have arrived at the same negative result. The following are the results of my researches on this uncrystallisable product obtained by the process of Hess and Geiger.

The aqueous solution of colchicine acidified with sulphuric or hydrochloric acid is of an intense yellow color: when it is reduced to a certain degree of concentration in the sand bath, it precipitates of a yellowish white with water; this, well washed and freed from coloring matter, is soluble in alcohol and in ether, and crystallises easily. A larger produce is obtained by using hydrochloric acid, and by allowing the reaction to proceed spontaneously. After a few weeks, a great many acicular crystals are found. These crystals, powdered and well washed, give at the first recrystallisation an almost white product, whilst, if we use sulphuric acid, we obtain only, with great difficulty, white and pearly crystals like the specimen, the resinous and coloring matter which accompanies this product being eliminated with difficulty.

This crystalline principle is not a salt; it is neutral, and the reagents indicate in it no trace of the acids employed in its preparation. I call it colchiceïne, for it differs from the colchicine of Hess and Geiger, which, as I have shown in my note, appears to be a complex compound.

The properties of colchiceïne are: to crystallise very easily in pearly lamellæ, to be completely insoluble in water, but to communicate to this vehicle a slight bitterness which increases perceptibly when it is carried to ebullition. At this degree it dissolves a considerable portion of the product, which is deposited immediately after cooling. The solvents of colchiceïne are alcohol, ether, methylic alcohol, and chloroform, which contract, in its presence, a very intense and persistent bitterness.

The alcoholic solution of colchiceïne is colored by the addition of bichloride of platinum, and no precipitate is formed. Pure and concentrated nitric acid dissolves colchiceïne; it is colored of a very intense yellow, passes to a violet color, then to dark red and light red, and finally to its original yellow color.

Concentrated sulphuric acid forms a solution of very intense yellow, which is preserved even when it is diluted with water; brownish flocks are formed after a long time. Hydrochloric acid dissolves it with a light yellow color. Finally, acetic acid likewise dissolves it, but without change of color.

Cochicine is soluble in ammonia and crystallises by evaporation in the air; it is soluble in caustic potassa, is colored green by chloride of iron and does not present any change of color, nor does it become turbid in presence of solutions of neutral trisacetate of lead, nitrate of silver, chloride of mercury, and infusion of galls.

Colchicine appears to be capable of combining with baryta, for when we mix the saturated solutions of hydrate of baryta and colchicine in methylic alcohol, a bulky, gelatiniform precipitate is very soon formed, soluble in the wood spirit, and even in an excess of the baryta solution.

Colchicine is unalterable in the air; it is without action on reddened or blue litmus paper. Exposed in a tube to the temperature of an oil-bath, it softens at once, and enters into fusion at 155° C. (311° F.); on the temperature being increased, it becomes colored at about 200° C. (392° F.) Heated on a platinum plate, it fuses, acquires a yellow color, and burns, leaving only a stain which has no action on moistened test papers. Heated with caustic potassa it disengages a gas which restores the blue color of reddened litmus. Calcined with potassa, we obtain, by following M. Lassaigne's process, a residue which, treated with a solution of a persalt of iron and afterwards with hydrochloric acid, produces a blue liquor, the deposit from which is Prussian blue.

The presence of nitrogen being demonstrated by these two experiments, I submitted colchicine to elementary analysis. The following are the results:—

Calculation.	Mean of experiments.
C = 62.83	C = 62.669
H = 6.60	H = 6.560
N = 4.19	N = 4.298
O = 26.38	O = 26.473
100.00	100.000

Pre-existence of Colchicine in the Seeds of Colchicum.—As this substance might be the result of the action of the reagents on the normal materials of the seeds of colchicum, I endeavored to assure myself of its pre-existence; this I think I have done by proceeding in the following manner:—

I dissolved the alcoholic extract of the seeds of colchicum previously deprived of oil and amylaceous matter, in alcohol, and decolorized it with well washed charcoal; after filtration, the charcoal was repeated, treated with boiling alcohol, and the product added to the first liquor. The syrupy extract which remained after distillation, dissolved in water and slightly acidulated with very dilute sulphuric

acid, furnished a flocculent precipitate; this liquid, filtered, yielded, after a few weeks, the mammilated crystals already mentioned. These last crystallised in alcohol, and were identical with colchicine.

The action of the acids on analogous bodies having given rise to splittings from which peculiar principles and glucose have resulted, I sought to ascertain the presence of the latter body in the mother liquors from which the crystals of colchicine were deposited; but I only succeeded in isolating a resinous matter insoluble in water, combined with the coloring matter, and very soluble in the alkalis—a product, consequently, of an acid nature. The characters of this resinous matter are, to be soluble in alcohol and ether, to be colored blood red with nitric acid, to dissolve in ammonia, likewise taking a very intense red color.

Physiological Experiments.—From the experiments made by Professor Schroff, of Vienna, the colchicine obtained by the process of Geiger and Hess has a poisonous action on rabbits in the dose of one hundredth of a gramme (about $1\frac{1}{2}$ grains); the animal died in from twelve to eighteen hours. Given in larger doses, even as much as 1 gramme, it caused death only after seven or eight hours.

When one hundredth of a gramme of colchicine is administered, the animal dies after ten or twelve hours; but in the dose of 5 centigrammes it causes complete paralysis of the limbs, and death ensues in a few minutes.

Repertoire de Pharmacie.

ACTION of BICHLORIDE of TIN on STARCH.

By M. H. DE PAYR.

ON wetting a microscopic slice of a horse-chestnut with a little bichloride of tin, the fecula is observed to disappear. This observation, which was made by Rochleder, has led to the following researches being made by M. Payr.

On mixing starch with bichloride of tin in a mortar, the starch is almost immediately dissolved; a slight pitchy residue forms in the solution, which may be separated by decantation. After having filtered the liquid portion, and having added anhydrous alcohol, an abundant precipitate is produced, of a brilliant white, which, washed with anhydrous alcohol, and dried *in vacuo* over sulphuric acid, is found to be composed in 100 parts of:—

C . . .	23.41	23.20
H . . .	4.11	4.16
O . . .	83.81	83.97
Sn. . .	38.67	38.50

On decomposing this matter by a current of sulphuretted-hydrogen a limpid liquid is produced, which is converted *in vacuo* into a white, friable mass, which the author found to be formed of:—

C . . .	40.90
H . . .	5.67
O . . .	52.43

Whence he deduces the formula



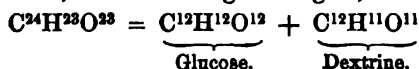
By treating starch with bichloride of tin at 100°C. (212°F.), a starch paste is obtained which afterwards liquifies; alcohol separates from it a white compound containing:—

C . . .	15.56	15.44
H . . .	3.05	3.30
O . . .	24.31	24.18
SnO. . .	57.08	57.08

The precipitate which absolute alcohol produces in this solution made with the aid of heat amounts to scarcely one-twelfth part of that formed with the solution made without heat.

The compound $\text{C}^{24}\text{H}^{23}\text{O}^{23}$, obtained with the salt made without heat, is not starch, for it is soluble in water, and is not turned blue by iodine. Neither is it dextrine or glucose; but, under the influence of the weak acids, it is readily converted into the latter.

From its properties and its composition, it takes its place between glucose and dextrine, or between sugar and gum, for



and, like glucose, it is composed of fecula and water.

Journal für Praktische Chemie.

On the IDENTITY of SANGUINARINE with CHELERYTHRINE.

By M. SCHIEL.

SANGUINARINE was discovered by Dana, who found it in the root of *sanguinaria canadensis*. M. Schiel, who established its composition fifteen years ago, has just ascertained that this composition agrees with that of *chelerythrine*, the alkaloid extracted from the *chelidonium majus*, and also called pyrrhopine. The following is the formula attributed by M. Schiel to their alkaloid:—



The author recommends the following process for its preparation.—

The root of *sanguinaria canadensis* or of *chelidonium majus* is set to macerate with water strongly acidulated with sulphuric acid; then after being expressed, it is treated with ammonia; the sanguinarine is precipitated, washed with a large quantity of water and dissolved in ether; the solution is decolorized with a little animal charcoal, and ether containing sulphuric acid is added.

The sulphate of sanguinarine, being insoluble in ether, is precipitated; after being washed with ether, it is dissolved in water and precipitated for the last time by means of ammonia: the product is pure sanguinarine.

Journal de Pharmacie.

INORGANIC CHEMISTRY.

On the FORMATION of INSOLUBLE SULPHUR, under the INFLUENCE of HEAT. By M. BERTHELOT.

THE singular phenomena which are presented by sulphur, when subjected to the action of heat,—the gradual increase of its viscidness, and its coloration, in proportion to the elevation of the temperature,—finally, its transformation into soft sulphur and insoluble sulphur, when suddenly cooled,—have formed the subject of investigation to many men of science. I shall quote some of their results further on.

Without repeating these various observations, I formed the plan of studying what influence temperature exercised in the formation of insoluble sulphur,—how far the phenomena presented by heated sulphur are connected with the production of this peculiar state of sulphur,—and finally, whether it would be possible to render it in any degree similar to the insoluble sulphur formed by the humid way. This investigation appeared to me the more useful, as insoluble sulphur is distinguishable from soft sulphur by its permanence at the ordinary temperature, and by its completely different relations to octohedral sulphur, with respect to the quantities of heat which it is capable of disengaging.

Sulphur, melted at 130° or 140° C. (266° or 284° F.), then suddenly cooled, remains quite soluble and crystallisable in sulphuret of carbon.

Sulphur, melted at 155° C. (307° F.), etc., contains a trace of insoluble sulphur.

Sulphur, melted at 163° C. (325° F.), etc., contains a small quantity of insoluble sulphur.

Sulphur, melted at 170° C. (338° F.), etc., contains a considerable quantity of insoluble sulphur. The sulphocarbonic solution, when evaporated, abandons crystallisable sulphur and some traces of sulphur which has become insoluble.

The existence of a variety of soft sulphur, soluble in sulphuret of carbon, but becoming insoluble during evaporation, was discovered by M. Magnus. The soft sulphur of the hyposulphites presents analogous characters, but more distinct.

Sulphur, melted at 185°, 205°, 217°, 230°, and 250° C. (365°, 401°, 422°, 446°, and 482° F.), etc., contains large quantities of insoluble sulphur. The proportion of insoluble sulphur formed 170°, 185°, 205°, and 230° C. (338°, 365°, 401°, and 422° F.), was found to be nearly the same in similar conditions.

I have not considered it necessary to push these experiments farther; the sulphur melted at 300° and 360° C. (572° and 680° F.), according to MM. Ch. Deville, Schrötter, and Magnus, furnishes a large quantity of insoluble sulphur, which establishes the continuity of its formation up to those elevated temperatures.

The result of the preceding experiments is, that the formation of insoluble sulphur commences at about 155° C. (307° F.); but it is then extremely slight. Towards 170° C. (338° F.), this formation is

on the contrary very considerable, and continues so to the highest temperatures. It is consequently towards 170° C. (338° F.) that sulphur assumes the peculiar state which corresponds to insoluble sulphur.

Now it is precisely at about the same temperature that fused sulphur assumes the characters of viscidness and coloration, according to the observations of Bellani, Dumas, Fuchs, and Ch. Deville. It is likewise at about the same temperature that soft sulphur is formed, the correlation of which with the thickening and coloration of the sulphur has been shown by M. Dumas. Between 150° and 200° C. (302° and 392° F.), according to the experiments of M. Despretz, the coefficient of dilatation of sulphur undergoes a considerable diminution, and passes to a very remarkable minimum. Finally, the experiments of M. Ch. Deville, on the rapidity of reheating and the rapidity of recooling fused sulphur, likewise shows a singular point about this temperature.

This accordance of so many experiments made from various points of view and at distant periods, is well worthy of observation. It shows that the increasing viscidness of the sulphur,—the progress of its dilatation,—that of its heating and cooling again,—and finally, the formation of soft sulphur and that of insoluble sulphur, are correlative phenomena; they are produced simultaneously, and at about the same limits of temperature.

We are thus led to think that the permanent states in which sulphur presents itself at the ordinary temperature are not accidental, and due to purely physical causes; I mean to say, to sudden cooling and an abnormal conservation of latent heat. This hypothesis, which appeared very probable when we were only acquainted with soft sulphur, still remains applicable to it; but it will neither explain the formation of insoluble sulphur—a more stable and permanent state—nor the circumstances relative to the part played by this sulphur in combinations. When this substance is produced under the influence of heat, its origin appears connected with the state of sulphur at about 170° C. (338° F.), and above that; at about this temperature, the nature of sulphur is changed: up to this heat, it possessed the molecular state corresponding to crystallisable sulphur, playing the part of an element supporting combustion; but under the influence of heat, the conditions of its stability are modified, and certain properties are manifested, corresponding to insoluble sulphur, playing the part of a combustible element. Reciprocally, sulphur cooled slowly to below 170° C. (338° F.), repasses to the state of fluid sulphur, corresponding to crystallisable sulphur, but without returning to it instantaneously. If cooled suddenly, it passes through the period of liquidity, now too short, without an entire change of nature, and a portion of the solidified sulphur preserves a molecular state, more or less analogous to that which the matter possessed at 170° C. (338° F.). This is amorphous and insoluble, whose existence is preceded by that of a corresponding soft sulphur.

These considerations give a faithful representation of the phenomena which sulphur manifests under the influence of heat; they lead

us to attribute them to chemical transformation, properly so called. To establish this opinion on a more solid basis, it would be necessary to prove that the whole of the sulphur is found at about 170°C . (338°F .), and above that, in a state corresponding to insoluble sulphur. Now, in ordinary conditions, sulphur suddenly cooled contains at most 30 or 40 per cent. of insoluble sulphur. So partial a result has been already explained by the consideration that sulphur necessarily passes through the period of liquidity below 170°C . (338°F .), during which it has a tendency to return to the state corresponding to crystallised sulphur. But we can still more completely foresee the transformation by closely studying the conditions in which it is performed. Indeed, the proportion of insoluble sulphur varies extremely even in the same operation, according to the duration of the cooling, which depends on the relation between the mass of the sulphur and its surface, its state of combustion when poured out, the calorific conductivity of the liquid into which it is poured, the temperature at which that liquid boils, etc.

Thus, for example, sulphur reduced into very fine threads or granules when being poured into water, is much richer in insoluble sulphur than if poured without these precautions; it will contain as much as 61 per cent. of insoluble sulphur.

If the fused sulphur is poured into ether, the cooling is rendered very rapid by the vaporisation of this liquid and by its low boiling point; moreover, the vapors of ether so rapidly formed reduce the sulphur into very thin pellicles. In these conditions we obtain as much as 71 per cent. of insoluble sulphur.

We may go still further, supporting our opinions on some very delicate considerations on the stability of insoluble sulphur formed by heat, and on the connection existing between this substance and the bodies which are capable of modifying it by contact, by predisposing affinity, or, more generally, by virtue of their electro-negative properties.

In the transformation of fused sulphur into insoluble sulphur, two successive phases are observed; at first the suddenly cooled sulphur remains soft, transparent, and elastic; if it is at this moment reduced into very thin threads or small granules, it is almost entirely insoluble in sulphuret of carbon. But, by degrees, especially in contact with a solvent, the sulphur bleaches and crystallises, and becomes in great part soluble in sulphuret of carbon.

I thought that it would be possible to prevent this second period of the transformation so as to obtain, in a solid state, almost all the insoluble sulphur at first formed: it is sufficient to augment its stability by contact with certain electro-negative bodies.

Thus the insoluble sulphur, prepared by cooling the fused sulphur, constitutes the least stable of all the varieties. Boiling alcohol dissolves it according to M. Ch. Deville's observation. It is even sufficient, as I have shown, to boil it for a few minutes with a small quantity of absolute alcohol, to change, by action of contact, the undissolved portion into sulphur soluble in sulphuret of carbon and

crystallisable. By this action of alcohol and several others, the insoluble sulphur obtained by heat is distinguished from the more stable state taken by combustible sulphur; namely, that of sulphur extracted from the chloride and bromide of sulphur. But all the varieties of insoluble sulphur may be brought to this limited state by the contact of certain electro-negative bodies. We can even, without attaining this extreme term, augment the stability of insoluble sulphur obtained by heat, and communicate to it properties analogous to those of the insoluble sulphur extracted from flowers of sulphur, a less stable variety than that of the chloride; but which, nevertheless, resists the action of alcohol. This variety presents, as to color, the same characters as the insoluble red sulphur of M. Magnus. The insoluble sulphur obtained by heat only needs to be kept in contact for a few days with the powerful mineral acids, and especially with sulphurous acid and fuming nitric acid.

Acting on these properties, I have succeeded in obtaining the maximum of insoluble sulphur. Sulphur, poured into water in very small granules, then kept under a layer of fuming nitric acid or of sulphurous acid, furnished as much as 75 per cent. (in nitric acid), and even to 86 per cent. (sulphurous acid) of distinctly insoluble sulphur.

The preceding facts confirm the connection which I have endeavored to establish between the formation of insoluble sulphur by the humid way, and the formation of analogous insoluble sulphur produced under the influence of heat. Moreover, they prove that if suddenly cooled sulphur cannot be changed completely into insoluble sulphur, by reason of the circumstances themselves in which it is cooled, we may at least obtain the greater part of the sulphur in an insoluble form in proper conditions.

Comptes Rendus.

INDUSTRIAL CHEMISTRY.

REPORT on a MEMOIR entitled—CHEMICAL RESEARCHES on WHEAT, its FLOUR, and PURIFICATION, by M. MEGE MOURIES.

By MM. DUMAS, PELOUZE, PAYEN, PELIGOT, and CHEVREUL, Reporter.

(Concluded from page 498).

A.—Panification by the Old Process.

Brown Bread.—The flour which gives the brown bread containing all the proximate principles of the grain of the wheat is consequently ready to undergo the greatest change from the action of those proximate principles which play the part of *ferments*.

The *cerealine*, which is the most powerful ferment contained in wheat flour, being in much larger proportion in whole flour than in that deprived of its bran, it is not surprising that its action should predominate over that of the *caseine* and *gluten*, which likewise act as ferments.

The consequence is the predominance of the lactic fermentation over the dextric, glucosic, and alcoholic fermentation which the caseine and gluten are calculated to produce.

This predominance is such that more dextrine, more glucose, and more lactic acid are produced in proportion than alcohol and carbonic acid gas from the yeast, and then ammonia and a brown substance are produced at the expense of the gluten; finally a portion of the gluten itself passes to the state of *lactic ferment*, and during the baking, starch is again transformed into dextrine and glucose.

In this manner, this reaction explains the coloration of brown bread and the development of ammonia; we see likewise how the diminution of the gluten and its alteration and the predominance of soluble matters—such as dextrine and glucose—explain the want of firmness of the crumb of brown bread, its softness, its sticky state, and its unfitness for use in soup.

We see also how much foundation there is for the opinion of the superior nutritive properties of brown bread. Even if there is more nitrogenous matter in the flour used in its preparation than in white flour, this would not be a reason for concluding that brown bread contains more nutritive properties than white, because, by the operation of panification, the nitrogenous principles are susceptible of being altered, and giving ammonia among other products.

White Bread.—The white flour with which bread of the first quality is made contains scarcely any cerealine. This, as we have said, having been almost or entirely removed with the bran, white flour is consequently in a favorable condition for the *alcoholic fermentation* indispensable to the rising of the bread, and which thus predominates over the *lactic fermentation*.

The dough from white flour, before being put into the oven, undergoes three fermentations—*alcoholic, acetic, and lactic*.

The first should predominate over the two others. It is made at the expense of the glucose, which if did not exist in wheat has been developed afterwards in the flour; it is caused by the *gluten ferment*, when no yeast has been added to the dough. For the proper action of the alcoholic fermentation it is necessary, during the time that the dough is being divided into loaves, that the quantity of carbonic acid gas necessary for raising the bread without breaking the upper layer should be produced—this upper layer forming the crust when baked; now this condition can only be fulfilled while the gluten retains its tenacity.

M. Mège Mouriès, after speaking of the inconvenience of a too great development of the carbonic acid which raises the bread and breaks it, speaks of the mischief of too acid and too much fermented yeast which is disposed to produce *lactic fermentation*—thus producing a more or less brown bread from a white flour.

B.—Panification by M. Mège Mouriès' Plan.

The process of M. Mège Mouriès consisting in three principal operations—1st, the grinding; 2nd, the preparation of the dough

with white flour and the water in which whole flour has fermented; and 3rd, the baking of the raised dough, is more simple than the old processes by means of which white and brown bread are produced, as we shall see in the second part.

1st. The Grinding.—In this process the corn only passes once through the mill; one single bolting is sufficient to obtain, 1st, fine white flour; 2nd, brown flour; and 3rd, the large and medium sized brans.

2nd. The Preparation of the Dough.—Subject to alcoholic fermentation brown flour with four times its weight of water, in which yeast and glucose have previously fermented; this neutralises the cerealine, if not absolutely, at least the greater part of its lactic activity; then separate the fine bran; finally, add to white flour the fermented water of the brown flour, with its deposit, and we have a dough which represents all the farinaceous part of the wheat.

The advantage of this process is not only the separation of the fine bran, but the neutralisation of the cerealine, and the production of a fresh quantity of yeast sufficient for giving to the wheat flour the degree of alcoholic fermentation necessary to raise the dough.

The yeast and glucose added to the brown flour water cause the neutralisation of the cerealine, and the proof of this is that on leaving from three to five parts of bran in the dough, we have, instead of a brown bread, a bread the crumb of which is incontestably white, as we have already shown. On the other hand, if the fermentation neutralises the yeast, it likewise produces a larger quantity than that neutralised. Thence this fermented water causes the movement of alcoholic fermentation to take place in the dough produced by a mixture of the various kinds of flour produced by wheat.

This explains the lightness of M. Mège Mourriès' bread.

3rd. Finally, the baking is proceeded with in the old manner.

Reflections on the Theory of Panification.

If the present state of science does not permit us to pronounce with certainty as to the peculiar nature of ferments, their number, and the precise characters of their respective species, the researches of M. Mège Mourriès show to a demonstration, that panification rests entirely on a suitable fermentation of the dough of wheat flour, and that on the manner in which this fermentation is conducted depends the whiteness or color of the bread.

If there is no doubt that cerealine, caseine, and gluten, under a still almost unknown influence, become ferments, acting, as we have seen, in a different manner under the same circumstances, the greatest uncertainty still exists as to the essential properties of cerealine, caseine, and even of gluten, obtained in a defined state of purity: consequently, we do not see clearly or precisely how caseine, although with a less powerful action than cerealine, produces with starch, dextrine, glucose, and lactic acid—how gluten, which, under the influence of the air, becomes an alcoholic ferment, becoming altered by the influence of the cerealine is transformed into ammonia,

a brown matter, and a substance which possesses activity as a *lactic ferment*. We have still to discover whether a nitrogenous matter, in passing through different molecular states, would not be liable to produce different effects of fermentation, of which one would correspond with each of these states.

We do not raise these questions as a criticism on M. Mège Mourès work, but on the contrary, to show its merit and novelty. For before him, these questions could not have been put so clearly and distinctly as now; moreover, it is the property of original works to multiply our points as we advance into hitherto unknown paths.

II.—THE RESEARCHES OF M. MÈGE MOURIÈS IN A PRACTICAL POINT OF VIEW.

To appreciate the importance of this work, we must treat of grinding such as it now is and such as it should be for the new process.

1st. *Of the Grinding of the Wheat.*

The following are the results of four operations of grinding made under the direction of MM. Mège Mourès and Salone. We shall here name only three products, *white flour, crude or brown meal, and various brans* per cent.

First Operation.—Made with new corn, weighing 80 kilos. to the hectolitre:—

White flour	73.899
Crude or brown meal	15.957
Bran	10.144

100.000

Second Operation.—Made with old corn of medium quality weighing 78.660 kilos. the hectolitre:—

White flour	74.800
Brown meal	12.390
Bran	13.310

100.000

Third Operation.—Old wheat of middling quality, weighing 78.660 kilos. to the hectolitre:—

White flour	72.060
Brown meal	14.250
Bran	13.690

100.000

Fourth Operation:—

White flour	72.720
Brown meal	15.720
Bran	11.560

100.000

Grinding, according to M. Mège Mouriès, is much simplified, consisting, as we have already shown, of only one passage through the stones and one bolting, giving these products—the *finest flour and white meal*, the *brown meal*, and the *various kinds of bran mixed*.

This simplicity is evidently very favorable to those who send the corn to be ground; because, with the exception of a very slight loss which is easily appreciated, the sum of the three products should represent the grain, and, moreover, their respective proportions control each other.

2nd. Panification by the Old and New Processes executed at Scipion, (Bakery to the Hospitals of Paris.)

Four operations were performed here; the last three were comparative. We must commence by observing that M. Mège Mouriès strictly adhered to the process already described.

The following are the results of these four operations per cent. of *ground corn*. In the documents succeeding this report we shall give the real weights of the flour subjected to panification as well as the manner in which the results were arranged per cent. These numbers are exactly ascertained by MM. Mège Mouriès and Salone:

	1st.	2nd.		3rd.		4th.	
	N.	O.	N.	O.	N.	O.	N.
Weight of dough		113	131	112	132	113	131
Weight of cold bread	109	90	109	93	113	92	109
Difference in favor of new process	"	"	19	"	20	"	17

The letter O signifies old process—N new process.

Before examining these figures, let us speak of the real difficulties presented by the appreciation of experiments whose object is to compare two different methods of panification.

Because it is incontestible that new wheat flour does not give, *ceteris paribus*, as white and as firm a bread as that of a less new flour, it is necessary, in comparing two processes of panification, to operate with the same flour; otherwise, errors may creep in, not only as to the age of the flour, but also as to its nature. Consequently, to judge of the bread prepared by the new process, it must not be compared with bread bought indiscriminately, the quality of the flour of which cannot be known.

On the other hand experiments made to ascertain the yield of two processes require long habit on the part of the baker, when we do not wish to make a long series of comparative experiments; because it is very difficult to bake so that the evaporation of water may be similar from each loaf.

We have consequently done all we could to avoid errors and to lessen them. We therefore made three comparative experiments with the same flour, and the last was peculiarly careful for the following reason. Instead of mixing the flour for the old process successively with the *chief yeast, first yeast, second yeast, and entire yeast*,

to cause the fermentation necessary to raise the bread, beer yeast was mixed in, that is to say the same ferment used for flour panified by the new process. Finally, although we consider that comparative experiments are alone conclusive, we have given the first because in questions so difficult to determine on a large scale as those connected with the yield of flour in bread, and being unable to make every experiment that we might desire, one experiment is not to be despised, especially when it gives results in conformity with the other three.

We feel that confidence may be placed in the results which we obtained, because of the agreement between the four operations made at Scipion, and the small difference existing between the numbers, 19, 20, and 17, which give the different yields of the baked bread.

As we wish to avoid anything that may lead to an idea that we give to these experiments an absolute determination of the yield of the two processes which we have compared, we beg to state that these figures do not represent the exact amount which 100 parts of flour subjected to the two processes will always give, considering the nature and humidity of the flour.

The Properties of the New Bread as compared with the Old.

The first comparison to be made between the two breads concerns the proportion of water which they are likely to lose by exposure to the air; for it might be said that it would not be possible for a notable quantity of nutritive matter to disappear, being transformed into evaporable matter in the new process; and that, in consequence of this circumstance or of some other, there would be more water in fresh bread than in stale. Experiment answered this question as follows:—

100 parts of crumb of new bread lost	. .	87.5 of water
„ „ stale „	. . .	37.8 „
100 parts of crust of new bread lost	. . .	14.2 „
„ „ stale „	. . .	12.0 „

These differences are quite insignificant.

M. Péligot found that at 120° C. (248° F.)

100 parts of new bread, crust and crumb, lost	. .	34.9
100 parts of stale „	. . .	34.1

M. Payen obtained very nearly the same result.

Finally, M. Mège Mouriès has always observed that his bread, like the common bread, lost at a temperature of 25° to 80° C. (79° to 86° F.), 80 per cent. of the water.

The new bread is lighter, and has rather more flavor than that made by the new process. We are unanimously of opinion that the flavor of new bread is more agreeable. We are likewise quite sure that it is very wholesome. The Rev. M. Hamon, Priest of Saint Sulpice and Superior of the Orphanage of Saint Charles, and Dr. Blatin, Physician to that same Institution, have given us a certificate which is

sufficient to dissipate any doubts as to its wholesomeness; they say that having used it daily for six months in the Orphanage, containing 100 children of from 2 to 9 years of age and 15 sisters, they have found it of an agreeable flavor, very nourishing, easy of digestion, and keeping well; the health of the sisters and children has been excellent.

Summary.

1st. The coloration of brown bread does not depend on the presence of bran in the bread, but on a peculiar fermentation of the flour—a fermentation which is produced by cerealine or a too great action of the rising of white dough. Two facts prove this; that by paralyzing the action of the cerealine of the bran we can make a white bread of flour containing it; in the second place, by allowing the fermentation to continue too long, a brown bread may be made from flour which contains no bran, as occurred at Scipion.

2nd. The process of grinding which suits the new process, much more simple than ordinary *grinding*, must necessarily be very advantageous to those consumers who have their corn ground, because it only requires the corn to be ground and bolted once.

3rd. The preparation of the dough by the new process is likewise much more simple than by the old; because, instead of requiring four operations—all of a difficult and delicate nature—it is only necessary to give to the brown meal mixed with water a movement of fermentation by means of yeast, to pass the fermented liquor through a sieve, to separate the fine bran, and then to use it for making dough with white flour, which then rises well.

4th. The result of the new process is to get from 100 parts of corn 86 or 88 parts of flour fit for making white bread, instead of 70 or 74 which was obtained by the old process. In the three operations made at Scipion, 100 parts of corn yielded 19, 20, and 17 parts more bread by the new process than by the old.

So much for white bread: but the new process has this advantage over the old—that it makes a bread which very nearly approximates to white bread in appearance, even when it contains the same quantity of bran as the brown bread of the old process. Doubtless in the country and wherever brown bread is much eaten the new process will be followed without removing the bran, by passing the fermented water through a sieve.

5th. We think that we have said sufficient to recommend the process for economy, agreeableness, and wholesomeness. This is a very important work; it is based on chemical experiments peculiar to M. Mège Mouries, and is in conformity with the most recent discoveries of organic chemistry; it has received the sanction of experience, having been in use for nearly a year, and we trust that the Academy will signify its approbation of M. Mège Mouries' labors.

Comptes Rendus.

THEORETICAL and PRACTICAL STUDIES on PRINTING, DRESSING and PAINTING.

By M. FRED. KUHLMANN.

HAVING since the year 1841 shown the utility of the soluble silicates in hardening stones and giving a longer duration to our buildings, I in 1855 called the attention of the Academy to the application of these same agents, to dressing and painting.

I have since examined the question of the fixation of colors with an exclusive view to painting and dyeing. I shall now show that my last investigations may be made useful as establishing some points of contact between the chemical operations which compose dyeing, properly so called, and the operations, hitherto exclusively mechanical and artistic, of painting and dressing. The Academy will judge whether I have too highly estimated the utility of the intervention of chemical reactions in processes which have now been long in use without requiring any serious modification.

After having ascertained by numerous experiments the influence of animal matters, and especially of albumen and caseum on the fixation of colors in dyeing, I wished to complete my demonstrations on this point by repeating my experiments, replacing these latter bodies by gelatine. Not being able in this place to coagulate the animal matter on the stuff by heat, with or without the assistance of an acid, I had recourse to a well known reaction, that of tannin, which transforms the gelatine into an elastic matter, insoluble in water, a real artificial leather.

By this chemical stratagem, I obtained the double result of permitting, by means of the animal matter, a more easy absorption of the coloring matters, and fixing simultaneously on the stuff a considerable quantity of tannin. The chemical action of this tannin on certain metallic salts, whether they enter into the composition of the mordants, or whether they serve as a dyeing bath, may be very useful in many circumstances.

Thus the madder colors may, by this means, be obtained richer and brighter, and the salts of iron forming a dye bath and acting in a state of more or less concentrated solution on the tannate of gelatine, enables us to obtain immediately all the shades from a pale gray to the most intense black.

I.—PRINTING ON FABRICS.

Printing with Tannate of Gelatine.—I have applied the combination of gelatine and tannin as a substitute for albumen, to fix the mineral colors and lakes on fabrics by means of printing. I print the colors rubbed with the gelatinous solution, and, after dessication, I pass the printed fabric through a tepid bath of tannin. If the price of pure tannin were not an obstacle to the utilisation of that matter, most beautiful impressions would be obtained by my process; the ground would not take the slight reddish tone given by a decoc-

tion of nut-galls or other usual tanning matters, and the operation of bleaching the ground would be unnecessary. By combining the operations of printing according to these instructions with the operation of dyeing black, we should have varied printing on a grey ground.

Fixation of Colors with Starch and Baryta or Lime.—I have not confined myself for the fixation of mineral colors and lakes, to the intervention of tannate of gelatine, I have likewise used other reactions. Baryta and lime decompose with remarkable distinctness the liquid paste of fecula or starch for the formation of an insoluble compound; I tried to render this reaction available for the fixation of colors on fabrics. For this purpose, I printed the colors rubbed with recently prepared tepid paste of fecula, then, after dessiccation, I passed the printed fabrics through a weak milk of lime, or preferably through baryta water.

The result of the fixation of colors by this process is attained without presenting the inconvenience of the coloration of the ground, but the colors are less solidly fixed than with tannate of gelatine.

Printing with Silicate of Soda.—Amongst many various applications of the soluble silicates, I have already mentioned the employment of these salts in printing on fabrics. After the treating with colors rubbed in a concentrated siliceous solution of 35 or 40 degrees the fabric should be left exposed to the air for several days, and the decomposition of the silicate, and the fixation of the color must be performed with a weak bath of sal-ammoniac.

Finally, I have successfully tried a mixed method, consisting in printing the colors mixed with the siliceous liquid, in which fecula and soap have been dissolved with heat, and fixing the colors with lime or baryta.

PAINTING IN DISTEMPER.—AGGLUTINATING MATTERS.

By bringing into painting in distemper the processes previously described for the fixation of the mineral colors on fabrics and paper, I have transformed this kind of painting into a true chemical operation.

Painting with Tannate of Gelatine.—My colors are applied by the ordinary processes, that is by means of a gelatinous solution; they may be pounced, and after this part is finished, the paintings are fixed by means of a decoction of nut-galls, or any other tanning solution. The gelatine is thus rendered insoluble, and the colors cannot be removed by washing.

One essential condition for the success of this mode of fixation is not at first to employ concentrated tanning solutions; it is necessary to apply several layers of these solutions more and more dense. If nut-galls are used, the decoction first applied should only contain the soluble principles of 6 or 8 parts of nut-galls to 100 of water; concentrated solutions would have too powerful an action on the painting, and would cause inequalities in the tints.

After the fixation of the painting by weak solutions, we may apply

without danger, more concentrated solutions, and the work may be finished with a decoction of one part by weight of nut-galls in five of water, and a varnish comparable to essence varnish may be applied without danger to the colors which are thus fixed.

Painting with Starch.—The question of economy having been the chief object of my consideration, I wished to substitute for gelatine, size of starch or fecula;* the price of fecula being less than half that of size, and this latter absorbs, to constitute a liquid fit for painting, scarcely half the quantity of water which enters into a starch of fecula of equal consistency.† There is therefore in this case a saving of 75 per cent. in the price of the agglutinating matter.

Fixation by Lime or Baryta.—Proceeding in the manner described for the fixation of printing, I have obtained the most satisfactory results with distemper painting with starch. The size of starch or fecula used tepid combines admirably with colors of all kinds, and they are applied with the greatest ease; only the amylaceous solution is not quite so good as the gelatinous solution for very fine paintings, still it answers extremely well for the general purposes of decorating apartments. After the application of two or at most of three layers of these colors, their fixation is rendered quite certain by washing with very dilute milk of lime or baryta water.

In the same manner as for printing on paper, after dessiccation, the uncombined lime or baryta is removed with a brush, and the portion of these bases fixed by the starch is so intimately combined, that it does not soil the colors applied.

Siliceous Painting.—In mentioning in previous publications the possibility of replacing oil, essences, and size, by siliceous solutions, I should have mentioned a few difficulties met with in this new kind of painting. In the first place, there is the necessity of allowing the siliceous colors to harden gradually so as to avoid peeling off, then the motion which takes place in wood during complete dessiccation, and finally, the existence in certain woods of resin, which resists the adherence of colors.

The first of these inconveniences, when the painting has to be applied to stone, exists least when the stone is most porous. Moreover, in all direct applications of siliceous colors, to stone or plaster, we must not be too prodigal of the silicates, so as to avoid the ulterior displacement of the colors in the form of scales; the ground should always remain absorbant and not completely saturated with the siliceous paste. Solutions at 18 or 20 degrees Baumé, applied in several layers, generally give good results. These degrees must be higher in painting on glass, which is the most difficult of all, and in which it is especially important that the colors should harden very

* Albumen, caseum, and all the other organic matters coagulable by lime or baryta may likewise be substituted for gelatine, but they do not appear to be more economical than starch. Milk has been tried, but has not become habitual in this kind of painting.

† To form size fit for painting, gelatine only takes 10 times its weight of water, whereas, fecula requires 20 to 24 parts of this liquid.

slowly, avoiding the action of hot, dry air, so that the contraction of the siliceous molecules may be effected gradually under the influence of the carbonic acid of the air. With this precaution, this kind of painting succeeds very well, and would be found very useful for the decoration of church windows, and certain parts of our general edifices.

Painting in Distemper Fixed by Silicates.—Having been thus led to study the conditions of the fixation of colors in distemper, I determined to experiment on the action of the silicates. The first results of the application of siliceous solutions on colors with size or starch were as discouraging as with tannin; every touch of the brush made a patch. By persevering in these trials, I soon convinced myself that by applying these solutions at a degree of concentration which should not exceed 5 or 6 degrees Baumé, the colors retain their uniform intensity, and that two successive applications of these solutions fix these colors in a very stable manner, and permit of their being washed with water.

Mixed Process and Varnish.—One process of painting in which the intervention of the soluble silicates has appeared to me very efficacious, consists in adding to the starch paste nearly the same volume of siliceous solution at 35 or 40 degrees, and using the mixture to mix the colors for application. The silicate of soda renders the paste of starch, or of fecula, more liquid, and thus permits of a more uniform application of the colors.

The same mixture of amylaceous and siliceous liquid may be of great use to cover all pictures in distemper with a very solid and very brilliant varnish, which may be used in an infinite number of other cases.

The siliceous fixation and varnishing of colors in paintings in distemper open a vast field for the decoration of our monuments and habitations. Some very important works executed at Lille, under my own eyes, have already attracted the attention of several very distinguished artists.

WHITE BASES AND COLORS.

In my siliceous paintings it is necessary to exclude the use of all colors which are altered by the alkaline reaction of the silicates; it is also necessary to exclude those mineral colors which are too rapidly decomposed by these salts. Thus, ceruse, chromate of lead, Scheele's green, Schweinfurt's green, Prussian blue, and many other colors, especially the lakes, cannot be used in siliceous painting, but sufficient remain to give ample variety to the pictures. The white basis which covers the best in this kind of painting is white zinc.

When we make paintings in distemper fixed by means of an alkaline solution of silicate, or of paintings mixed with a combination of fecula paste and siliceous solution, or even when the painting is made by means of starch fixed by lime or baryta, it is desirable to avoid those colors which are altered by the alkalies; but this is not the case in the application of my method of fixation with tannate of gelatine,

which permits of the employment of colors of every description, with the exception only of certain metallic salts, either soluble or hydrated.

I would call the attention of architects and painters to the remarkable reaction of lime and baryta on starch paste. This reaction enables us to wash, even with hot water, some very economical paintings, in which chalk, kaolin, gypseous alabaster, the ochres, etc., are applied after being rubbed with a slightly warm paste, containing about five per cent. of its weight of fecula. The fixity of these colors is also remarkable when they are tempered by means of a mixture of starch paste and a solution of silicate of soda, without the necessity of any intervention of lime or baryta.

Plaster.—I have very successfully applied baked plaster to painting; this plaster, especially when produced from crystallised gypsum, gives very beautiful colors, whether applied by means of a solution of gelatine, which forms a true stucco, or by means of starch paste fixed with lime or baryta. In either case, the siliceous painting or varnish may be applied on this white ground without producing any scaling, which is to be feared when we cover ordinary ornaments of moulded plaster with a siliceous layer.

Artificial Sulphate of Baryta.—Of all my applications to painting in distemper, the most important appears to me the substitution of artificial sulphate of baryta for ceruse, white zinc and other white ground color. I have thought that the use of baryta white would be capable of such great extension as to permit of its manufacture on a large scale in my factories, where it is established with the manufacture of the soluble silicates, which have already taken an important place in industry. I have therefore wished to render my new process generally known.

Artificial sulphate of baryta, the result of a chemical precipitation, is obtained and sold sometimes in dry cakes, but more generally in a consistent paste, requiring no labor in rubbing before application to painting. It is applied like all white ground colors, in successive layers by means of size, starch, or, finally, by means of a mixture of starch and siliceous solution. Although almost transparent when applied with oil, this sulphate covers perfectly and as well as ceruse and oxide of zinc when mixed with size or starch, and has the advantage of being only about one third the price. In a firm paste, the price is 22 francs the 100 kilogrammes. It is not affected by emanations of sulphuretted hydrogen, and gives paintings of a whiteness and softness unequalled by the finest white lead.

This product has already been used under the name of *permanent white*; it is used to give white and satin surfaces to papers for staining and in the preparation of enamelled cards.

By opening a new and unlimited use for artificial sulphate of baryta in its application to painting in distemper and siliceous, I trust that I have made a great progress in the decoration and preservation of our monuments and houses.

Baryta white will enable us very cheaply to make white paintings, either dull or bright, according to the method used in its application and

fixation; paintings which will rival those of silver white and varnish. No painting of antiquity is comparable to the grounds executed with baryta white applied with gelatine, or preferably, with fecula, or a mixture of fecula paste and siliceous solution.

There is another important consideration, that by the substitution of artificial sulphate of baryta for ceruse and white zinc, as also by the substitution, in many circumstances, of paintings in distemper for paintings in oil, or with essences, independently of the great economy attained, I have placed the art of painting and the manufacture of white bases, in the most satisfactory sanitary conditions. Not only have I avoided the dangers which result from the manufacture and use of white lead, and even of white zinc, but I have also suppressed the equally serious inconveniences arising from the odor of essences.

Having desired to ascertain the innocuousness of the manipulation of baryta white, I made many experiments with it. Whereas a few centigrammes of ceruse, of white zinc, and even of natural carbonate of baryta, may produce greater or less effect upon the health, according to the strength of the animals, I have for ten consecutive days fed poultry on a paste of rye flour, to which had been added a fourth part of its weight of artificial sulphate of baryta, without any inconvenience arising from this diet. A little dog, weighing only $2\frac{1}{2}$ kilogrammes, received in its food for two following days, and at one meal, 22 grammes of artificial sulphate of baryta without any suffering whatever arising from it.

I must add that I have succeeded in making very hard mouldings in plaster by mixing this body with a solution of gelatine, and then impregnating the objects moulded with a decoction of nut galls, or by mixing the plaster with flour paste, and immersing the mouldings in milk of lime or baryta water.

Siliceous solutions may be used for fixation in either case with great success.

I have also hardened plaster mouldings by steeping them in baryta water, or by several superficial washings with this solution. In these cases the baryta forms, by the decomposition of the sulphate of lime, a layer of artificial sulphate, and the lime, thus set free by the disengagement of sulphuric acid, then gradually attracts carbonic acid from the air, which gives to the plaster casts, without any change of form, a very consistent covering which will bear washing.

Comptes Rendus.

On the MANUFACTURE of the CHINESE GREEN DYE, called LO-KAO.

By the Rev. M. HELOT, Missionary of the Company of Jesus.

(Continued from page 497.)

Manufacture of the Lo-kao at Azé.—In the account which we have previously given of the dyeing of cotton cloth by the bark of the *Lo-za*, it will have been observed, that the cloth after each immersion in the dye, is placed to dry and exposed to the sun, *without having been rinsed in*

pure water. Now it is from the *excess of dye* in this cloth that the substance *Lo-kao* is extracted.

The process is as follows: five or six vessels filled with clear cold water are prepared. Into the first vessel the dyed cotton cloth is immersed and shaken about (without precisely being washed); the water becomes slightly colored green. The same process is repeated in the second vessel, in the third, in the fourth, and so on; the cloth being lastly dried, is ready for sale. The water from the several vessels is collected into a large boiler; on the surface is spread a somewhat thick layer of cotton threads, and the whole is then heated. Upon boiling, the coloring matter suspended in the water, but not dissolved, is entangled by the cotton threads, and subsides, while the water itself becomes transparent: this water is then poured off and the cotton threads subjected to the same operation a second time, and so on until they are sufficiently loaded with the green color. In general, about six pounds of cotton thread are required for absorbing the excess of green coloring matter set free in the washing of 300 pieces of cloth, each 30 feet long by one foot broad.*

The cotton threads being properly charged with color, are washed, being rubbed in the hands, six pounds requiring about 100 pounds of clear cold water. An impalpable green powder is separated, and settles to the bottom of the vessel. The cotton threads are washed in this way in eight or ten waters, when they are put aside to be again charged. The green powder collected in the several vessels having been united and thoroughly washed by decantation, is rapidly dried in the following manner: a sheet of bibulous paper having been placed upon a layer of ashes, the color in the condition of a thin paste is poured on to it, and carefully and gently spread out; the ashes absorb the moisture, and the sheet is soon ready to be removed and dried in the sun. The thin layer of color cracks, and separates from the paper in the form of little flakes, which are disposed of in trade under the name of *Lo-kao*. The thinner the flake, the more it reflects,—still more so if it is dried on a polished surface; so in fact, this *reflet* must be cautiously accepted as an indication of the goodness of the dye. Forty pieces of cloth afford about one ounce of *Lo-kao*. One of the manufactories at Azé dyed during this year 8000 pieces, and sold upwards of 12 pounds of *Lo-kao*. The five factories at Azé could hardly produce altogether more than 30 pounds, or 40 pounds of the dye. The manufacturers of the dye sell it only in packets of ten ounces, the average price being weight for weight, about the same as silver, say eight or nine francs per ounce; but the demand which Europeans have of late made for it, has caused an advance, and when I was at Azé, the price had risen three dollars per ten ounces, being, in fact, 15 dollars instead of 12.

I have made some minute inquiries as to adulteration, but all the workmen whom I have asked myself, and still better those whom I have caused to be interrogated by a skilful Chinese, have alleged that

* This foot is about equal to the metrical foot or 0.33 metre.

the thing could not be done, and that any foreign substance, even in small proportion, would vitiate the color too much for it to pass undetected. The only addition that I can discover as being made, are fine iron filings, which give weight without impairing color; but they are easily perceived upon wetting a little of the dye and rubbing it on a glass with the finger-nail. The dye may lose, however, much of its beauty, if not carefully preserved from damp. For this reason it ought to be wrapped in a piece of skin and kept in a vessel containing fragments of quick lime, which of course must be renewed as they become hydrated. The readiness with which the *Lo-kao* changes by moisture, explains why it can only be prepared in winter, since in spring and summer the solutions would speedily become mouldy, and the color would be lost.

The manufacture carried on at Azé is confined to the dyeing of cotton cloth with solutions of the bark of the *Lo-za*, and to the extraction of the green dye *Lo-kao*, the latter being all sold. I have been told, however, of three houses at Soo-chow who make use of *Lo-kao*, and have obtained the following particulars regarding its employment from a workman who has been engaged in dyeing with it.

To form the dyeing solution, the requisite quantity of *Lo-kao* is placed in a little cotton bag: the proportion of one ounce of the dye for every ten pieces of cloth (each 30 feet by one foot) dyes a strong color; the same quantity used for 25 or 30 pieces of cloth imparts a lighter color, such as that of the specimen I have sent. At all times, it is desirable to prepare no larger quantity than is likely to be used at once. The *Lo-kao*, being tied up in a little bag, is softened and reduced to a pulpy condition by being allowed to soak for twelve hours in water. Some more water is then added, and the bag is squeezed and rubbed in the hands until the *Lo-kao* is well diffused through the water. Carbonate of soda is then added, in the proportion of ten ounces to one of the dye: the liquor is then heated until perfect solution takes place. The quantity of water is arbitrary, depending on the intensity of tint required. The bath being in readiness, and heated to from 50° to 60° (122° to 140° F.), the cloth to be dyed is immersed in it. Such cloth ought to be perfectly white and deprived of its stiffening by washing.

The liquor is wrung out by twisting, and the cloth, without being dried, is dipped a second time, and again wrung out: it is afterwards rinsed in clean water, and dried in the shade. Such cloth is known in Chinese commerce by the name *Ou-lo-sé*, i.e. water-lilly green color, or *Ou-lo-pou*, i.e. cloth of the color of the leaf of the *nenuphar*. A piece of cloth 30 feet by 1 foot is reckoned to cost for dyeing 15 cents, which for a square *metre* would be a little over 5 cents.

The conclusions I have arrived at may be thus summed up:—

1. The shrub affording the dye *Lo-kao* is of two species, both growing wild—the one on the mountains in the S.W. of Tche-kiang, the other in the fertile plains of the same province.

2. It is by the maceration of the bark of these shrubs, assisted by the action of the sun, that the green color is extracted. It is not

prepared from the seed, which is however a little black berry like the *casia*, which, crushed upon white paper, stains it green. No use is made of this berry at Azé.

3. Cloth colored green by the liquor obtained direct from the bark of the *Lo-za*, ought to be regarded not as dyed with the *Lo-kao*, but rather as a residue obtained in the preparation of this substance. This will explain how the color of this expensive dye is imparted to so cheap a material. Silk is said to be never dyed with *Lo-kao*.

4. The only mordants used are lime, which is added in the maceration of the bark, and carbonate of soda, which is employed to render the *Lo-kao* soluble.

A peasant of Shantung has, however, assured me that alum is used in that province.

5. The quality of the dye depends on the fineness of its paste. The best *Lo-kao* is that obtained in dyeing cloth successively with the *Hom-bi* and the *Pa-bi-Lo-za*, such as in the process carried on at Azé. I have been assured that it is also prepared from the bark of the *Pabi-Loza* only, but at Azé they consider such product inferior.

6. To preserve the dye, it must be kept secure from humidity and consequent mouldiness.

[*Note by the Translator.*—In the above communication, the French system of representing the sound of Chinese words has been followed; and as this may occasion perplexity to some readers, I avail myself of the kind assistance of a Chinese scholar to add for comparison a list of the principal words as spelt according to Dr. Morrison's manner :—

<i>Hélot.</i>		<i>Morison.</i>
Lo-kao		Lũh-kaou.
Lo-za		Luh-tsai.
Pa-bi		Pih-pi.
Hom-bi		Hung-pi.
Lieu-lo-chou		Lew-luh-shoo.
Lieu-lo-tche		Lew-luh-che.
Se-lo-pou		Se-luh-poo.
Ou-lo-pou		Ho-luh-poo.
Ou-lo-sé		Ho-luh-tsai.
Azé		?

Pharmaceutical Journal.

AGRICULTURAL CHEMISTRY.

INVESTIGATIONS *as to the QUANTITIES of NITRATES contained in the SOIL and in WATER.* By M. BOUSSINGAULT.

(Continued from page 501.)

IF meteoric waters, which agriculturists cannot command, sometimes produce an unfavorable effect on cultivation, by their abundance and by the unsuitableness of the times at which they come, it is not the same with spring water and river water used in irrigation, or with those imbibed by a valley in a fit position for humectation. These waters, when poured on the earth yield to it the whole of the useful substances which they hold in solution or suspension: calcareous and alkaline salts, carbonic acid, organic matters, &c.; and to show in what large proportion these substances whether dissolved or carried are introduced, I will just mention that, in a series of experiments which I undertook to ascertain the volume of water necessary for irrigation during the summer in our climate, I have readily caused the absorption by one hectare of earth closely sown with clover, of 97 cubic metres of water every 24 hours. This was only watering at the rate of 9.7 litres of water per square metre: spreading upon the soil a layer of water whose thickness did not reach 0.01 m.

Among the useful salts which irrigation brings to a soil we must mention the nitrates, whose fertilising powers are described by M. Henri Sainte-Claire Deville, in the work which he published on drinkable waters, and from which he drew the consequence, that the water of springs and rivers is a powerful manure, because of the silica and alkalis which it contains, and the organic matters and nitrates from which plants draw the nitrogen indispensable to their organism.

It is not necessary to insist on the interest attaching to an estimation in water, of so active a manure as saltpetre; the results which I have obtained, by showing how variable the proportions of this element are, likewise prove the value of similar investigations.

Thus I could scarcely estimate the nitrates, in the immense mass of water contained in the lakes of the Vosges mountains.

The water of the Lake Stern, in the higher valley of Massevaux, which is surrounded by a syenitic rock, only contained the equivalent of 0.01 mgr. per litre of nitrate of potassa. This water was taken on the 22nd October, 1856, (I shall always give the date, because, in water as in earth, the proportion of nitre is not always alike).

The water of Lake Seven, in the same valley, a little below Lake Stern, and from which the Doller flows, gave the equivalent of 0.07 mgr. of nitrate of potassa, on the 23rd October, 1856. The tank of Soultzbach, near Woerth (Bas Rhin), formed by the water of the little river Soultzbach, is surrounded by the Vosges. In a litre there was only 0.03 mgr. of nitrate, on the 24th August, 1856.

Spring Water.—I have examined the water of 14 springs; the poorest in nitre were those of the Liebfrauenberg and the rivers of the Fleckenstein, both of which spring from the Vosges. The litre contained the equivalent of 0.03 mgr. to 0.14 mgr. of nitrate of potassa.

The spring waters in which I found most saltpetre, are those of Eberstrom (Bas Rhin) and Roppentzwulloe (Haut Rhin), they contained per cubic metre, the equivalent of 14 and 11 grammes of nitrate of potassa. These waters are used for irrigation.

River Water.—Of the river waters analysed, the least charged with saltpetre are those of Seltz and Sæter, tributaries of the Rhine; 0·7 gr. to 0·8 gr. per cubic metre.

The rivers whose waters contained the most nitrate, are the Vesle in Champagne and the Seine. The water of the Vesle contained 12 grammes per cubic metre; the water of the Seine 9 grammes. This last number is deduced from six determinations, made between the 29th November, 1856, and the 18th January, 1857.

In 1846, M. H. Sainte-Claire Deville found in the water of this river, in nitrate of soda and magnesia, the equivalent of 18 grammes of nitrate of potassa per cubic metre.

When the water is low this river carries to the sea the equivalent of 58,000 kilogrammes of nitrate of potassa, and at an average flow 194,000 kilogrammes per 24 hours. On the 18th August, 1856, the cubic metre of this water contained one gramme of nitrate. This river on that day carried to the sea 95,000 kilogrammes of saltpetre in the 24 hours.

If we now consider, that the volume of the Seine water is considerably less than that of most of the large rivers which traverse the various continents, we shall understand what an immense mass of saltpetre is continually carried into the ocean, and the incessant activity with which the phenomena causing nitrification, must take place on the surface of the globe.

Well Water.—I have found more nitrates in wells sunk in villages and rural works, than in springs and rivers; but here also the proportions are very variable. For example, the water of the well of Bechelbronn, which truly is not quite free from traces of petroleum, only contains traces of nitrates, whereas the water of the wells of Woerth and Freischwiller (Bas Rhin), which are cut through lias clay, contain 66 and 91 grammes per cubic metre. But it is in the wells of large towns, that the largest quantity of nitrates are found. This fact has been long known, and M. Henri Sainte-Claire Deville found in the water of a well at Besançon, the equivalent of 198 grammes of nitrate of potassa per cubic metre. The proportion of nitre which I found in the water from 40 wells, chosen from the 12 arrondissements of Paris is still higher. These estimations were executed with the two processes which I have always employed comparatively, the decoloration of indigo and the ingenious method due to M. Pelouze.

The waters containing the smallest proportion of nitrates per cubic metre, were the following:—

	Gr.	
Rue Guérin-Boisseau	206	of nitrate of potassa.
Rue Saint Martin	233	„ „
Rue Saint George's	238	„ „
Rue des Petites-Ecuries	258	„ „

The waters which gave the most nitrates were drawn from the oldest quarters.

In the water of the wells :—

	Kil
Rue de Fouarre	1·031
Rue des Foin-Saint-Jacques	1·500
Rue Saint Landry	2·093
Rue Traversine	2·165

In two wells in market gardens in the Faubourgs, the cubic metre of water contained 1·258 kilos. used 1·546 kilos. of nitrates. We see that 100 cubic metres of this water, used exclusively for watering, take into the earth 120 and 125 kilogrammes of saltpetre, whose usefulness as manure is indubitable, especially when we consider that in summer one hectare of market garden earth absorbs daily, 80 to 40 cubic metres of water.

The large proportion of nitrates found in the water of the wells in the metropolis, is doubtless due to the modifications which are undergone by the organic matters, with which the soil is constantly impregnated. The purity of the air and water, whose effects are so powerfully manifested on the public health must be seriously affected by them. I have shown before, that rain after having purified, by passing through it, the air of a great city, holds in solution or suspension much more ammonia, much more putrescible substances than that which falls in the country; and I must now mention that the water of wells, after having infiltrated a soil which resembles a nitre bed, is polluted with evidently injurious substances. So true is it, that a large condensed population carries within itself the germs of insalubrity.

At Paris, in consequence of the geological structure through which it passes, the water collected in the wells is not drinkable, and it is not used for cooking; accordingly, it might be supposed that the population were safe from the injury it might do. This idea would be erroneous, for it is easy to show, that each inhabitant daily consumes the whole of the substances dissolved in a certain volume of this water. In the first place it has been ascertained that within the walls, strong wines and spirits are mixed with the water of the wells, and it is positively averred that the bakers use nothing else to make their bread.

A thousand kilogrammes of flour, require in their preparation 617 litres of water. The yield, 1373 kilogrammes of bread, necessarily contains all the soluble substances in the 617 litres of water.

In one kilogramme of bread, there is consequently all that would be found in 45 centilitres of well water.

Let us now see how much nitrates this water will introduce.

The water of the well of the Hotel Scipion, hospital bakery, contains the equivalent of 0·31 gr. of nitrate of potassa per litre; this is one of the waters containing the least of these salts.

One kilogramme of bread, prepared with this water, would there-

fore contain 0.14 gr. A kilogramme of bread made with the water of the well of the Rue St. Landry, would contain the equivalent of about one gramme of nitrate of potassa.

In these small doses, it is doubtful whether the nitrates are injurious; but the mischief is, that their presence in bread is an indication of organic matters proceeding evidently from suspicious sources, such as drain water. Moreover, it must be remembered, that each year when the Seine rises and there are subterraneous inundations, these put the lower layers of the soil in connection with the upper portions and with the receptacles of filth, and the waters, washing the earth carry with them sporules of cryptogamic vegetation, that injurious growth which is all the more to be dreaded because, notwithstanding their frail appearance, their organisation resists the heat at which bread is baked, as was proved by M. Payen, and more recently by M. Poggiale.

The directors of the hospitals have now, if I am rightly informed, determined to supply the bakery of Scipion with water from the Seine, and this example will probably be imitated throughout Paris.

From these researches, I may be permitted to conclude that, with respect to the fertilising principles which they impart to the soil, by irrigation or imbibition, the waters which circulate on the surface or at a slight depth, act much more by means of the saltpetre, than of the ammonia found in them. In my memoir on the ammonia in water, I showed that river water seldom contained more than 0.2 gr. and spring water more than 0.02 gr. of alkali, per cubic metre; now the results obtained hitherto would indicate in one cubic metre of the same water the equivalent of six or seven grammes of nitrate of potassa, answering as a nitrogenous manure, to 1.10 gr. of ammonia. These numbers are very nearly the same as those which M. Bineau deduced from his chemical studies on the waters of the Rhone.

The geological constitution of a country has, moreover, the greatest influence on the proportion of saltpetre. This influence, which M. Bineau has likewise ascertained, is especially demonstrated in the course of this work. Thus in the lakes in Syenite, the waters scarcely show the traces of nitre; those which come from the red or quartz freestone of the Vosges, only appear to contain about 0.5 gr. per cubic metre; whereas in calcareous soils, when they come from the cretaceous group, and upper tertiary deposits of chalk, spring and river waters furnish per cubic metre the equivalent of 15 grammes of nitrate of potassa, and the proportion has varied from six to 62 grammes.

If in springs and rivers more nitrates exist than ammonia, the contrary appears to be the case with rain and snow.

Some experiments, continued for six months in 1852, established that the rain water, collected at a great distance from inhabited places, contained on an average 0.74 mgr. of ammonia per litre. Since then Messrs. Lawes and Gilbert have found a nearly similar result, by observations continued during a whole year at Rothamstead.

During the summer and autumn of 1856, I examined 90 samples

of rain, collected at Liebfrauenberg. In 76 samples the nitrates could be estimated, which agreed with M. Borrale's results, and the quantitative numbers which I obtained, although not perhaps everything that could be wished for, nevertheless, authorise me in believing that rain, when it falls among fields, in the neighborhood of extensive forests, contains much less nitric acid than ammonia.

After this Memoir had been read, M. PELOUZE mentioned an experiment, showing that the nitrates are destroyed under the influence of putrescent animal matter. He has ascertained that saltpetre disappears slowly in white of egg, and that the acid of this salt is changed into ammonia. This reaction explains why we only find a trace, if even that, of nitrates in liquid manure, in stagnant water, &c. &c.

M. J. REISET's important experiments have shown, that farm-yard manure sets free a portion of the nitrogen which it contains. It is probable that the reaction mentioned here, is likewise due to a disengagement of nitrogen.

M. BOUSSINGAULT replied, "I am fully of the same opinion as M. Pelouze. The transformation of nitrogenous matters into nitrates has a limit; when these matters predominate nitrification ceases, which is the reason that we find only traces of nitrates in the manure, taken from the upper portion of manure pits, and that no trace of these salts is found at the bottom of these pits. That saltpetre should manifest itself it, is indispensable; 1st. That the animal matter, the manure, should be incorporated with an earthy or alkaline matter; 2nd. That there should be access for atmospheric air and moisture; 3rd. That the whole should be sheltered from the rain.

Comptes Rendus.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

On the PHYSIOLOGICAL MECHANISM of the FORMATION of SUGAR in the LIVER. By M. CLAUDE BERNARD.

In a former communication I have shown results which appeared to me incompatible with the various chemical theories previously promulgated to explain this singular production of saccharine matter in an animal organism. These experiments caused me to examine the mechanism of the glucogenic function of the liver from a new point of view, and they have led me to think, contrary to those previous opinions, that the sugar was not formed at once in the hepatic tissue by the direct decomposition of such or such an element of the blood, but that it is always preceded by the creation of a peculiar matter capable of forming it by a sort of secondary fermentation. I concluded that to make any fresh progress in the glucogenic question, it would be absolutely necessary to isolate this hepatic manner existing before the sugar, so as to study its characters and to determine the physiologico-chemical part which it plays.

All the views which I published in my first memoir have been fully proved by experiment, and I have now to communicate the positive existence and isolation of the glucogenic matter existing before the sugar, to which it is very easy, as will be seen, to assign its part in the physiological mechanism of the formation of sugar in the liver.

It is evident from what I have previously said, that the glucogenic matter created by the liver in the physiological state during life, is capable of changing into sugar, simply by means of a ferment, and independently of the vital influence. The experiment of the washed liver which again filled itself with saccharine matter is the proof of this.

The only difficulty remaining was, consequently, to separate the matter in question from the tissue of the liver, and to isolate it from the ferment which accompanies it. I shall not enter into details of all the means by which I have attained this result, because the knowledge of all those difficulties becomes useless and even disagreeable to the mind when the question is explained and simplified. I will only say that when I saw that cooking stopped the formation of a fresh quantity of sugar in the washed liver, I for some time falsely imagined that the glucogenic matter must be an albuminoid matter, alterable by heat, whereas in reality it was only that the ferment was destroyed by cooking; I ascertained that this was the case by causing the washed and cooked liver to ferment by means of the ferment removed from fresh hepatic tissue.

Hence it was proved to me that the hepatic glucogenic matter had the faculty of dissolving in boiling water, and that it might thus be separated from its ferment, which remained coagulated with the other albuminoid matters of the liver. The process is thus fully traced. It is true that these things might be differently interpreted by considering that the glucogenic matter extracted by boiling, or even cold water, is not the primitive matter itself, but already the result of a transformation. This interpretation, which appears to me less probable, would, however, in no respect alter the general signification of my experiments in a physiological point of view.

It is probable that a great many other means of extracting the glucogenic matter from the liver may be considered more advantageous: I shall simply give the process which I preferred.

The liver is taken still hot and bleeding from a healthy animal; any animal will do, and fed on any kind of food. But, to simplify the question on this point, I will observe that the following experiments were made on dogs, fed exclusively on meat. The tissue of the liver is cut into very thin slices, which are immediately thrown into water which is kept constantly boiling, so that the tissue of the organ is suddenly coagulated, and that the glucogenic matter in contact with its ferment may not have time to change into sugar under the influence of a too slowly rising temperature. The pieces of coagulated liver are then brayed in a mortar, then this species of *bouillie* of liver is allowed to cook for three-quarters of an hour or an hour, in sufficient water only to cover the tissue, so as to obtain in the concentrated decoction

as large a quantity as possible of the matter which is capable of changing into sugar. The tissue of the liver thus cooked is then pressed in a cloth, or under a press, and the liquid of the decoction is poured on a filter, through which it passes with an opaline tint. To this liquid is then added four or five times its volume of alcohol at 38° or 40°, and under its influence we see formed an abundant flocculent precipitate of a yellowish or milky white, which is formed of the glucogenic matter itself, still retaining sugar, bile, and other undetermined nitrogenous products. All the precipitate, again collected in a filter, is then washed several times with alcohol, so as to deprive it as far as possible of the sugar and soluble biliary matters. In this state the dried precipitate is a greyish substance, sometimes gummy, to which has been given the name of *crude glucogenic matter*. It possesses the property of redissolving in water, to which it always communicates a strongly opaline tint, and from which it is entirely precipitable by concentrated alcohol.

The aqueous solution of this crude glucogenic matter, before being treated with potassa, is colored by iodine, does not reduce the salts of copper dissolved in potassa, and does not ferment with yeast. Nevertheless, if left for a long time, this substance appears to me to become partially changed into sugar; this is doubtless when some foreign matters are still mixed with it.

To purify this glucogenic matter, and free it from nitrogenous matters, such as the very slight traces of glucose which it may still retain, it is boiled in a solution of very concentrated caustic potassa for a quarter or half-an-hour, an operation which makes no change in it, and does not alter its fundamental properties; it is then filtered with the addition of a little water, and the whole solution is again precipitated by the addition of four or five times its volume of alcohol at 38° or 40°. Then, stirring it with a glass rod, the precipitate divides, having at first had a great inclination to adhere to the vessel. After repeated washings with large quantities of alcohol, the potassa is removed as far as possible, and the glucogenic matter is found under the form of a granular substance, almost pulverulent. This matter, thus prepared, still retains a certain quantity of potassa, which cannot be removed by simple washings with alcohol; for this purpose the matter must be redissolved in water, the carbonate of potassa saturated with acetic acid, and then heated again with alcohol, which precipitates the matter, and separates it from the acetate of potassa, which remains soluble in the liquor. The glucogenic matter then loses its granular form, and assumes the aspect of a white, very fine flocculent substance, when in suspension in the alcohol; pulverulent and resembling flour when dried.

Thus prepared, the glucogenic hepatic matter possesses characters which render it in every respect analogous to hydrated starch which has begun to decompose. This is a neutral matter, without odor or taste, resembling starch on the tongue. It dissolves, or more correctly, is suspended, in water, to which it communicates a strongly opaline tint. Microscopical examination shows no peculiarity of structure.

Iodine gives a coloration which varies in intensity from deep violet to a pale maroon red; the coloration is rarely purely blue. When heated to redness with soda-lime, this hepatic matter does not disengage ammonia, which indicates that it contains no nitrogen. (The crude glucogenic matter treated in the same manner does disengage ammoniacal vapors.) It does not reduce the salts of copper dissolved in potassa, does not undergo alcoholic fermentation under the influence of yeast, is completely insoluble in strong alcohol, and precipitable from its aqueous solution by the sub-acetate of lead, animal charcoal, &c.*

But the property of the hepatic matter which interests us the most is that which relates to its transformation into sugar. It is then that the physiological analogies between this substance and hydrated starch are seen most clearly. We see that all the influences, without a single exception, which transform vegetable starch into dextrine and glucose, will likewise change the glucogenic matter of the liver into sugar, passing through an intermediate stage analogous to dextrine. Thus, prolonged boiling with the mineral acids diluted with water, the action of vegetable diastase and that of all the analogous animal ferments, such as the pancreatic juice or tissue, the saliva, the blood, &c., very quickly transform the glucogenic matter into sugar. When this gradual transformation takes place, the glucogenic solution, from being opaline, becomes by degrees transparent, and at the same time it loses the faculty of being colored with iodine. But soon afterwards, and only when the definitive change into sugar has been effected, the solution acquires the properties of reducing the salts of copper dissolved in potassa, of fermenting under the influence of yeast, and giving alcohol and carbonic acid. I must add that the action of the diastasic ferments produces this transformation into sugar in a few minutes, when care is taken to keep the liquids at about the natural heat of the body, between 35° and 45° C. (95° and 113° F.) The aqueous solution of the hepatic glucogenic matter does not change spontaneously into sugar; it changes only with great difficulty when left to itself, and partially resists the putrefaction of the cooked tissue of the liver.

Torrefaction and the limited action of the ferments and mineral acids, change the glucogenic matter into a body which has characters in every respect similar to those of dextrine.

* When the tissue of the fresh liver is brayed, and the hepatic pulp coagulated cold, with a sufficient quantity of alcohol at 38° or 40°, we precipitate the glucogenic matter and its ferment. After having, by repeated washings with alcohol, removed the sugar and dried the matter which is reduced to a sort of powder of tissue of liver, if we replace it in cold water we obtain an opaline solution, containing the hepatic glucogenic matter and its ferment. What proves this is, that if this solution is left to itself, it becomes very rapidly charged with sugar. When the transformation into sugar is complete, the ferment may be precipitated by alcohol separate from the sugar, and thus isolated. But when we add alcohol to the solution before the sugar appears, we precipitate the glucogenic matter with its ferment. When the matter thus obtained is boiled with caustic potassa, there is a disengagement of ammonia produced by the destruction of the nitrogenous matter of the ferment mixed with the glucogenic matter.

This substance is insoluble in concentrated alcohol; dissolves in water, giving a transparent solution; it is no longer sensibly colored by iodine; does not reduce the salts of copper dissolved in potassa; does not ferment with yeast, and deviates the plane of polarisation to the right.

In a very dilute aqueous solution of this matter examined by M. Biot, at the Collège de France, in a tube of 320 millimetres, a well-characterised deviation to the right was ascertained, with very marked development of tints round the minimum of intensity of the extraordinary image.

From all the experiments which I have before described, it is consequently established that the liver of dogs fed exclusively on meat possesses the peculiar property, exclusive of every other organ of the body, of creating a glucogenic matter quite analogous to vegetable starch, and capable like that of being changed finally into sugar, passing through an intermediate stage similar to dextrine.

Without any doubt, the study of the glucogenic matter of the liver must not stop here. It was necessary to ascertain exactly its elementary composition and its constitution; to know whether this matter changes entirely into sugar, and whether in this transformation other products might not be formed; and to make a profound study of the very apparent parallelism between the transformation into sugar of this glucogenic matter of the liver, and the transformation into sugar of vegetable starch. This study belongs to chemists. I am at present satisfied with having proved the existence of this peculiar substance, which always precedes the appearance of sugar in the liver, and establishing a fact, which is capable of powerfully elucidating the physiological mechanism of the formation of sugar in animals, and at the same time of furnishing conclusions of the greatest interest to animal physiology.

(To be continued.)

EXPERIMENTS on UREA and the URATES. By M. GALLOIS.

In a memoir presented to the Society of Sciences of Göttingen, in February, 1848, MM. Wöhler and Frerichs announced in very succinct terms, that urea introduced into the digestive organs is found again intact in the urine. But other and no less distinguished observers having expressed a different opinion, I wished, by means of experiments on animals, to endeavor to throw some light on the question.

In order more easily to find in the urine the urea introduced, it was necessary to have recourse to an animal whose urine normally contains very little urea. I selected the rabbit; I fed it on carrots, and I continued this regimen along throughout the duration of my experiments.

I commenced by estimating as accurately as possible, by means of M. Millou's process, the proportion of urea found normally in its urine in 24 hours; then, in the first series of experiments, I injected into its

stomach, three days running, five grammes of urea. In a second series, I did the same. I collected with care and at one time the urine of the 24 hours, and I very easily established this first proposition; urea taken into the stomach of rabbits passes intact into their urine, and in considerable proportion.

In the second place, I collected the urine by fractions in order to ascertain how long it would be before the urine would appear in the product of the renal secretion, and after how many hours the kidneys ceased to eliminate it. The elimination of the urea is very considerable after from 30 to 40 minutes, and it ceases after from 60 to 72 hours, whatever quantity of urea may have been taken.

Poisonous action of Urea.—No treatise on toxicology has hitherto made any mention of the deleterious action of urea on the living organism. It is, however, a very active poison to rabbits. Authors content themselves with saying, that when urea accumulates in the blood, it occasions, in consequence of its conversion into ammonia, a series of symptoms which constitute uremia. Now, this latter opinion appears to me very questionable, as I shall proceed to show.

I have administered urea in the dose of 20 grammes to five rabbits, the weight of which varied from 1500 to 2000 grammes (three to four pounds), and all died with the same symptoms. The symptoms of poisoning by urea in rabbits were—acceleration of respiration, weakness of the limbs, tremors with shocks, general convulsions, tetanus and death. There are most frequently no post-mortem appearances.

I may add, and I designedly insist much on this, that natural urea poisons rabbits exactly like artificial urea, and that death cannot be imputed to cyanides contained in the artificial urea, for chemical reagents did not detect the presence of these bodies in that which I used.

Moreover, I think I may conclude from my experiments that urea poisons as urea, and without being converted into carbonate of ammonia, for at the very moment when my animals fell a prey to the most acute symptoms, I never detected in the air which they expired the presence of carbonate of ammonia.

According to MM. Wöhler and Frerichs, uric acid, introduced into the system, would contribute to the generation of mulberry calculi, by being converted into oxalic acid, urea and allantoin. These learned observers have not found allantoin, but they say they have proved the production of oxalic acid and urea. I have observed that the proportion of urea has not been augmented in the urine of the rabbit in whose stomach I had injected, first 2.50 gr., and then 7.50 gr. of urate of potassa. On the contrary, it appeared less than before the administration of the urate, and the opposite result obtained by MM. Wöhler and Frerichs seems to me to be owing to their having analysed the urine passed at some particular time, instead of analysing the urine of the 24 hours. The microscopical examination of this urine has not discovered to me oxalate of lime. Nor have I observed it in the urine of the dog, into whose jugular vein I had injected three grammes of urate of ammonia at twice. Finally, I have made two experiment

on myself; the first time, I swallowed five grammes of urate of potassa, and the second time 4.10 grs. After the first dose, I found numerous crystals of oxalate of lime in my urine, and after the second, I found none.

From these experiments, I think I may conclude that there is probably a relation between the uric acid and the oxalic acid diatheses. But uric acid, in being oxidised in the organism, does not always give urea, allantoin and oxalic acid. Without taking account of the intermediate products, which appear to be me to be variable, oxalic acid appears to me to be really a product of the combustion of uric acid in the economy, and, in combining with ammonia, it may, by ulterior reactions, engender oxalate of lime.

Comptes Rendus, No. 14, April 6, 1857.

PUBLIC HEALTH.

STEVEN'S NEW PROCESS of FERMENTING and MAKING BREAD
without the USE of ALUM, BAKERS' YEAST, RICE, or POTATOES.

MR. E. STEVENS, of the Cambridge Road Bakery, near the Eastern Counties Railway Station, has succeeded in making perfectly light, very sweet, easily digestive, and highly nutritious bread, by the following very simple means only—

Best and choicest wheaten flour,

Common table or culinary salt,

Filtered spring water,

Pure country-barm, (about the 15th or 20th part usually required and used.)

He has published a Pamphlet entitled, "*A Digest on the Manufacture of Pure Bread*," in which he publicly challenges the scientific world in these words:—

"Every loaf of Bread manufactured at my establishment is stamped with my name in full; and I invite and publicly challenge the most rigorous microscopical and chemical analysis,—binding myself to forfeit £500 to any charitable institution in the metropolis [named by the analyst] upon the discovery of an atom of alum, OR ANY KIND OF ADULTERATION, in a loaf of bread stamped with my name." See Pamphlet, page 17.

This having attracted the notice of Dr. Hassall, of the "*Lancet*" Sanitary Commission, and Professor Pepper, of the Royal Polytechnic Institution, Mr. Stevens's bakery was visited by those gentlemen a few weeks ago.

The manufacture of pure bread being a most important question, a large meeting was held on the the 31st of March, to hear a lecture by Professor Pepper on the subject at the Royal Polytechnic Institution; this was attended by B. B. Cabbell, Esq., S. Gregson, Esq., M.P., Sir James Clark, Bart., Sir James Eyre, T. Walker, Esq.; Drs. Dickson, Wilson, Camplin, Taylor, Mackenzie, Croft, Jones, O'Connor, Miller, Mason, Ingoldby; Mr. John Watt, (Editor of THE CHEMIST),

Mr. J. F. Pearsall, and many other scientific gentlemen. Dr. Hassall was also present, and exhibited a number of preparations and diagrams illustrative of the adulteration of bread and other articles. All the various processes of bread-making were exhibited, from beginning to end, as well as samples of pure and adulterated bread.

When it is remembered that bread furnishes the greater part of the nourishment by which the *health and strength* of the body is maintained, that it is in truth the support—the very “*STAFF OF LIFE*,” it will be evident that the question of the purity of bread is one of vital importance to human life, and consequently, that the deliberations of the Medical Faculty could not be devoted to any subject which more closely concerns the welfare of the people, than that which relates to the condition of its daily bread.

Wheaten flour owes its superiority to nearly all other kinds of flour to the large amount of gluten which it contains, and which is the constituent that gives value to it, and upon which its nutritious properties depend; this amounts in ordinary flour to not less than 12 per cent.; now, rice and potatoes both so commonly added to bread, contain not more than 7 per cent. of that substance; that is, they are but little more than half as nutritious as good wheat flour, and consequently, any bread to which these articles are added is robbed of much of its nourishment. But the evil does not end here,—the rice and some other substances swell up and absorb, when made into bread, a much larger quantity of water than wheat flour, and thus the quality of the bread, as a life-sustaining food, is still further reduced.

Some bread was shown at the lecture which had been made with 14lbs. of wheat flour and $1\frac{1}{2}$ of rice. This quantity of wheat flour should yield 26lbs. of bread. The quantity actually furnished by the above proportions of materials was 37lbs.

Unfermented bread being so objectionable from its heavy nature, from the chemicals used in its preparation, and being very far from pleasant to the palate, it will be necessary to explain fully the particulars connected with fermented bread,—the only kind of bread in general consumption.

Although nothing has hitherto been discovered equal to yeast for the purpose of leavening bread, yet it is essential that this should be of a certain kind—should be used only in a certain way—and as small a quantity as possible should be employed as serves to render the bread light and porous. When yeast is used in excess or is improperly prepared, the quality of the bread is undoubtedly much injured, it becomes too much leavened or fermented, and *the vitality of the yeast is not all destroyed*: and hence, in weak stomachs in particular, a fermentation is created, the food turns sour, so that “the stomach” finds one of its chief “difficulties” in the use of improperly fermented bread.

It can admit of no doubt that the less yeast used, so that the loaf is light, the better and sweeter the bread eats; it is more nutritious and wholesome, and never turns sour.

Mr. Stevens considers that he has succeeded in removing the only material objections ever urged against the use of fermented bread; namely, its liability to cause fermentation in the stomach, acidity, flatulency, and indigestion, by employing only the purest and best materials and not more than one-twentieth part of the yeast ordinarily used.

The more common adulterations were shown to consist, in the addition to *wheat flour*, of which alone genuine bread ought to consist, of other flours or vegetable substances, as *beans, peas, rye, barley, oat and Indian corn, flour, potatoe flour, rice, and potatoes*.

It was chemically and microscopically shewn in the lecture, that much of the bread we eat is saturated with alum and stuff, and it behoves us to be careful how we add to the large amount of saline matter daily ingested.*** Our forefathers considered (and we think rightly) that to adulterate food, and to *convert that which should be a source of health and strength into one of injury and disease*, was no light offence.

With regard to the use of alum, it is important that the opinions of that great authority, Baron Liebig, should be generally known; he states, "The bakers in Belgium discovered about 20 years ago how to bake from damaged flour,—by adding sulphate of copper, a poison, to the dough,—a bread in appearance and external properties as beautiful as from the best flour. This mode of improving its physical properties, of course, deteriorates its chemical properties. *Alum has the same effect as sulphate of copper*. Since phosphoric acid (contained in wheat flour) forms with the alumina of the alum a compound hardly decomposable by alkalies or acids, the addition of alum may perhaps explain the *indigestibility of London bakers' bread, which strikes all foreigners*."

We consider that Messrs. CHAMBERS, the eminent Scotch publishers, have observed very truly, "That the INHABITANTS OF LONDON will not eat *wholesome bread*; if it is not whiter than the *best wheat flour* will make it, a Cockney disdains it; so alum comes into use, causing about *ten thousand indigestions a week*; and then comes the doctor, to whom the baker is the best friend. We have a private opinion of our own, that if LONDON BREAD were *nothing but bread*, the London mortality bills would decrease by a remarkable per centage."

As "*The Times*" (in a powerful leading article on this subject), writes, "What we want is, to get the very thing we ask for and pay for. Strange that, with all the wealth of the world at our call, there are very few in this metropolis who can get a glass of pure water to drink, or a BIT OF GENUINE BREAD TO EAT."

The examination excited the greatest interest, and we trust it will give a stimulus to the manufacture of pure bread in the metropolis, if bread is still to be called the "staff of life." We have much pleasure in bearing our testimony to the light, sweet quality and flavor of Mr. Stevens's genuine bread, who we find has vouchers, speaking in the highest terms of it, from nearly every medical authority in the kingdom.

A FEW REMARKS on the ECONOMIC USES of the MARINE ALGÆ.

By P. L. SIMMONDS.

THE premiums for essays offered by Sir. W. C. Trevelyan, on the application of the marine algæ and their products as food or medicine for man and domestic animals, will, I trust, be the means of eliciting much valuable economic information in a comparatively neglected field of inquiry. The subject is one that had not been lost sight of by the Council of the Society of Arts, for in the premium list for the present year, information, I perceive, is invited as to the mode in which size from sea-weed is prepared and used by the Chinese.

Sea-weed occupies, perhaps, the largest geographical range of any known vegetable production, being found in more or less abundance wherever there is sea to nourish it. And yet, economically, it has been applied to few purposes esculent, medical, or manufacturing, and in a chemical point of view, the fuci have been considered almost beneath investigation. Far, however, from being what Dr. Johnson terms them, "noxious and useless plants," they subserve many important purposes in the economy of nature, and might be much more generally utilized.

The natural history view of the algæ family has been investigated by several popular scientific inquiries, but no one has thought it worth while to go into any lengthened investigation of their uses to man and the domestic animals, or to consider how they might be more generally applied to economic purposes. A very valuable paper appeared recently, however, in the *Edinburgh Philosophical Journal*, by a well-known author and scientific investigator, Dr. John Davy, F.R.S., which contains some analyses and remarks well worthy the consideration of those intending to compete for the prizes offered. The experiments and researches are, however, confined to four species of sea-weed common to our coasts, but they may, as Dr. Davy hopes, induce others to engage in and extend the inquiry concerning a class of substances which certainly have not yet received the degree of attention they deserve, especially from the analytical chemist.

A brief digest of their present uses, and the state of our knowledge in respect to sea-weeds, from the most reliable information, scattered over a variety of sources, may not be unacceptable in guiding the inquiries of those who have not given much attention to this interesting branch of study. In its various phases, esculent, medicinal, manufacturing, and agricultural, it requires closer investigation. The abundance of vegetation with which the All-wise Creator has stored the deep has certainly not been provided in vain, and was doubtless intended to contribute to the service of man as well as of the lower animals, and for the general economy of nature. I can glance but briefly at some of the varieties of sea-weed, leaving the fuller details to be filled up by those who have more leisure and facility for prosecuting the inquiry.

Dulse.—The *rhodomenia palmata* of Greville, passes under a

variety of names, *dulse*, *dyrisk*, or *dellish*, and among the Highlanders as *dullisg* or *water-leaf*. It is nutritious but sudorific, and has a smell resembling violets. It is employed as food by the poor of many northern nations. When well washed this alga is almost tasteless; chewed—the manner in which it is commonly eaten by the peasantry of Ireland, without being dressed—it has a mucilaginous feel in the mouth, with a slight acrid aftertaste.

The Icelanders wash it thoroughly in fresh water and dry it in the air, when it becomes covered with a white powdery substance, which is sweet and palatable; it is then packed in close casks and preserved for eating. It is used in this state with fish and butter, or else by the higher classes, boiled in milk, with the addition of rye flour. In Kamschatka, a fermented liquor is produced from it.

It is consumed in considerable quantities throughout the maritime countries of the north of Europe and in the Grecian Archipelago. Cattle are very fond of this sea-weed, and sheep and deer eat it with so much avidity, that they are occasionally drowned by going too far from land in quest of it at low-water. The fronds of another sea-weed, the *fucus vesiculosus* of Linnæus, forms a considerable part of the winter food of cattle and sheep in the Western Hebrides. It is known as *sea-ware*, and *sea-wrack*, and as *black* or *swine tang*. It makes good manure, and furnishes kelp. In Gothland the bladder *fucus* is generally given to pigs; other common species constitute a part of the fodder upon which the cattle are supported in Norway.

In Iceland the *fucus vesiculosus* passes under the name of *doughlaghman*, and of four kinds of sea-weed used for food on the coast of Donegal, this species seems to be held in the greatest estimation.

To go farther from home, we find the large sea-tangle, *laminaria potatorum*, furnishing the natives of Australia with a proportion of their instruments, vessels, and food, while other species of the same family constitute an equally important resource to the poor on the west coast of South America, and to the Fuegians, and inhabitants about the Straits of Magellan.

At a recent meeting of the Royal Society of Van Dieman's Land, samples of algae, cast upon Sloper island, were presented and examined, which are known to yield a pure and delicious jelly, to which purpose it has been for years applied.

Growing most commonly, and in the greatest abundance, on every part of the earth bound by rock and washed by salt water, is a sea-weed, known by the name of "*bull kelp*;" it varies in length and substance, according to its local position; it has a thick stem, and flat oval shaped leaf, and is about the thickness of sole-leather. The aborigines of Van Dieman's Land, New South Wales, and New Zealand, and probably those of every sea-girt shore, highly prize this weed as an article of food; they estimate it, indeed, as most nutritious and palatable. Mr. Howie was first made acquainted with the nourishing properties of the "*bull kelp*" many years ago, when engaged with several aborigines on King's Island, burying the drowned people by the ill-fated ship *Catarique*, and when falling

short of provisions he feared the necessity of having to return to Melbourne for a supply. The aborigines requested him to keep the provisions left for the use of himself and the white people with him; they told him they had plenty of good food—showed him the bull kelp, and explained their mode of dressing it. Mr. Howie partook of it, and has, since then, frequently for days together, lived entirely upon it. He described it as exceedingly nutritive, and even fattening.

The mode of dressing bull kelp is very simple; it is picked from the rocks, and dried in the sun; after which it is roasted. The aborigines place it on the wood embers of a fire, and keep turning it, until all parts are equally cooked; it is then soaked in fresh water for ten or twelve hours, and is fit to eat, or, being hung up and dried, may be preserved for many months. The natives carry it about with them on their journeys, and require no other description of food; they use it as we do bread, eating it by itself, or with animal food.

It is not possible to mistake any other description of sea-weed for bull kelp. None other grows like it. Possessed of this knowledge, and the mode of converting it into wholesome and even nourishing food, in the event of being cast away upon any uninhabited island, in the absence of shell fish, the flesh of animals, or vegetable productions, man cannot starve; here is food attainable at all times and in all positions—presuming that he can obtain fire under all circumstances. The information cannot be too generally circulated.

Tangle.—The leathery flat fronds of *laminaria digitata* (Lamouroux), when young, are employed as food for both man and cattle. It is known under the several names of tangle in Scotland, red-ware in the Orkneys, sea-girdles in England, and sea-wand in the Highlands.

The tender fronds of the young stems are eaten by the Scotch, and they are boiled and given to cattle in Nordland. The dried stalks serve in the Orkneys and Bretagne for fuel. The stems are made into knife-handles.

Laver, sloke, or slokaum (*porphyra laciniata*, Agardh) is employed as food, salted, eaten with pepper, vinegar, and oil; another species, *p. vulgaris*, has also similar nutritious properties. This alga is much used for culinary purposes in England, under the name of laver, in Ireland as sloke, and in Scotland as slaak. It makes a most wholesome and very palatable dish, the thin purple and green fronds being stewed. They are gathered during the winter months only, being too tough in the summer. After being properly cleaned, the seaweed is stewed with a little butter, to prevent its getting a burnt flavor, and is brought to Belfast, where, according to Mr. Drummond it is sold by measure, usually at the rate of 5d. per quart. Before being brought to table, it is again heated with an additional quantity of butter, and it is generally eaten with vinegar and pepper. The pepper dulse (*laurencia pinnatifida* of Lamouroux) distinguished for its pungent taste, is often used as a condiment, when other sea-weeds are eaten. There was a time when the cry of "buy dulse and tangle," was as common

in the streets of Edinburgh and Glasgow, as is that of "water-cresses" now in our metropolis.

Another sea-weed, growing on rocks in the sea, in deep water, passes under the various local names of badderlocks and hen-ware in Scotland, honey-ware in Orkney, and murlins, &c., in other localities. It is the *alaria esculenta* of Greville. The stud mid-rib is eaten, when the olive green frond is stripped off. It forms a part of the simple food of the poorer classes on the Irish and Scottish coasts, in Denmark, Iceland, and the Faroe Islands. This fucus is of most rapid growth, and will spring up to six feet in length in the course of six months.

Mr. Johns (Bot. Ramb., 279) does not speak very highly of this alga as an esculent. "Being curious," he says, "to discover what kind of fern the rocks round the coast near the Giant's Causeway afforded, I stopped one of the men who was going home with his bundle, and asked him to give me a taste, prepared in the way in which it was generally eaten. He accordingly stripped off all the expanded part of a long and narrow leaf, and presented me with the stem or midrib. It was, I must confess, as good as I expected, but at best a very sorry substitute for a raw carrot, combining with the hardness of the latter the fishy and coppery flavor of an oyster. I made a very slight repast, as you may suppose, and after having given the man a few pence for his civility, continued my walk. My guide, however, seemed to think, that if I did not choose to enjoy to the full the advantage which I had purchased, there was no reason why he should not. He accordingly stayed behind for a minute or two, and when he rejoined us was loaded with a supply of the same plant, which he continued to munch with much apparent relish as we pursued our walk."

Mr. Drummond (Mag. Zool. and Bot. II., p. 148) gives a somewhat different account, both of the part which is eaten and its flavor—his observations referring to the coast of Antrim. "It is often," he says, "gathered for eating, but the part used is the leaflets, and not the midrib, as is commonly stated. These have a very pleasant taste and flavor, but soon cover the mouth with a tenacious greenish crust, which causes a sensation somewhat like that of the fat of a heart or kidney."

The long string-like sea-lace, or, as the Orkney people call it, sea-catgut, (*chorda filum*), is collected as food for cattle in Norway, and when twisted and dried, it possesses a strength and toughness that adapts it for fishing lines.

The broad green membranous fronds of *ulma latissima*, (Linnaeus), which have a bitterish salt mucilaginous taste, are employed as food, stewed, and seasoned with lemon juice. It bears the name of green laver, or sloke, and oyster green.

Another nutritious alga is the dulse of the south west of England, *iridæa edulis* of Bory, which according to Stackhouse, is employed as food by fishermen, either raw or pinched between hot irons; its taste

is then said to resemble roasted oysters. The fronds are flat, succulent, and of a dull purple.

Under the several names of Carrigeen moss, Irish moss, Pearl moss,—the fronds of *chondrus crispas*, (Lyngb), when bleached, enter now to some extent into commerce. Containing a large quantity of gelatine, it has been successfully applied, instead of isinglass, in the making of blanc-mange and jellies. The blanc-mange thus made is hardly distinguishable from that prepared with the most costly article, and is in common use in Ireland at the tables of the opulent. This alga contains 77 or 78 per cent. of mucilaginous and gelatinous ingredients, and its gelatine even exceeds isinglass in its power of coagulating milk on cooling after boiling. According to the analysis of Dr. Davy, it appears to be composed principally of three substances; one analogous to gum, soluble in cold water, and having most of the properties of mucilage as represented by gum-arabic; one analogous to gelatine as existing in isinglass soluble in boiling water; and the third neither soluble in cold nor boiling water, the nature of which remains to be ascertained. These constituents appear to exist in the following proportions :—

28.5	Gummy matter.
49.0	Gelatinous matter.
22.5	Insoluble matter.

100.0

Carrigeen moss is coming into use for the purpose of feeding stock, and has been employed for stiffening silks, &c. A fucus, probably allied to this, found at the Cape of Good Hope, is boiled into a jelly, and, being mixed with sugar and the juice of oranges or lemons, makes a very agreeable dish.

The *laminaria saccharina* is interesting from the fact of its containing sugar. It is highly esteemed in Japan, where it is extensively used as an article of diet; being first washed in cold rain-water, and then boiled in milk or broth. Dr. Stenhouse obtained from it mannite, from which he manufactured an article as purely white and almost as sweet as loaf sugar.

Another fucus, the *durvillia edulis* of Bory, is sold commonly as a table esculent in the markets of Chile.

Sea-weed supplies a favorite article of food to millions of people of all classes in the Japan and the Curile islands. The kambou of the Japanese, and the sea-cabbage of the Russians, is the *fucus saccharinus* which is found in great abundance on the islands and shores of Eastern Asia. It is spread out upon the sand to dry, then collected together in heaps resembling haycocks, and covered with matting, until the time arrives for loading the vessels which carry it from the Kurile islands to the ports of the southern islands of Japan. It is used in soup, or, wrapped round when fish, both are boiled and eaten together. Often it is boiled on the fire; salt is strewed on it, and it

is eaten without any further dressing. On some parts of the coast of Nippon it is gathered, dried, and roasted over the coals, rubbed down to a very fine powder, and eaten with boiled rice or in soup.

Experiments made by Dr. Apjohn in Conjunction with Dr. Davy, in July, 1854.

Specimens supplied by Dr. Davy, and dried at 212°.	{	<i>Chondrus crispus</i> , bleached <i>Fucus vesiculosus</i> <i>Rhodomenia palmata</i> (Dylick).	Nitrogen Per Cent.
			2.152
			2.379
			3.776

Experiments made by Dr. Apjohn, in August 1854.

	Water.	Dry Matter.	Per cent. of Nitrogen in Dry Matter.	Protein contained in Dry Matter.
<i>Chondrus crispus</i> , bleached, from Bewly and Evans	17.92	82.08	1.584	9.587
<i>Chondrus crispus</i> , unbleached, Ballycastle	21.47	78.53	2.142	13.387
<i>Gigartina mammosa</i> , Ballycastle	21.55	78.45	2.198	13.737
<i>Chondrus crispus</i> , bleached, Bewly and Evans, 2nd expt.	19.79	80.21	1.485	9.281
<i>Chondrus crispus</i> , unbleached, Ballycastle, 2nd expt.	19.96	80.04	2.510	15.687
<i>Laminaria digitata</i> , or Dulse Tangle, Ballycastle	21.38	78.62	1.588	9.925
<i>Laminaria digitata</i> , or Black Tangle, Ballycastle	31.05	68.95	1.396	8.725
<i>Rhodomenia palmata</i> , or Dylick, Ballycastle	16.56	83.44	3.465	21.658
<i>Porphyra laciniata</i> , or Levre, Ballycastle	17.41	82.59	4.650	29.062
<i>Iridaea edulis</i> , Ballycastle	19.61	80.39	3.088	19.300
<i>Alaria esculenta</i> , or Murlina, Ballycastle	17.91	82.09	2.424	15.150
Means	20.42	79.58	2.407	15.045

N.B.—The amount of water given in this table is considerably less than what belongs to the algæ when fresh from the sea, for they had all undergone a partial drying preparatory to being sent up from Ballycastle to Dublin for analysis.

That these sea-weeds are deserving of the reputation which they have amongst the poor, to whom their use as articles of food, with one or two exceptions, is chiefly restricted, and most of all in Ireland, is in a manner demonstrated by the results of the experiments of the able chemist I have just quoted, and on whose accuracy the most perfect reliance may be placed. Dr. Apjohn, in an interesting lecture which he delivered at a meeting of the Royal Agricultural Society of Ireland, and since published in that Society's Journal, has given some account of these experiments.

Per-Centage of Nitrogen in Various Edible Substances dried at 212°.

Potatoes	.	.	.	.	.	541
Flour of first quality	.	.	.	.	.	1·817
Beet roots (mean of 13 experiments)	.	.	.	.	.	1·848
Mangolds (mean of 3 experiments)	.	.	.	.	.	1·781
Swedish turnips (mean of 5 experiments)	.	.	.	.	.	1·843

Means 1·567

These results are so unexpected, that could there be any doubt (which I have not) of their accuracy, it might be questioned. The mean of them shows that the proportion of nitrogen these alga contain, exceeds not only that of the ordinary articles of vegetable food, but even that of wheaten flour of the first quality, being as 2,407 to 1,317.

Corsican Moss.—Properly this consists of a species of *plocaria*, *p. helminthochortos* of Nees. It is the *gracilaria sperococcus*, or *gigantina helminthochortos* of others. As sold in the shops this so-called moss consists of various marine productions, especially of *laurencia obtusa* with a very little *plocaria* intermixed. It is obtained on the coast of Corsica, and elsewhere in the Mediterranean, and is nutritious, strong scented, and was formerly held in repute as a vermifuge.

Plocaria tenax, the *sperococcus*, or *gracilaria tenax* of other authors, found in the Eastern seas, has a gelatinous frond, and, from its glutinous, nutritious properties, is used for soups and jellies among the Chinese, and also as size and gum. It is imported in large quantities into Canton, from the coasts of Fo-kein and Tchekiang, and is believed to be the chief ingredient of the Chin-chin glue.

The Ceylon Moss, *gigantina lichenoides*, is a small and delicate fucus, of white color and flattened filiform shape, and contains about 70 per cent. of starch and vegetable jelly. Prepared with milk and sugar, and flavored by lemon juice or sherry, it would afford the invalid a pleasant article of diet at sea, where other jellies or their materials cannot be so easily preserved.

Agar-Agar.—This is the Malay name for an alga, which has long been an article of commerce in the Eastern seas, and is extensively imported into China. When boiled with sugar it forms a sweet jelly, much resembling that made from calves' feet, and is highly esteemed, both by Europeans and natives, for the delicacy of its flavor. Its cheapness and admirable qualities render it worthy of greater attention as an article of trade.

The bamboo-lattice work for lanterns in China is covered with paper, which, when saturated with this gum, is semi-transparent. It is also used in the manufacture of silk and paper, and is preferable to flour for making paste, as insects avoid it.

Agar-agar is prepared at Malacca, in the shape of a clear jelly, which is much admired. The plant is found on the rocks at Pulo Ticoos, and on the shores of the neighboring islands. It is blanched in the sun for two days, or until it is quite white. It is obtained on

the submerged banks in the neighborhood of Macassar Celebes, by the Bajow-laut, or sea-gipsies, who send it to China. It is also collected on the reefs and rocky submerged ledges in the neighborhood of Singapore, and constitutes the bulk of the cargoes of the Chinese junks on their return-voyages. There are two or three qualities, ranging in price from 12s. 6d. to 30s. per $1\frac{1}{4}$ cwt.

The Ceylon moss, *gracellaria (gigartina) lichenoides*, a small and delicate fucus, is also of a gelatinous nature, and after being washed in fresh water, and pressed to remove the salt and some mucilage, it is employed as a preserve.

Some of these sea-weeds are probably used by the Salangana, or esculent swallows, in constructing their nests, which are esteemed so great a food delicacy by the Chinese.

A sea-weed not described, but passing locally under the name of Akar jong barallah, is eaten by the Malays, the fronds being dressed with curry.

Occasionally species of *nostoc* are eaten in the East. In a communication made a year or two ago to the Bombay branch of the Asiatic Society, specimens of *nostoc collinum* were sent by Mr. Frere from Shikarpoor, which were found pretty thickly strewn over a space of two or three square miles, after a shower of rain, in the month of May, 1855. The natives described it as a shower of gosht (meat), which they say it resembled, being, when fresh, of a soft pulpy consistence, and like flesh in color.

It was an alga, not far removed in the vegetable kingdom from the sea-weeds which are frequently boiled down into a jelly for food; and in China, as well as in some other countries, certain species of *nostoc* are commonly eaten. The Scindians were, therefore, not far wrong in calling it gosht, or meat; and from the inconceivable smallness of the germs from which it is first generated, like the green which makes its appearance over objects that have never been exposed to the monsoon, yet present this color a few days after it has commenced, the germs of this *nostoc* might have been brought through the air to the place where they at the appointed time for their germination passed into visible forms, which having no earthly origin that the ignorant Scindians could conceive, and coming into tangible masses just after a storm, not unnaturally led to the conclusion, that they must have been showered down from the heavens. Happily, many of these events which appear miraculous to the ignorant, and give rise to scepticism among many at the present day, because they are not understood, derive explanations from science which defy all attempt at disbelief. It is wonderful to the Scindian to see what he can only conceive to be a substance rained from heaven; it is no less wonderful to the naturalist, who can trace this substance to its germs, to wonder even still more how the first germs were generated which uninterruptedly and unerringly goes on producing its like. Thus does the Creator keep up his power of eliciting wonder and admiration from the most ignorant to the most learned.

Everywhere, where sea-weeds are easily obtained, they are in repute as manure. Their fertilizing powers are quite in accordance with their composition, the large proportion of azote and the considerable portion of the inorganic substances which they contain, and which are equally the elements of our cultivated crops, especially phosphate and carbonate of lime, and one or both of the fixed alkalies.

The Duke of Hamilton assigns to each of his tenants in the Isle of Arran, a portion of the sea-shore to collect sea-weed. In Ireland this is the chief fertilizer for thousands of acres of potatoe land.

The great demand for this manure, the high prices it brought, and the great breadth of ground devoted to potato-planting last season, may be inferred from the fact, that it is computed by those who have had the best opportunities of forming an accurate estimate, that the large sum of £10,000 has been paid for sea-weed at the Galway docks alone. If we take into account the quantities which have been disposed of at Oranmore, Kinvarra, Ballyvaughan, Barnas and the other creeks and landing-places within the bay, the cutting of sea-weed this season must have realised upwards of £13,000. It has been conveyed to a considerable distance, by boats along the lakes, by carts on the road, and even by railway. Perhaps in no former years has the use of it been more general, or the price paid for it so high as in the present season.

In the Channel Islands the season for cutting sea-weed is fixed by law, and it is not only used for manure, but serves also to some extent as a fuel, and makes a hot if not a cheerful fire. There is no reason why decomposed and concentrated sea-weed might not be made a valuable fertilizer. Almost all the sea-weeds yield soda and iodine on incineration. Until very recently they have been collected in large quantities and burned for the sake of the soda yielded by the ashes; after separating the alkali, iodine was obtained from the mother liquors. Although the trade in kelp has been nearly annihilated by the plan of making soda from common salt, still sea-weed ashes constitute the sole source from which iodine is manufactured.

To sum up, in the words of Dr. Davy, are not these esculent sea-weeds, on account of the iodine and bromine which they more or less contain, deserving of more general use? Nowhere, I believe, where they are in common use is bronchocele known; and, as far as I have been able to ascertain, scrofulous complaints are rare, and even pulmonary consumption; whilst on the contrary, in inland districts, even in our own country, bronchocele and scrofula are more or less prevalent, especially amongst the laboring class, who rarely have the benefit of articles of food known to contain iodine. And this remark applies to our troops, whose dietary hitherto has been regulated with such a marked neglect of medical science.

In relation to use, these vegetables have for the most part the recommendation of cheapness, so as to be within the reach of the poorest; and their not being liable to spoil by keeping when dried, is

also a circumstance in their favor; and another, I may add, is that they are grateful to the palate, and some of them—dulce, for instance—even in their undressed state.

8, Winchester Street, Fimlico.

MATERIA MEDICA, PHARMACY, AND THERAPEUTICS.

METHOD of PREPARING LACTATE of ZINC. FORMULÆ for its EMPLOYMENT. By Dr. HERPIN.

LACTATE of zinc has not hitherto been used in medicine. Dr. Herpin has recently shown the advantages which may be derived from its use in the treatment of epilepsy; we shall extract from his memoir the facts relative to the preparation and properties of this salt.

Lactate of zinc may be obtained, 1st, by the saturation of lactic acid; 2nd, by double decomposition.

First Process.—Lactic acid is put in contact with pulverised zinc. The metal dissolves and lactate of zinc is formed, with a disengagement of hydrogen. Lactic acid may likewise be saturated by oxide of zinc, or better still by the carbonate of this base, both being freshly precipitated and carefully washed. In the latter case, when once the disengagement of carbonic acid ceases, so as to saturate every trace of acid, it is boiled and filtered; on cooling, it deposits crystals of lactate of zinc.

They only require one or two crystallisations to obtain a perfectly pure product.

Second Process.—The preparation of lactate of zinc by double decomposition necessitates two distinct operations; the first consists in transforming lactate of lime into lactate of potassa; the second in putting together lactate of potassa and acetate of zinc to obtain lactate of zinc.

When a solution of lactate of lime is precipitated by carbonate of potassa, carbonate of lime is deposited, and the liquor contains lactate of potassa, which is separated by filtration. The carbonate of potassa should be in excess, and the precipitate carefully washed, so as to extract the last traces of soluble lactate. On saturating the excess of carbonate of potassa with lactic acid, we obtain by evaporation pure lactate of potassa, in deliquescent crystallised masses, soluble in alcohol. But for the preparation of lactate of zinc, it is not necessary to use so pure a product, it is only necessary to take a liquid containing at once the lactate and carbonate of lime, produced by the precipitation of the lactate of lime; by pouring it in excess, without heat, into a concentrated solution of acetate of zinc, it forms a precipitate of lactate and carbonate of zinc.

After having collected this mixture it is subjected to pressure; it is treated with distilled water, and by filtering while hot, the insoluble carbonate is easily separated from the lactate of zinc which remains in solution. The liquor is evaporated and crystallised, and the salts obtained are purified by fresh crystallisations. If we are careful in the

two successive operations to use the carbonate of potassa and the lactate of the same base in excess, we shall have no reason to fear the presence of any salt of zinc besides the lactate.

The lactate of zinc is most frequently found in the form of white plates, formed by the union of small prismatic crystals of four sides, terminated by summits truncated obliquely. It is inodorous, of a slightly saccharine taste, afterwards styptic. It is sparingly soluble in water, at the ordinary temperature; in fact it requires to dissolve it, sixty times its weight of water. It is, on the contrary, very soluble in hot water, for it dissolves in six times its weight of boiling water.

Lactate of zinc is insoluble in alcohol; this property may be made available in its separation from the acetate of the same base. On treating two alcoholic solutions of lactate of potassa, and acetate of zinc with each other, we obtain instantaneously a precipitate of lactate of zinc.

The medical properties of lactate of zinc had not been ascertained, when Dr. Herpin undertook the study of this salt. The stability of composition and the solubility of this compound, have induced him to substitute lactate of zinc for the oxide, in the treatment of epilepsy.

He prescribes it in the following forms:—

1st. In powder.

Pulverised lactate of zinc	.	.	1 to 16 grammes.
Pulverised sugar of milk	.	.	5 ,,

Forming 20 doses—three each day.

2nd. In pills,

Pulverised lactate of zinc	.	.	1 to 16 grammes.
Syrup of gum	.	.	q. s.

For 20 to 40 pills—three to six each day.

Journal de Pharmacie et de Chimie, April, 1857.

MISCELLANEA.

The ARCHER TESTIMONIAL.

SINCE the short article which appeared in our last number was written, Mr. Frederick Scott Archer, who discovered the application of collodion to the photographic process, which he published to the world in a communication to this journal, in March, 1851, has been called out of this life, to the deep regret of his bereaved family, his friends, and all who are in any way interested in the beautiful art of photography.

In *THE CHEMIST* for September, 1856, in reviewing Mr. Hardwich's valuable *Manual of Photographic Chemistry*, we expressed a feeling that some public testimonial ought to be made to Mr. Scott Archer in consideration of the immense benefit he has conferred on the world,

and in our last number, knowing that Mr. Archer was seriously ill, but hoping that his life might yet be spared, we called upon our readers to assist in organising a subscription for this purpose. It is very gratifying to us to find that many gentlemen of eminence have entertained the same sentiments, and that a committee has been formed, to which we have been invited to assist, and by which the following circular has been issued:—

“The ARCHER TESTIMONIAL.

“To Photographers and all interested in the beautiful art of Photography, this most earnest appeal is addressed, on behalf of the widow and children of the late Mr. Frederick Scott Archer, by whose premature death they are left in dependent and straitened circumstances.

“The Photographic world must acknowledge a deep sense of obligation to the lamented Mr. Archer, for his wonderful discovery of the application of Collodion to the Photographic process; and which, indeed, brought about—so to speak—a complete revolution in the art, almost superseding every other process. His generosity in unreservedly giving to the public this marvellous improvement, disdaining to secure its advantages to himself and family, enhances his well-earned fame, and entitles him to lasting gratitude. In the words of an able writer in the Quarterly Review, ‘Not only did the adoption of this vehicle at once realize the desires of the most ardent Photographer,—not only, thus applied, did it provide a film of perfect transparency, tenuity, and intense adhesiveness,—not only was it found easy of manipulation, portable, and preservable, but it supplied that element of rapidity, which, more than any thing else, has given the miraculous character to the art. Under the magician who first attempted to enlist the powers of light in his service, the sun seems at best to have been but a sluggard; under the sorcery of Niepce, he became a drudge in a twelve hours’ factory. On the prepared plate of Daguerre, and on the sensitive paper of Fox Talbot, the great luminary concentrates his gaze for a few earnest minutes; with the albumen-sheathed glass, he takes his time more leisurely still; but, at the delicate film of Collodion, which hangs before him finer than any fairy’s robe, and potent only with invisible spells, he literally does no more than wink his eye, tracing in that moment with a detail and precision beyond all human power, the glory of the heavens—the wonders of the deep—the fall, not of the avalanche, but of the apple—the most fleeting smile of the babe, and the most vehement action of the man. Further than this, the powers of Photography can never go; they are already more nimble than we need. Light is made to portray with a celerity only second to that with which it travels; it has been difficult to contrive the machinery of the camera to keep pace with it, and Collodion has to be weakened in order to clog its wheels.’

“Mr. Archer’s early professional career—that of a sculptor—was one to which he was most ardently attached, and in which he acquired some distinction. His ever active mind early led him to perceive the

advantages likely to accrue to his art by enlisting the then new art of Photography, and rendering it available for the purpose of arresting the momentary or more permanent expressions of the human face, thereby securing a reference in the studio. The value of such an application of Photography to art, induced him to apply himself to it, and master all its chemical and optical difficulties. He could not long remain content with the slow processes then in use; and one of the earliest discoveries made by him was the superiority, as a developing agent, of pyrogallic acid over gallic acid; this he was the first to draw attention to.* Dissatisfied with paper as a medium, he set himself the task of finding something better, and was eventually rewarded, as before stated, by the discovery of Collodion. A few months enabled him to perfect his process, which he published and freely gave to all in 1851.† Other valuable improvements subsequently emanated from his active, intelligent mind; and up to the last day of his invaluable life, he was engaged in elaborating and perfecting new processes, hoping he might be spared long enough to enable him to complete what he had so well begun. It was ordained otherwise, and he was taken away at a time when success appeared to be more certain than at any former period of his eventful life, and when in all probability he would have been enabled to make ample provision for those nearest and dearest to him.

"Mr. Archer's medical adviser—a friend who knew him well—is of opinion, that he in a great measure sacrificed his life to his love of the art, and his intense desire to improve it. His labor and discoveries gave the means of occupation and of livelihood to thousands of his fellow-creatures; it is, therefore, more confidently hoped that all those who are deriving advantage from his genius, will cheerfully respond to this appeal."

The above circular states the case so ably, that we have thought that we could not do better than insert it here.

We have now earnestly to call upon our readers to subscribe liberally to this fund. The readers of *THE CHEMIST* constitute a very large body of the most eminent men of science in the kingdom, as well as of the largest manufacturers—men who can well afford to contribute handsomely towards a provision for the unprovided wife and children of a departed brother; and we call upon them to come forward, and show by their subscriptions that the bereaved family of a man who had the nobleness of mind to bestow on the world the great discovery by which, had he patented it, he must speedily have realised a handsome fortune, may safely be left to the generous care of those who follow science, whether as a profession or as applied to the arts.

* [Mr. Archer first published this discovery in *THE CHEMIST* for April, 1850, in the No. for July, 1850, he contributed another article on the same subject.—*Eds. CHEMIST*].

† [See the *CHEMIST* for March, 1851, in which this great discovery was first published.—*Eds.*]

Let every one give according to his ability, and at once. Let chemists show that the followers of science are a generous and kindly confraternity; let us take by the hand the widow and children of our deceased fellow-laborer. Let not poverty or the struggles of dependence be the lot of those whose husband and father was so great a benefactor to the public. Scott Archer needs no monument; his name will go down to posterity in honour without any such hacknied and commonplace memorial.

We call upon our readers to give us reason to be as proud of their character for generosity and justice as we are gratified by their great and increasing support.

The circular we have reprinted states to whom subscriptions may be paid. We also shall be happy to receive and forward to the committee such sums as our readers may think fit to transmit to us, (post-office orders or cheques to be made payable to Mr. John Watt,) and we will acknowledge receipt in our next number. We also intend to publish an entire list of subscribers to the testimonial as soon as it is complete; and we hope and trust that every one who reads this will immediately forward his contribution.

There is one class on whom it is incumbent to come forward liberally; we allude to the great number of persons who procure a living by taking portraits by Mr. Archer's process. From these alone, a very large sum ought to be obtained.

The dealers in photographic chemicals and apparatus, who sell large quantities of collodion for photographic purposes will all, undoubtedly, feel it their duty to subscribe.

We have no doubt that subscriptions will also be forwarded from other countries. We trust that the first of our numerous American readers who reads this notice will at once commence the movement amongst his countrymen, and we confidently hope, that the generous and kindly American nation will be behind no other in this matter.

The English Government, we should hope, will conceive this a case entitled to its consideration, and we earnestly urge her Majesty's Ministers, as soon as possible, to recommend the Sovereign to bestow a pension on Mrs. Archer. We believe that the Government has turned Mr. Archer's process to useful account. We know that the Emperor of Russia had views taken at Sebastopol, and transmitted to him, from time to time, of the position of our army in the Crimea, and of the state of the beleaguered city itself. Truly, there appears to be scarcely any limit to the utility of this wonderful invention.

As a tribute of respect to departed worth, and of deep sympathy with the cause of the fatherless and widow, the following gentlemen have undertaken the duties of a committee, to receive subscriptions, and carry, out in its fullest integrity, the object of this testimonial.

Committee.—HERBERT INGRAM, Esq., M.P.; Dr. JOHN DIAMOND; Jabez HOGG, Esq.; P. LE NEVE FOSTER, Esq.; GEORGE DE MORGAN, Esq.; Dr. HYDE SALTER; HENRY POLLOCK, Esq.; ROBERT HUNT, Esq., F.R.S.; J. E. MAYALL, Esq.; T. FRED. HARDWICH, Esq.; NATHANIEL MACHIN, Esq.; A. SWEETING, Esq.

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The following Bankers have very kindly consented to receive Subscriptions.—THE LONDON AND WESTMINSTER BANK and THE UNION BANK OF LONDON (ARGYLL PLACE).

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THE CHEMIST.

METALLURGY.

TINFOIL PACKAGES.

To the Editors of THE CHEMIST.

DEAR SIRS,—

THE public is not perhaps aware, how much its health and safety frequently depend on the vigilance and observation of the chemist. A circumstance has lately presented itself to my notice, which calls forth the severest condemnation of the use of *impure* tinfoil for wrapping up grocers' articles, which will shortly furnish matter for trial in a court of law. The article in question, is one of daily consumption; what gave rise to suspicion, was the curious appearance of the package on being opened, which led to its being brought to me.

On examination, not only was the foil eaten through, but the paper and its contents were contaminated with lead. On analysing the foil, so far from its being pure tin, which would have been uninjurious, it consisted for the most part of lead with a *mere fraction* of tin, just sufficient to prevent its oxydation and discoloration from exposure to ordinary atmospheric influence; but certainly not proof against the action of organic or other matter wrapped up in it, as was evident from the numerous holes in the foil and the poisoning of the contents. This is no light matter you will say, and it is to be hoped that the jury will mark their sense of the abomination in their award of damages, as it is not so much the loss of trade on the part of individuals bringing the action as the health of the community, which would be greatly compromised by it.

I am, Dear Sirs,

Yours faithfully,

JOHN HORSLEY, F.C.S.

The Laboratory, Cheltenham.

On the METALS of PLATINUM and on their TREATMENT by the DRY WAY.

By MM. H. SAINTE CLAIRE DEVILLE, and H. DEBRAY.

WE have undertaken to apply to the treatment of the platinum ore, in order to extract from it the useful metal, and to make its docimastic test, new methods, founded on the exclusive employment of reagents, by the dry way, and of the high temperatures which are necessary, for bringing such refractory matters to the state of fusion. Before giving

the processes in detail, it is useful to make known the properties which these metals present to us, in the physical and chemical circumstances in which we placed them. All the simple bodies of the platinum ore have a common physiognomy, and among these, we meet however with some which possess intimate analogies with the most dissimilar bodies of chemistry, from osmium, which is a metalloïd, to rhodium which is a more noble metal than gold. Their common character is in the marked tendency to return to the metallic state, under the most feeble influences, in the easy dissociation of the elements of their combinations.

Platinum.—This, after palladium, is the most fusible metal of the group. When once it is fused, it volatilises perceptibly and presents at the moment of solidification the phenomenon of *rochage*, which had hitherto been observed only in silver. There is therefore probably formation of an oxide of platinum at a very high temperature, a combination which is destroyed when the metal cools. This theory of *rochage* is justified by an experiment which we have made by carrying silver to a temperature much higher than that which is necessary for its vaporisation. The silver is oxidised, fumes like a bath of lead, and suddenly condensing these vapors, it is perceived that they are constituted of yellow oxide of silver, which forms a stucco rather lighter than the stucco of lead. In order that this experiment may be conclusive, the silver must be perfectly pure. Proust had already remarked that silver oxidises before the blow-pipe.

To make platinum rock a mass of 500 to 600 grammes at least of metal must be kept in fusion for a long time in lime, and the metallic bath must be suddenly uncovered. When it is allowed to cool slowly, the platinum does not rock.

The fusion of the platinum in lime refines it to extreme perfection, and gives a metal as soft as pure copper, which has been proved at the French Mint. The metal is whiter than ordinary platinum, and it is then fit for the manufacture of plate, because it has lost all porosity. However, it still has the property of condensing gases on the surface, and of producing the phenomenon of the lamp without flame: its density is 21.15.

Palladium.—Palladium may also be made to *rock*, and that with still more facility than platinum. Only that the oxygen being disengaged only at the moment when the upper layer of the metal is solidified, the ingot which has *rocked* is cavernous, although its surface is perfectly regular. Palladium, which is very near to silver, is perhaps more oxidisable than silver; for its surface is always tarnished by a slight layer of oxide. It is volatilised at a very high temperature, producing greenish fumes, which are condensed into a dust of the color of *bister*, a mixture of the metal and its oxide. Like silver, it dissolves in hydriodic acid with disengagement of hydrogen: like platinum, and the metals in platinum ore in general (except, perhaps, ruthenium, which we have not studied in this respect) palladium is capable of producing the phenomenon of the lamp without flame by the employment of certain precautions.

Osmium.—This substance is infusible under the ordinary pressure, like arsenic, which it very much resembles. But, at a very high temperature, it volatilises rapidly without oxidising, and without leaving any residue when it is pure.

The temperature at which osmium disappears is not lower than that at which platinum itself emits vapors. It is known that osmic acid enters into ebullition towards 100° C. (212° F.) This curious property has enabled us to determine the density of its vapor.

The experiment which we tried twice at temperatures of 246° and 286° C. (475° to 546° F.), much higher as is evident than that of the boiling point of the matter, and very different from each other, gave us two nearly identical numbers, 8.89 and 8.87. From this it is deduced that the equivalent of osmic acid corresponds to two volumes of vapor. These numbers indicate, besides, that the equivalent of osmium will probably have to undergo a slight correction, which will render it equal to the equivalent of platinum. In this operation, the quantity of osmium reduced is inappreciable, and we may rely on the accuracy of the cypher, which is obtained by employing M. Dumas's process. A remarkable peculiarity is observed at the moment when the flask is opened over mercury. In contact with the osmic acid, the mercury acquires the property of moistening the glass, and the flask is found to be coated to singular perfection by osmium reduced by mercury, or by an amalgam.

The osmic acid which was used in our experiments was very pure; it was prepared by Berzelius' process, that is to say by roasting osmium in a current of hydrogen, an operation which readily furnishes considerable quantities of acid of irreproachable quality.

Rhodium.—Rhodium fuses less easily than platinum, so much so that the same fire which enables us to bring to the liquid state 300 grammes of platinum, fuses in the same time only from 40 to 50 grammes of rhodium. We observed no indication of volatility in this metal, but it oxidises very superficially, like palladium, and rocks in the same manner as it. The surface of the ingot is often blueish. When the rhodium has been properly refined and freed from silicium and osmium, by roasting in contact with lime on which it is fused, it has very remarkable physical qualities. Less white and less brilliant than silver, it is as ductile, and as malleable, according to M. Chapins' observations.

M. Chapins, a manufacturer of platinum, well known to the Academy, showed us an alloy of platinum and rhodium containing 30 per cent. of the latter metal, manufactured in the establishment of MM. Desmontis and Chapins, and which we fused much more easily than rhodium. This alloy, after fusion and refining, may be wrought perfectly, and furnishes chemical vessels which possess the precious property of being unattackable by aqua regia, and may consequently be of great service in chemical analysis.

Iridium.—Iridium is the most refractory of the metals of platinum ore: ten grains of iridium are with difficulty fused during the time necessary for rendering 100 to 150 grammes of platinum perfectly

liquid. After the fusion and refining, iridium is still brittle, although it may be flattened a little under the hammer. It gives no sign of volatility. Finally, it has the property of condensing gases on its surface, and of being capable of serving for the experiment of the lamp without flame.

Ruthenium.—We will say nothing about our experiments on this metal, which we have not yet been able to obtain of a sufficient degree of purity. It seems, however, that under the influence of very high temperatures and in contact with lime, ruthenium disappears, producing a crystalline scoria, and leaving a very heavy butter whose density is at least 17, very refractory, and which appears to contain much iridium.

We cannot conclude without thanking MM. Desmontis and Chappin, for the liberality with which they have placed at our disposal a large quantity of very pure platinum, and of metals still so rare, which they have prepared with such great care.

In an early communication, we shall explain in detail the method of the dry way by means of which we have treated platinum, osmium, and iridium, for extracting the metals either in the pure state or in the state of alloy. We can, indeed, prepare an alloy containing, besides platinum, the rhodium and iridium of the ore, and presenting after fusion, perfect malleability and ductility as well as extreme rigidity. This last property, which is very precious in certain cases, characterises the platinum of Janet, prepared by means of arsenic by a process now abandoned.

Comptes Rendus, No. 21, May 25, 1857.

ORGANIC CHEMISTRY.

On ALCOHOLIC FERMENTATION. By M. BERTHELOT.

ALL bodies capable of undergoing alcoholic fermentation have been collected into one group, and designated under the generic name of *sugars*. Cane sugar is the type of this group, of which it constitutes the longest known term; next to it are classed grape sugar or glucose, cane sugar interverted by the acids, sugar of milk which becomes fermentable only after having undergone this same action of the acids, and, finally, more recently, melitose.

All these bodies, submitted to the action of beer-yeast, are capable of producing alcohol and carbonic acid; in other conditions, they ferment with generation of lactic acid and butyric acid. All are neutral and represented in their composition by carbon and water; all, finally, appear to possess certain general properties, such as that of combining with energetic bases, of being destroyed with great facility under the influence of heat and under the influence of reagents.

In the course of my researches on the synthesis of the neutral fatty bodies, I have been led to compare with sugar, properly so called, a great number of other substances which differed from them hitherto

in the absence of fermentation in contact with beer-yeast. Glycerine, mannite, dulcine, &c., and the sugars themselves possess, indeed, common properties of extreme importance; these bodies unite with the acids and form neutral combinations analogous to the fatty bodies in all their characters: these are the polyatomic alcohols. Neutral like the true sugars, endowed with similar taste and solubility, glycerine, mannite, &c., unite like the sugars with the powerful bases, and are transformed in an analogous manner by chemical agents: they have besides very nearly the same centesimal composition as the sugars properly so called, and are represented by formulæ of the same order, in which the carbon is a multiple of 6. Only, whilst the sugars contain hydrogen and oxygen in the proportions which form water, glycerine, mannite, &c., contain an excess of hydrogen, a difference which corresponds to a greater stability.

These analogies have led me to investigate whether it would not be possible to cause glycerine, mannite, &c., to undergo the same phenomena of fermentation as are manifested by the sugars properly so called, and principally to induce in all these bodies alcoholic fermentation.

I have, indeed, succeeded in causing glycerine, mannite, dulcine, and sorbine, to ferment in a direct manner, with production of alcohol and carbonic acid. Only this fermentation is generally accompanied by a disengagement of hydrogen, which is a consequence of the composition of the fermentable bodies. I have likewise induced lactic fermentation* and butyric fermentation of several of these same substances.

If the result of these experiments is analogous to the alcoholic fermentation of sugars, their conditions are very different, both as regards the very long time which they require and the agents employed producing them (carbonate of lime and caseine). Besides, in these conditions, the formation of the alcohol is never preceded by the previous transformation of the mannite, glycerine, &c., into sugar properly so called. In pursuing these observations, I have been led to investigate whether these same conditions, which are very distinct from the employment of yeast, might likewise induce the alcoholic fermentation of sugar properly so called, that of sugar of milk, and, finally, that of various substances convertible into sugar under the influence of the acids, such as gum and starch. In these circumstances, the alcoholic fermentation of the last three bodies is not preceded by their metamorphosis into sugar properly so called. This fermentation appears, then, to be direct, as well as that of mannite and glycerine.

In detailing the results of these observations, I shall discuss their multiple conditions, and I shall endeavor, as much as possible, to analyse the part performed by the various substances, whose presence is indispensable for the accomplishment of the phenomena. These

* For a paper on lactic fermentation by MM. Boutron, Charland, and Fremy, see the Chemist, July 1841, p 198.

experiments require the assistance of a temperature below 50° (122° F.); they also require several weeks or even several months for their accomplishment; they furnish not only alcohol but also several other substances formed simultaneously. Water, the common medium of all fermentation is also necessary, and carbonate of lime and a nitrogenous matter of animal or analogous nature. Without carbonate of lime, mannite, glycerine, &c., cannot, in ordinary circumstances, give rise to the alcoholic formation. When we operate with sugars properly so called, the presence of carbonate of lime is no longer indispensable; however, it still exerts a marked influence on the phenomena, and increases the proportion of alcohol formed. In these experiments, the carbonate of lime appears to act by maintaining the liquor in the neutral state by the saturation of the acids produced, and by guiding in a determined direction the decomposition of the nitrogenous body which induces the fermentation; I have been able, moreover, in operating with glucose, to replace the carbonate of lime with a great many other bodies capable of fulfilling the same neutralising function, such as the earthy carbonates, various metallic carbonates and oxides, such even as the metals themselves, as iron and zinc. Most of these experiments were performed at the same time comparatively with beer-yeast.

The study of the body necessary for inducing these metamorphoses, that of the ferment, has particularly fixed my attention. This ferment was in general formed of caseine; but any other nitrogenous matter of animal nature is capable of exerting the same influence. The very various experiments which I have made on this point, conform moreover, and extend the researches of M. Colin* concerning the part performed by these bodies in the alcoholic fermentation of sugar. No nitrogenous substance, out of the foregoing category, has produced the same phenomena.

The influence of the nitrogenous matters is owing to their composition and not to their form, for the same changes are operated with the most various substances, and especially with gelatine, an artificial compound deprived of all structure properly so called. The development of peculiar organised beings is likewise by no means necessary; it may be avoided by operating out of contact with the air; the fermentation is not prevented by doing so, nor is it even made slower. Hence, in these experiments, the cause of the fermentation appears to reside in the chemical nature of the bodies capable of playing the part of ferments, and in the successive changes which their composition undergoes. These changes are still little known; but they are attested by a characteristic phenomenon, which the action of beer-yeast on sugar does not present. While the mannite is being destroyed, the nitrogenous matter is decomposed without rotting, and loses, under the gaseous form, almost all the nitrogen which enters into its constitution. Thus the saccharine body and the nitrogenous

* Thénard, *Traité de Chimie*, tome V., p. 63, (1836). Colin, *Annales de Chimie et de Physique*; 2de Série, t. XXXVIII., p. 128, and t. XXX., p. 42.

body are decomposed at the same time, exerting on each other a reciprocal influence.

What is the intimate nature of this double phenomena, and its relation to the actions of contact* which that of beer-yeast on sugar so much resembles? Of this we are still completely ignorant. Only we are led to think that the action of the nitrogenous matters and that of beer-yeast itself depend, not on their organic structure, but on their chemical nature, the same as the action of emulsine or amygdaline of diastase on starch, and of the pancreatic juice on the neutral fatty bodies; the same as the action of glycerine on oxalic acid, of sulphuric acid and electro-negative bodies on cane sugar (inversion), on alcohol (etherification), and on essence of turpentine (isomeric modification). The action of diastase, of emulsine, and of the pancreatic juice, may be elucidated up to a certain point, because these substances act in the state of solution: yeast is not subject to this kind of check. But the analogous efficacy, although in a less degree, which nitrogenous matters of animal origin possess, even in the absence of all special organic structure and of all formation of living beings, tends to assimilate alcoholic fermentation to the various fermentations induced by emulsine, by diastase, and by the pancreatic juice.

From the facts described in my memoir, glycerine, mannite, dulcine, sorbine, sugar of milk, cane sugar, and glucose, belong to the same general category of organic compounds, characterised not only by analogous composition and by analogous physical qualities and chemical functions, but also by the singular property of being decomposed spontaneously under the influence of nitrogenous ferments, giving rise to alcohol, and to lactic, acetic, and butyric acids. This aptitude for fermenting, so conspicuous in glucose, less evident in cane sugar, still less so in sugar of milk and in sorbine, becomes more and more difficult to bring into play in matters which contain an excess of hydrogen, such as mannite, dulcine, and especially glycerine. These matters, more stable as regards heat and reagents, also resist more the influence of nitrogenous ferments; but the similar metamorphoses which they, however, may undergo under this influence, tend to approximate them to sugar properly so called.

When we consider that these analogous bodies are found in abundance, free or combined, in the tissues of vegetables, that they are directly connected with the insoluble principles which form their woof, finally, that most of the phenomena of botanical physiology seem to be based on their transformations, it will be easy to comprehend the interest attaching to the study of their reactions. The metamorphoses which they undergo by the way of fermentation present a very peculiar interest, on account of the resemblance which exists between these phenomena so very different from ordinary affinities, and the vital phenomena properly so called. To study fermentations, to guide

* Thénard, *loco citato*.—Mitscherlich, *Annales de Chimie et de Physiques*; 2nd Series, tome VII., p. 30, &c.

them at will towards the accomplishment of definite chemical transformations, is to set to work mechanisms analogous to those which preside over the metamorphoses of the matter in living beings.

Comptes Rendus, No. 14, April 6, 1857.

CHEMICAL RESEARCHES *on the* CYCLAMEN.

By M. S. De LUCA.

THE root of the cyclamen* is a tubercle which occurs under the form of a flattened orbicular body, of a brown color outside and white within, furnished with blackish radicles. This plant is cultivated in France for its beautiful purple flowers; but its root contains a fermentable saccharine matter, starch, gum, and acrid, irritant, and poisonous substances. The juice extracted from this tubercle is acid, and possesses an extremely acrid and styptic taste. All these properties have led to the chemical researches which form the subject of the present communication to the Academy. This first part comprises the extraction of the poisonous matter contained in the tubercle of the cyclamen—*cyclamine*, and the study of its principal properties.

Four kilogrammes of the tubercles of *Cyclamen Europæum* were operated on: after having washed them externally and having afterwards cut them into small pieces, they were introduced into a large flask with four quarts of rectified alcohol. The whole was left for 45 days in a place sheltered from the light, at the end of which time the alcohol was decanted. The same tubercles were afterwards crushed in a mortar and introduced into the same flask with three quarts of alcohol, and at the end of a month, the alcohol was pressed out. The grounds still retained a slight acrid taste, and after being reduced to a paste, were mixed with two quarts of alcohol in the same flask. The alcohol was pressed out after a contact of twenty days. The alcohol of these three operations was united, and after being filtered the greater part of it was condensed by distillation in a water bath. The residue thus obtained, which was of a gelatinous appearance, was evaporated to dryness, under shelter from the light, in a porcelain capsule in a water bath, and afterwards exhausted cold with rectified alcohol.

The alcoholic solutions of these operations, united and filtered, were placed in a capsule and left to spontaneous evaporation for forty days at the bottom of the cellar of the laboratory of the Collège de France. At the end of this time, there was deposited at the bottom of the capsule a whitish amorphous matter, under the form of small agglomerations. This matter was carefully collected, washed several times

* This tubercle contains on the average 80 per cent. of water, and leaves by incineration about half a per cent. of ash. This ash, according to the analysis made by M. Ubaldini, contains potassa, soda, lime, magnesia, silica, chlorine, sulphuric acid, phosphoric acid, and oxide of iron, and not a trace of manganese or alumina.

with alcohol, and afterwards dissolved in boiling alcohol. This alcoholic solution always deposited on cooling the matter dissolved under the same form of small amorphous agglomerations: this was the active matter contained in the tubercle of the cyclamen, *cyclamine*, which was dried under shelter from the light, in *vacuo* and in presence of concentrated sulphuric acid. The following are the properties of this new matter.

Cyclamine is an amorphous and white substance, without odor, opaque, friable and light, neutral to reagents; exposed to contact with damp air, it increases in bulk and absorbs a large quantity of water. Put in contact with cold water, it acquires a certain transparency, and assumes the appearance of a very adhesive and viscid jelly; by the spontaneous evaporation of its alcoholic solution made without heat, or by the cooling of its hot alcoholic solution, it is deposited under the form of small amorphous and white agglomerations, which turn brown by the direct action of the light: without the aid of heat, they do not readily dissolve in water, and this solution produces an abundant froth by agitation, like soap and water, and has besides the singular property of coagulating, like albumen, at the temperature of 60° to 70° C. (140° to 158° F.) On cooling, and after resting for two or three months, the coagulated part redissolves in the mother liquor, and may be coagulated again by heat; it does not contain nitrogen, and dissolves in considerable proportion in alcohol, with the aid of a gentle heat; it contains neither phosphorus nor sulphur, and, heated on a sheet of platinum, it yields a bulky charcoal which burns completely without residue: its alcoholic solution is not colored by iodine, even after it has been coagulated by heat, does not reduce potassio-tartrate of copper, and does not ferment with beer yeast.

Its aqueous solution readily absorbs the vapor of bromine and coagulates without being colored, when the bromine is not in excess; chlorine acts in the same manner. By the action of synoptase, with the aid of a gentle heat—30° to 35° C. (86° to 95° F.) in a water bath, it is divided, producing glucose, which reduces potassio-tartrate of copper, and which ferments with production of carbonic acid and alcohol; acetic acid dissolves it cold and does not coagulate it with heat; hydrochloric acid also dissolves it cold, and coagulates it towards 80° C. (176° F.) with production of glucose; concentrated sulphuric acid produces with cyclamine a yellow coloration, which afterwards becomes of a permanent violet red; this coloration disappears on the addition of an excess of water, and at the same time, a white precipitate is produced; bichloride of mercury has no action without the aid of heat, on the aqueous solution of cyclamine, whilst gallic acid coagulates it; nitric acid attacks it even cold, and gives rise to acid compounds which combine with the alkalies, whilst the same acid acts differently on cyclamine according to its degree of concentration, but its action becomes energetic with heat. By the action of fused potash, cyclamine disengages hydrogen and gives rise to a peculiar acid very sparingly soluble in water; the taste of cyclamine is manifested in a few seconds with a very peculiar acridness, which especially affects the throat;

cyclamine dissolves with heat without decomposition in glycerine, in absolute alcohol, in wood spirit, and in the alkalis; the alcohols also dissolve it without heat, but in small quantity; ether, sulphuret of carbon, chloroform, essence of turpentine, and the essential oils, do not dissolve it. The elementary analysis of cyclamine gives the following numbers :—

	I	II
Carbon	54.55	54.54
Hydrogen.	9.11	9.12

The action of the juice of the tubercles of cyclamen and that of cyclamine on the animal economy are worthy of notice; indeed, the juice of these tubercles introduced into the stomach of a rabbit in the dose of 10 or even 20 grammes did not kill the animal, and it may here be remarked that pigs eat these tubercles without inconvenience. On the other hand, this same juice acts poisonously on small fishes kept in a large quantity of water (1 cubic centimetre of juice in two or three quarts of water.)

M. Bernard has been kind enough to make some experiments with the juice extracted from the two tubercles which I placed at his disposal, by injecting it into the lungs and into the cellular tissue, in order to see if the active matter presented any analogy to curari. He employed the juice which he had expressed in three days, and he made the following experiments: 1st he injected two grammes of it into the craw of a young greenfinch, which died very rapidly: 2. He injected four grammes of it into the trachea of a rabbit, which died in ten minutes with convulsions: 3. He introduced one gramme of the liquid under the skin of a green-finch, which died in twenty minutes with convulsions: 4. A frog which received under the skin two grammes of the solution, died in half-an-hour: the heart ceased beating, the nerves and the muscles were very little excitable, the intestines were considerably meteorised and distended by gases.

All these experiments show that the active matter contained in the tubercle of the cyclamen, acts on the animal economy like curari, but less energetically. The aqueous solution of cyclamine acts in an analogous manner.

The poisonous action of cyclamine and of the juice of the cyclamen is almost neutralised by bromine. The following are some experiments which were made in M. Bernard's laboratory, in order to ascertain if bromine acted on cyclamine in such a manner as to annihilate its effects on the animal economy, as it does with the poison of the curari: 1. One cubic centimetre of an aqueous solution of cyclamine was introduced under the skin of a frog, and it died in five minutes: 2. A second frog, with the same treatment, but operating with the solution of cyclamine saturated with vapors of bromine, died in three hours and a half: 3. Two cubic centimetres of cyclamen juice were introduced under the skin of another frog, and it died with convulsions in twenty minutes: 4. The same quantity of juice saturated with vapors of bromine caused the death of a frog in four hours.

Cyclamine, in its property of coagulating by heat, resembles albumen; in the manner of depositing from its alcoholic solutions, it resembles mannite; in the character of frothing by agitation possessed by its aqueous solution, it appears analogous to saponine; by its solubility in water after coagulation, it might be confounded with some organic salts of lime; in its action on the animal economy, it behaves like curari, and by its divisions it presents the characters of salicine and populine.

The aqueous solution of cyclamine observed with M. Biot's apparatus, presents a feeble rotatory power to the left.

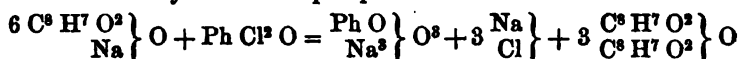
I may mention, in conclusion, that I have been greatly aided, in all these experiments, by my friend M. Ubaldini.

Comptes Rendus, No. 14, April 6, 1857.

On a NEW DERIVATIVE of ANISIC ACID. By M. F. PISANI.

ANISIC acid being a monobasic acid, having its chloride and its amide, should also have its anhydrous acid corresponding with benzoic acid. I have prepared this anhydrous acid.

I obtained it by treating, according to M. Gerhardt's process, anisate of soda with oxy-chloride of phosphorus.



The anisate of soda should be previously dried, then reduced to powder, and heated in a flask with the quantity of oxy-chloride of phosphorus required by theory. When the reaction is terminated, it is redissolved in cold water and filtered. After the anhydrous acid, which remains on the filter, has been washed, it is pressed between folds of blotting paper, and then crystallised in ether.

Anhydrous anisic acid crystallises in small silky needles, which are grouped around a common centre. It is insoluble in water, and fuses in that liquid at the boiling temperature. Alcohol and ether easily dissolve it, especially with the aid of heat; it is more soluble in a mixture of the two. It is insoluble in potassa and ammonia, and it is only with heat that these alkalis convert it into ordinary acid. Long boiling with water effects the same transformation. It melts at 99° C. (210° 2' F.), and distils at a higher temperature. It gave by analysis:—

Found.	Calculated.
C = 66.95	67.13 { C ⁸ H ⁷ O ³ }
H = 5.17	4.89 { C ⁸ H ⁷ O ³ } O.

Action of Heat on Anisate of Ammonia.—It is known that the ammoniacal salts of benzoic, cuminic, and some other monobasic organic acids give, when they are heated, a nitrite, at the same time disengaging water. I had thought that anisate of ammonia would give the same reaction, but it did not do so; for this salt is divided by heat into ammonia and ordinary anisic acid, having the same crystal-

line form, and the same properties; only, its fusing point is a few degrees higher. The following is the analysis of this acid:—

Experiment.		Theory.
C = 62.75	62.99	63.15
H = 5.28	5.40	5.26
		$\left\{ \begin{array}{l} \text{C}^8 \text{ H}^7 \text{ O}^3 \\ \text{H} \end{array} \right\} \text{O}.$

Comptes Rendus, No. 16, April 20.

On GUANINE. By MM. NEUBAUER and KERNER.

GUANINE was discovered by Unger, in Peruvian guano, and was at first confounded with the *Xanthine* which M. Marcel extracted from a calculus. MM. Will and Gorup Besanez have recently found guanine in the excrements of spiders.

To prepare guanine, the authors pursued the following process:—5 kilogrammes of well pulverised guano were made into a clear decoction with water, then mixed with milk of lime containing 3 or 4 kilogrammes of lime; it is digested for three or four hours in an iron boiler at a temperature near the boiling point, until the liquid, at first brown, becomes green, and until the effervescence has ceased; it is passed through a cloth, the residue is treated with a little water, and it is filtered after two hours. The above liquids are then mixed and allowed to settle, and then hydrochloric acid is added, but not to complete saturation, and it is left to repose. At the end of twenty-four hours, the guanine is deposited under the form of a yellowish powder, which is washed with water.

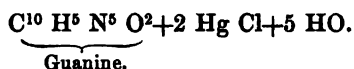
This guanine still contains a little uric acid. It is introduced into a porcelain capsule with a little water, and heated to boiling, and pure hydrochloric acid is gradually added; when the greater part of the matter is dissolved, it is thrown on a filter; the uric acid is retained on the filter; the filtered liquid gradually abandons beautiful crystals of hydrochlorate of guanine, tinted yellow by a coloring matter which is eliminated by recrystallisation.

With this hydrochlorate dissolved in water acidulated with hydrochloric acid, guanine may be prepared by precipitating by ammonia. But, as it is difficult to obtain a pure product by this way, the authors give the preference to the following process; to a concentrated solution of hydrochlorate of guanine is added, hot, an alcoholic solution of bichloride of mercury, until a small portion of the liquid produces a powerful red coloration with carbonate of soda; it is stirred with a glass rod, and it very soon deposits a yellow powder, formed of guanine and bichloride of mercury, which is washed with alcohol, then dissolved in hot water acidulated with hydrochloric acid, and a current of sulphuretted hydrogen is passed in, which precipitates the mercury in the state of sulphuret; the hydrochlorate then separates under the form of colorless and pure crystals.

The authors have confirmed by analysis the following formula given by M. Unger for guanine :—



Guanine forms with the different acids readily crystallisable salts; like urea, it combines with certain salts, and forms with them peculiar combinations. Such is the compound of which we have just spoken, and which has the formula—



It crystallises in microscopical crystals, soluble in the acids and in cyanide of potassium.

The authors also describe some double salts, such as the double chloride of mercury and guanine.

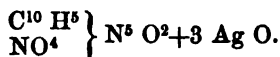


which is formed when, instead of operating with aqueous solutions, alcoholic solutions are employed.

The double nitrate of mercury and guanine is produced in the same conditions as the double nitrate of mercury and urea; like it, it is insoluble, and if the urine contains guanine, the process of estimating urea recommended by Liebig, would encounter an obstacle analogous to that caused by the presence of allantoin.

The authors have also prepared and examined the double chlorides of zinc and guanine, and cadmium and guanine, and they have studied the decomposition experienced by this alkaloid in contact with nitric acid. They have ascertained that in this case guanine acts like tyrosine, which, according to Strecker, gives rise to oxalic acid and nitro-tyrosine.

Nitro-guanine $\left. \begin{matrix} \text{C}^{10} \text{H}^4 \\ \text{NO}^4 \end{matrix} \right\} \text{N}^5 \text{O}^2$ obtained thus is a salifiable base; the authors have prepared nitrate of nitro-guanine, which gives with ammonia and nitrate of silver a bulky orange precipitate, detonating with heat and leaving a residue of silver. This combination is formed of—



Annalen der Chemie und Pharmacie.

*On TRANSFORMATION of MANNITE and GLYCERINE into a
SUGAR properly so CALLED.*

BY M. BERTHELOT.

THE analogies which exist between the alcoholic fermentation of mannite and of glycerine, and the alcoholic fermentation of the sugars properly so called, have led to the opinion that these two fermentations cannot be really distinct: if mannite and glycerine furnish alcohol, it is because they have previously passed to the state of sugar.

In order to examine this question, I have undertaken a great variety of experiments; their results have varied according to circumstances. In the normal conditions of the alcoholic fermentation of mannite and glycerine,—I mean under the simultaneous influences of carbonate of lime and casein, the transformation of glycerine and mannite into alcohol, either of 40 degrees or of 10 degrees,—is effected in a direct manner, without our being able, at any period of the experiment, to seize the least indication of the temporary existence of a sugar properly so called. But the regular course of these experiments is subordinate to the presence of carbonate of lime; when it is not present, the fermentation is not developed; the mannite and glycerine remain unaltered; sometimes, and only in peculiar circumstances, the formation of a sugar properly so called may be observed. I shall give a summary of these various observations.

Mannite and glycerine dissolved in water, were abandoned at the ordinary temperature in contact with all the tissues and integuous substances of animal, natural, or analogous substances which I could procure. In several cases a sugar properly so called was produced, capable of reducing the potassio-tartrate of copper, and of immediately undergoing alcoholic fermentation under the influence of beer yeast. Some of the conditions of this formation of sugar are susceptible of a rigorous definition, the others are exceptional.

Thus, I have observed this formation with albumen, casein,* fibrine, gelatine, the cutaneous, renal, and pancreatic tissues, &c., but always accidentally, and without succeeding in fixing the conditions of the phenomenon.

One tissue only, that of the testicle, caused in an almost regular manner the transformation of mannite and glycerine into sugar properly so called. The following are the circumstances: We take testicles of man or animals (cock, dog, horse), and cut them into small pieces, and leave them in a solution formed of ten parts of water and one part of mannite or glycerine; the weight of the animal tissue (supposed dry) should represent about $\frac{1}{10}$ of the weight of the mannite and glycerine. We operate in an open flask, under the influence of diffused light, and at a temperature between 10° and 20° C. (50° to 68° F.) The tissue remains in general without putrifying; if it becomes rotten, the experi-

* The following are some of the causes of error against which it is as well to be on one's guard in these experiments: 1. Albumen and casein contain small quantities of sugar, from which it is necessary to free them. 2. The mannite of commerce, even the finest, must likewise be purified, for it contains one or two per cent. of sugar. This sugar comes from the manna, which contains 10 or 15 per cent. of it. Almost all analysts have noticed the presence of a sugar in manna; I have verified it in all the samples I could procure, and especially in products as fresh as possible, and of certain origin, which Baron Anca has been kind enough to send from Palermo at my request. The proportion of this sugar in manna does not increase under the influence of time, or of a dark and damp place. Independently of sugar and mannite, manna contains less than half its weight of substances which are almost unknown; thus, the employment of manna in these experiments could lead to no conclusion. 3. The so called purified glycerine of commerce contains a body capable of reducing the tartrate of copper. It is necessary to purify the crude glycerine oneself.

ment has failed. The formation of mouldiness, and particularly of the *Penicillium glaucum*, is likewise prejudicial, although in a less degree. The liquor is tested from time to time; after an interval varying between three months and a single week, we ordinarily prove the appearance of a substance capable of reducing the potassio-tartrate of copper, and of fermenting immediately in contact with beer yeast. At this moment the pieces of testicle are separated by decantation, and they are repeatedly washed until all the mannite or glycerine is removed; in this state they have acquired the property of transforming these two substances into true sugar. To attain this object, the experiment which I have just described is repeated, with the prepared tissues; it generally succeeds, and almost always furnishes a certain proportion of sugar. It is even sufficient to impregnate the tissue with a solution of mannite or glycerine to observe, after a few weeks, a very abundant formation of sugar.

Some experiments performed with dulcine have given rise to similar results.

The sugar thus formed is analogous to glucose in most of its properties; it could not be obtained under the crystalline form; it is very soluble in water, in aqueous alcohol, and in glycerine, from which it can scarcely be separated. It is a very hygrometric body, very alterable during the evaporation of its solutions, susceptible of turning brown under the influence of the alkalis, and of reducing the potassio-tartrate of copper. The ammonio-acetate of lead does not precipitate it in sensible proportion. In contact with beer yeast it ferments immediately, with production of alcohol and carbonic acid. It was very important to ascertain whether this sugar possessed the rotatory power: unfortunately, the facility with which it is colored and altered during the concentration of its solutions prevented me from establishing this point with complete certainty. I only once succeeded in observing a deviation of the tint of passage equal to $5^{\circ} 5'$ in a length of 200 millimetres, with a liquor containing about $\frac{1}{10}$ of sugar; this sugar would, therefore, be levogyrotory, and distinguished from glucose and from most of the other sugars by the direction of its rotatory power. I hope to establish this essential character by further observations.

What is the origin of this substance, and what is the influence which the testicular tissue exerts on its formation?

I will mention one of the most decisive among the numerous experiments which I have made for elucidating this point.

On the 18th of December, 1856, 2 grammes of fresh testicle of the fowl (representing in the dry state 0.280 gr.) 5 grammes of mannite and 50 grammes of water were weighed out, and introduced into a flask communicating with the atmosphere through a tube filled with carded cotton; the flask was left in a laboratory moderately heated. On the 12th of April, 1857, the experiment was ended. The liquor contained 0.250 of sugar properly so called. The pieces of testicle had retained their form and their microscopical appearance; a very attentive examination detected in them some almost inappreciable traces of vegetation. Washed and dried, the insoluble portion of

these pieces weighed 0.230. They had then lost 0.050. This loss is however, more apparent than the real; for the fresh testicles contain a certain proportion of saline and other substances soluble in water; moreover, a portion of the tissue is disaggregated, and likewise becomes soluble without being converted into sugar; all these products are estimated as loss, although they are found in the soluble state and partly coagulable during the evaporation of the liquors. If we take account of these various circumstances, and of the proportion of sugar formed in the preceding experiment and in several others, without speaking of the analogies of composition and constitution which exist between the sugars, mannite and glycerine, we shall be led to regard the sugar produced in the foregoing experiments as resulting especially, or perhaps even exclusively, from the transformation of mannite and glycerine. I may also confirm this conclusion by other experiments in which the testicular tissue has produced, without undergoing perceptible diminution, as much as sometimes consecutively the transformation of mannite into sugar.

These phenomena tend to assimilate the influence of the testicular tissue to the actions of contact, properly so called, observed in mineral chemistry; this interpretation is confirmed by the permanence of the microscopical structure of the testicular tissue in the course of the experiments. But these are probabilities rather than a demonstration. Indeed, the animal tissues do not possess that absolute invariability of composition which often characterises the mineral compounds acting by contact. At the same time that the tissue acts, it is gradually altered; it is decomposed without putrefying, as my analyses testify.

Thus, we cannot positively decide whether the tissue operates by the action of contact by reason of its organic structure, or of its chemical constitution, or indeed, even whether the fact of its decomposition exerts any influence. Finally, the contact of the air, without which these experiments could not succeed, introduces a new complication, for it permits the development of microscopical animal and especially vegetable beings; this development can never be completely avoided, but it seems more prejudicial than favorable to the formation of sugar. In the most successful experiments, the organised beings were least developed; thus, in that of which I have just mentioned the numerical results, their presence was manifested only by a very minute examination.

These details, which I have sought to render as truthful as possible, show how complex are the phenomena of fermentation, and how many unknown or obscure elements they contain; however chemists may put in play the forces which induce them, may make them act on definite bodies, and direct them towards the accomplishment of determinate metamorphoses. It is nearly in the same manner that they make ordinary affinities act, whose intimate nature is scarcely better known to them. The employment of ferments is also distinguished only by the pre-existence of a form, of a peculiar constitution, extremely mobile, and produced beyond our intervention, under the influence of life.

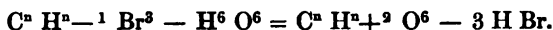
However this may be, the experiments which I have just detailed are distinguished by their synthetical character from the fermentations hitherto known. Instead of changing sugar, mannite and glycerine into alcohol, lactic acid and butyric acid, compounds more simple and more difficult of decomposition, they lead us to converting mannite and glycerine, very stable bodies deprived of rotatory power, and which are very approximate to those which we know how to produce, into a substance endowed with a less stability, and with a higher order of complication, I mean into a variable sugar, analogous to the sugars which are formed, under the influence of life, in the tissues of vegetables and animals.

Comptes Rendus, No. 19, May 11th, 1857.

On the ARTIFICIAL FORMATION of GLYCERINE.

By M. A. WURTZ.

IN my note on acetal and glycols (see THE CHEMIST, Sept. 1856, p. 724), I expressed my opinion that the processes adopted for the synthesis of the glycols or diatomic alcohols, should lead to the artificial formation of the glycerines or triatomic alcohols. A glycerine derived indeed from a bromide of carburetted hydrogen, $C^a H^{a-1} Br^3$, by the substitution for each equivalent of bromine, of an equivalent of oxygen, and an equivalent of water:—



These bromides $C^a H^{a-1} Br^3$ may be prepared by various processes; and, if we turn to account the powerful affinity of binnine for silver, we are able indeed to realise the substitution in question. I communicated in my note three means of preparing the compound, $C^6 H^5 Br^3$. One of these means consists in treating the iodide of allyle $C^6 H^5 I$, or ioduretted propylene of M. Berthelot by bromine. The iodised liquid is placed in a flask with a long neck, surrounded by a frigorific mixture, and one and a half times its weight of bromine is poured into it by small portions at a time. The iodine is separated and is found in the crystallised state in the cooled liquor, whilst three equivalents of bromine are fixed in the group $C^6 H^5$.

Properly purified, the tribromide which is thus obtained, is presented under the form of a colorless dense liquid, or slightly rose-colored, when it retains a trace of iodine. Exposed to a temperature below $10^\circ C.$ ($50^\circ F.$), it crystallises in beautiful colorless prisms, fusible at $16^\circ C.$ ($60^\circ 8' F.$). It distils without alteration at $217^\circ C.$ ($422^\circ F.$). 205 grammes of acetate of silver were treated with 115 grammes of this tribromide, dissolved in five or six times its volume of crystallisable acetic acid. The mass thus formed was heated for eight days in an oil-bath to from 120° to $125^\circ C.$ (248° to $257^\circ F.$) The reaction being completed, the contents of the flask placed on a filter, and the bromide of silver washed with ether.

The liquor having been distilled until the thermometer marked

140° C. (284° F.), the residue was treated with lime and ether. The perfectly colorless ethereal solution, left by evaporation on a water-bath, 15.5 gr. of a neutral yellowish oil, which was submitted to distillation.

Only a few drops passed over below 250° C. (458° F.), and a greater part of the liquid distils at 268° C. (514° F.) The product, which is perfectly colorless, is neutral, possessed of a slight acetic odor, heavier than water, at the bottom of which it forms an oily layer. Agitated with a great excess of water, it is dissolved. It is soluble in all proportions, in alcohol and in ether. It contains:—

	Experiment.	$C^{18}H^{14}O^{12}$
Carbon	49.12	49.5
Hydrogen	6.60	6.4

The formula $C^{18}H^{14}O^{12}$ is that of triacetine. The product analysed contained, indeed, three equivalents of acetic acid, and one equivalent of glycerine; 0.647 gr. of this artificial triacetine were saponified with baryta water. The liquor, freed from the excess of baryta, by a current of carbonic acid and by ebullition, was precipitated by sulphuric acid; 0.974 gr. of sulphate of baryta were obtained. This quantity of baryta salt corresponds to 2.8 or three equivalents of acetic acid.

To obtain artificial glycerine, triacetine was saponified with baryta water, and the liquor freed from the excess of baryta was evaporated to dryness. The residue was redissolved in absolute alcohol, to which a small quantity of ether was added. The ethereo-alcoholic liquor, evaporated on a water-bath, left glycerine which was distilled *in vacuo*. Towards 200° C. (392° F.), a very thick, perfectly colorless liquid passed over, of a decidedly sweet taste, soluble in all proportions in water and alcohol, insoluble in ether. This liquid gave by analysis:—

C	38.5
H	8.6

The formula $C^6H^8O^6$ requires—

C	39.1
H	8.6

To prove by another reaction its identity with glycerine, it was treated with iodide of phosphorus, Ph I; a very vivid reaction was set up, and ioduretted propylene was obtained.

I shall develop hereafter the theoretical consequences, which may flow from the facts I have just detailed. For the moment, I will confine myself to establishing, that the tribromide $C^6H^8Br^3$, fusible at 16° C. (60° 8' F.) and boiling at 217° C. (422° F.), is not identical with tribromhydrine. According to the experiments of M. Berthelot, this last compound is a colorless liquid, fuming slightly in the air, and distilling towards 180° C. (356° F.) If, nevertheless, the solid tribromide gives a glycerine compound in reacting on acetate of silver, this fact must doubtless be attributed to a molecular change, occurring at the very moment of the reaction.

Comptes Rendus, No. 15, April 13, 1857.

ANALYSIS of the EDIBLE TRUFFLE (TUBER CIBARIUM, BULL.)

By M. J. LEFORT.

ALMOST all the truffles found in commerce in Paris come from Périgord, Dauphiné and Var, and belong to the black variety. The first which are collected are white, or greyish inside; but when they approach maturity, that is to say, in the early part of December, their parenchyma assumes a deep brown tint. Those which remain in the earth become white again, and then decompose. The experiments which we have made related to the tubercle in its different stages of maturity, and after having been carefully freed from the earth. We found that it has the following composition:—

Water	Chlorine
Odorous principle	Potassa
Vegetable albumen	Soda
Mannite	Lime
Fixed fatty matters	Magnesia
Brown coloring principle	Oxide of iron
Cellulose	Silica
Citric acid	Sulphuric acid
Malic acid	Phosphoric acid

1. The quantity of water which a very ripe and very healthy truffle contains, is 70 per cent. The white truffle contains 71.25.

2. The perfume of the truffle possesses very great diffusibility; it does not appear to belong to the category of essential oils.

3. The truffle reduced to paste and treated with cold water, gives a yellowish, turbid, and very aromatic liquid, which passed through a fine cloth, then heated to boiling, precipitates a small quantity of vegetable albumen.

4. We have said elsewhere that it was sufficient to concentrate the juice of champignons to obtain crystallised mannite; the truffle likewise contains mannite, but in combination with bimalate of lime. It is found as well in the white truffle as in the black; but the former contains, moreover, a small quantity of pectine, which disappears when the tubercle arrives at maturity. It is probable that this pectine is converted into mannite, during the life of the vegetable.

5. The decoction of the truffle, abandoned in sufficiently large quantity to the temperature of 35° to 40° C. (95° to 104° F.) with beer yeast, did not undergo alcoholic fermentation, a result which is opposed to that which Vauquelin, Bracconnot and myself have observed with the cultivated champignon.

6. Parmentier had already remarked, that the truffle contained a free acid. It is sufficient, indeed, to place a certain quantity of fresh pulp of the truffle on a sheet of litmus paper to obtain the acid reaction. We easily proved in this cryptogam the presence of citric and malic acids; but all the experiments undertaken with the view of detecting in it the presence of fumaric acid have been fruitless. This acid exists in the edible champignon.

7. The truffle cut in thin slices and exposed to the air, speedily loses the characteristic perfume; it ultimately gives out a strong, disagreeable odor (*sui generis*), which arises from a fixed fatty matter. Its extraction may be very well effected, but always in small quantity, by means of sulphuric ether, which is digested with the tubercle reduced to powder. We then obtain a substance of butyrateous consistence, yellowish, crystallisable in very small mamalons at 35° C. (95° F.), it flows like a thick oil. The alkalies readily dissolve it with the aid of heat, and the liquor is precipitated when a greater quantity of water is added. The mineral acids likewise precipitate it, and the fatty matter reappears with all its original characters. This reaction proves, that like the fatty matter of the edible champignon, that of the truffle is not saponifiable by the alkalis.

8. In the cultivated champignon, the coloring principle is especially accumulated in the spores. It is the same with the truffle. A white truffle cut in thin slices and examined with a microscope, shows two distinct parts; one white, opaque, and solid, and retaining throughout the duration of the vegetable its form and color, and which constitutes the variously anatomised veins which the eye distinguishes in a tubercle cut in two parts; this substance is cellulose: the other, white, transparent, parachymatous, which shows with the microscope a great number of flattened discs, which we can compare to nothing better than the globules of the blood. These organs, which are disseminated in all parts of the tubercle, are sporanges which, subsequently, give rise to the spores. It is the latter which, owing to their great number, communicate to the ripe tubercle the well known brown tint. According to our researches, the coloring matter of the truffle is a peculiar principle, which we believe to be identical with that of the cultivated champignon, but which, at all events, never acts like a mixture of ulmine and ulmic acid. It is known that Braconnot regarded the coloring principle of the *agaricus atramentarius* as a mixture of ulmine, ulmic acid and carbonaceous earth.

9. All these experiments enable us to conclude, that with the exception of the fermentable sugar and the fumaric acid, which exist in the cultivated mushroom, the edible truffle possesses the same constituent principles as that cryptogam.

Comptes Rendus, No. 18, May 4, 1857.

PRE-EXISTENCE of VALERIANIC ACID in the FRESH ROOT of VALERIAN.

By M. PIERLOT.

THE author sums up in the following terms, the results of his researches:—

Valerianic acid pre-exists in the fresh root of valerian, of which it constitutes one of the proximate principles. It is found in the water of vegetation, free from any saline combination. It may be directly extracted, without the intervention of any chemical agent. The fresh root contains more of it than the dried root.

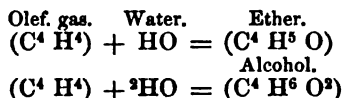
Comptes Rendus, No. 15, April 18, 1857.

On HYDRIDE of AMYLE.

By T. AND H. SMITH, EDINBURGH.

In presenting a specimen of the new anæsthetic discovered by Professor Simpson, the hydride of amyle, and which we had the pleasure of preparing for him, it may not be out of place to make a few remarks regarding its history, chemical characters, and physical properties.

Dr. Frankland, Professor of Chemistry, Owen's College, Manchester, discovered it in the course of an investigation undertaken for another object. It had long been disputed whether ether and alcohol were mere hydrates of olefiant gas,



or whether they were constituted by the union of a compound radical ethyle ($C^4 H^5$) with oxygen ($C^4 H^5$) + O = ($C^4 H^5 O$) in the case of ether; alcohol being merely its hydrate ($C^4 H^5 O$) + HO = ($C^4 H^6 O^2$).

This last view had been advocated strongly by Liebig, although the very existence of organic radicals remained at that time entirely hypothetical; yet Liebig expressed his conviction that the ethyle radical ($C^4 H^5$) would be obtained in a separate state.

Dr. Frankland, in entering on the interesting and able investigation of which the hydride of amyle was one of the fruits, was influenced by the desire of deciding this question by separating the radicals themselves, and more especially those of the different alcohols and their derivative ethers. The subject was undoubtedly a most difficult one, and required a clear head, profound chemical knowledge, and nice accuracy of manipulation.

Dr. Frankland began with wine alcohol, and unquestionably separated the radical ($C^4 H^5$) ethyle, but which existed as a vapour even below the zero of F. He concluded, from the much higher equivalent of amylic alcohol ($C^{10} H^{12} O^2$) and the higher density of its vapor, that the radical amyle, if he should succeed in obtaining it, would exist as a liquid at ordinary temperatures, and thus give the means of ascertaining its nature more certainly, and also give a clue to a correct understanding of the character and functions of the compound radicals in general, and more especially of those which exist in the class of bodies called alcohols, on account of the great theoretical and practical interest belonging to them in a chemical point of view.

The method by which Dr. Frankland obtained and proved the existence of amyle, the radical of amylic alcohol, was to heat for several hours in an oil-bath iodide of amyle, prepared from pure fusel oil by the simultaneous action of phosphorus and iodine on it,* in sealed

* The iodide of amyle is a colorless, transparent, heavy liquid; density 1.5. It is not readily decomposed, not changing color even when kept for a long time in diffused daylight, although the direct rays of the sun decompose it easily. It mixes readily in all proportions with oils and ointments. In consequence of this,

glass tubes along with granulated zinc, at a heat of 190° C. (374° F.), and then distilling off the products of decomposition. The first two-thirds came off at a heat of about 80° C., and the remaining third at about 160° C. (320° F.) The first distillate on rectification proved to be a liquid boiling at about 55° C. (95° F.), and on acting upon the liquid with anhydrous sulphuric acid, he separated a carbo-hydrogen polymeric with olefiant gas $C^{10}H^{12}$, and on then distilling the part unacted on by the acid, he obtained a liquid having the composition $C^{10}H^{12}$, with the very low boiling point of 30° C. or 86° F., which, in place of having an unpleasant odor and tar-like taste as the mixture had, possessed, on the contrary, a grateful fruity odor, and was tasteless.

This was the hydride of amyle, whose decided and energetic anæsthetic action Dr. Simpson has just discovered.

It is a clear colorless mobile liquid, and is the lightest fluid known, having the very low density of 0.638 at 57° F., and a vapor density of 2.5. It is very inflammable, and burns with a brilliant white and smokeless flame. It is insoluble in water, but readily soluble in alcohol and ether. It is a very stable compound, resisting the action of fuming sulphuric acid and the most powerful oxydising agents. Its rational formula is $C^{10}H^{11} + H$, and therefore the hydride or hydruret of the radical amyle.

The liquid that was distilled last from the tube at a high heat proved to be the pure radical, and which had now been got in a separate form for the first time. This liquid is colorless and clear, of a density of 0.770, and boils at 155° C. (311° F.), and the density of its vapor is 4.9.

As Dr. Frankland found in his previous investigations for the discovery of the radical of wine alcohol, that on exposing the iodide of ethyle with an equal bulk of water along with zinc in sealed tubes to the action of heat, the only product was the hydride of ethyle, he was led to try the same process with the iodide of amyle, and the result of the use of water in the reaction was exactly analogous. Instead of a mixture of amylene, amyle and its hydride, he now obtained as the only product of decomposition the hydride of amyle, which distilled off at a very low heat, and after digesting the liquid for 24 hours with caustic potash, and redistilling at 35° C. (95° F.) was obtained quite pure.

He also found, that by the addition of water to the iodide and zinc, the decomposition easily took place at a heat of 140° C. (285° F.), whilst with the iodide and zinc alone a heat of 190° C. was required, besides the necessity of amalgamating the zinc.

On proceeding to prepare the hydride for Dr. Simpson, we very soon discovered the unsuitableness of sealed tubes, as used by Dr. Frankland, for its preparation in sufficient quantity to test its powers as an anæsthetic. We therefore got much larger tubes made, and

and the large quantity of iodine—two-thirds of its weight—which it holds in a perfect state of solution, it seems to us to be worthy of attention as an outward application, and all the more that the iodine is in the most favorable condition for being absorbed.

carefully annealed, of one inch internal diameter, and $\frac{3}{8}$ of an inch thick; and instead of being sealed, which necessarily involved their destruction at each operation, we had them effectually closed by accurately ground stoppers, and these again strongly fixed down in their places by a metal bridle, with a binding screw. But from the enormous pressure, not less than 300 lbs., or 20 atmospheres, by calculation, to the inch, there was extreme difficulty in getting the stoppers perfectly tight. Seeing, however, superior advantages in this plan over the other, if the stopper could be made tight, we were induced to persevere, and at length, with great care in the grinding, succeeded in making an absolutely tight tube, even under such great pressure.

But from the great risk of explosion with such sized tubes, we afterwards gave up the idea of glass ones altogether. One of these, after being exposed to the usual heat with a charge for more than an hour, burst into innumerable fragments with a tremendous explosion, breaking the thermometer and blowing out the sides of the bath, whose scalding contents were scattered about in all directions, but fortunately without doing any personal injury. This occurrence proved the necessity of adopting another plan, and we therefore thought of a copper tube, which idea, however, was nearly put aside, under the fear of a strong galvanic action being induced, and interfering with the desired action; but, on reflection, such a result seemed to be of a favorable nature rather than otherwise, and we have now found that a copper tube answers admirably—and, in fact, part of what we made for Dr. Simpson was prepared in one.

A key to the explanation of the formation of the hydride of amyle is found in the tendency of the radical amyle to combine, not only with electro-negative, but also with electro-positive elements. Thus, there are not only compounds like the following:—

Amyle.		Electro-negative elements.	
$C^{10} H^{11}$	+		O
$C^{10} H^{11}$	+		Cl
$C^{10} H^{11}$	+		Br
$C^{10} H^{11}$	+		I;
but also—			
		Electro-positive elements.	
$C^{10} H^{11}$	+		Z
$C^{10} H^{11}$	+		H.

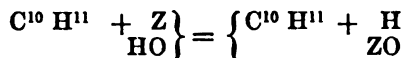
Dr. Frankland succeeded in preparing both zinc methyle, zinc ethyle, and zinc amyle. These have all an intense attraction for oxygen. In the case of zinc methyle, it equals potassium in that respect; a small glass tube, containing only a few drops of the liquid zinc methyle, actually became red-hot on being placed under water, from the great intensity of the chemical action. The zinc ethyle is a liquid of 1.18 density, boiling at $118^{\circ} C.$ ($244^{\circ} F.$), and having a vapor density of 4.25.

All these metallo-organic bodies have such an attraction for oxygen,

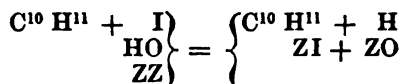
that the most extraordinary precautions are necessary in preparing and distilling them, both to get quit of every trace of water, and to exclude the presence of air by replacing it, either with an atmosphere of hydrogen, nitrogen, or carbonic acid.

The moment one of these metallo-organic compounds comes in contact with moisture, it is decomposed; the zinc takes the oxygen and the radical the hydrogen from an equivalent of water.

One of the methods of analysing it is, indeed, founded on this character; thus, in the case of the amyle compound the action is as follows:—



And the production of hydride of amyle appears to be as here represented:—



Consequently, in distilling after the compulsive action of heat and great pressure, the hydride comes over, and there remains in the digester an oxyiodide of zinc.

In preparing the hydride of amyle the first step necessary is the purification of the crude fusel oil, which is a mixture of propylic ($\text{C}^6 \text{H}^8 \text{O}^2$), butylic ($\text{C}^8 \text{H}^{10} \text{O}^2$), and amylic alcohols ($\text{C}^{10} \text{H}^{12} \text{O}^2$), and of other alcohols much higher in the series; so that the boiling point, as we found, rises much higher than even 400°F . before the distillation comes to a close. As the boiling point rises from that of propylic alcohol (203°F) to that of amylic alcohol (269°F), it can hardly be doubted that the hydrides of the corresponding radicals would vary in their boiling points in at least an equal degree; and as the hydride of amyle has a boiling point of 86°F ., there can be little question that the boiling points of others lower in the series would be so low, that at ordinary temperatures they would exist only as vapors. Dr. Frankland accordingly found that the hydride of ethyle ($\text{C}^4 \text{H}^6 + \text{H}$) still remained gaseous, even when submitted to twenty atmospheres of pressure.

A question here suggests itself to the mind; may not these hydrides existing only in vapor at ordinary temperatures, be anæsthetics themselves, and if so, may their use as such be not an altogether impractical idea?*

From what is known of the volatile liquids got from wood and coal tars and bituminous minerals, these naturally suggest themselves as cheap and ready sources of the hydride of amyle, but from these it does not appear to us that it could be obtained in a sufficient state of purity as an anæsthetic, even though it may be found to form one of their ingredients.

* For instance, may not their saturated solution in alcohol, in which they are readily soluble, give a ready method of obtaining their use as anæsthetics?

Moreover, the difficulty of separating it from other ingredients, either by fractional distillation or otherwise, would be so great that success would be obtained at too costly a rate; and, after all, we fear the hydride of amyle would hardly fail to repel by the taint attaching to it from such sources.

With a view of getting quit of the expense of the iodine, which, however, can be recovered, we thought of trying the chloride in place of the iodide of amyle, but completely failed in getting the hydride from it, although the digester was subjected to the same heat as the iodide, and for double the time. On removing the preserve from the plug, after proper cooling, an escape of gas took place with a loud report, showing the presence of an uncondensable gas, most probably hydrogen; and on subjecting the contents to distillation, nothing came over till the heat reached the boiling point of the chloride, when nearly the whole of it distilled: besides, the zinc, instead of being matted together, as in the case of the iodide, by the formation of oxyiodide of zinc, was found in the tube quite loose.

In making the iodide of amyle, when the fusel oil has been nearly saturated with iodine, a solid crystalline matter separates, which becomes more abundant on the cooling of the liquid. So far as we have examined the compound, it seems to be a subiodide of phosphorus. It fumes strongly on exposure to the air, and on throwing it, after being firmly pressed in blotting paper into a glass tube containing water, a hissing like red-hot iron in water is produced, and on applying a light to the escaping gas, it takes fire and burns with a brilliant phosphorus flame, and gives abundant white fumes of phosphoric acid, and leaves a red crust of suboxide of phosphorus (?) on the mouth of the tube. The gas appears to be phosphuretted hydrogen. The water on examination was found to contain hydriodic and phosphoric acids. Heated in a test tube it melts at a low heat and boils, giving a sublimate, which condenses in crystals nearly colorless on the sides of the tube. On applying a light to it in the open air it takes fire, burning with a weak flame and the production of a violet colored smoke. Further investigation into its nature has meanwhile been prevented by falling short of material.

Pharmaceutical Journal.

NOTE on the PROTO-SULPHIDE of CARBON or SUB-SULPHURET of CARBON, CS.

By M. ERNEST BAUDRIMONT.

It is known that there exists a sub-sulphuretted carbon CS^2 corresponding to carbonic acid, CO^2 , but the sub-sulphuretted carbon, CS, corresponding to carbonic oxide, CO, was not hitherto known. I have the honor of announcing to the Academy of Sciences, the discovery of this compound, which I have been studying for some months past.

I will confine myself, for the purpose of securing priority, to indica-

ting briefly the preparation, properties and analysis of this new body, intending hereafter to present a complete work on this subject.

This sub-sulphuret of carbon is obtained :—

1. By decomposing the vapor of ordinary sulphuret of carbon CS^2 , by means of spongy platinum or pumice stone heated to redness ; in those circumstances, CS^2 is divided into an abundant deposit of sulphur which obstructs the pores, and into a gaseous body, which is the sub-sulphuret, CS ; this reaction, which is very clear, sufficiently explains the formation of the proto-sulphide of carbon.

2. It is likewise obtained in the ordinary preparation of the liquid sulphuret CS^2 , and simultaneously with it.

3. By the decomposition of the vapors of CS^2 , at a red heat with the aid of pure lamp-black, wood charcoal, and especially of animal charcoal in pieces.

4. By the decomposition at a red heat of CS^2 by hydrogen.

5. By the calcination of sulphuret of antimony with an excess of carbon.

6. By the reaction at a red heat of carbonic oxide on sulphuretted hydrogen,



7. By the reaction of sulphurous acid on bicarburetted hydrogen at a red heat.

8. By that of bicarburetted hydrogen on chloride of sulphur at a red heat.

9. It is produced during the decomposition of sulphocyanogen by means of heat, &c.

The first process gives this gas very pure ; the other methods give it mixed with sulphuretted hydrogen and carbonic oxide. It is purified by passing it rapidly through solutions of acetate of lead, protochloride of copper dissolved in HCl, and then, drying it, it is collected over mercury.

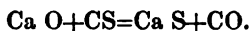
It is gaseous, colorless, of an odor resembling that of ordinary sulphuret of carbon, but not disagreeable, and powerfully etherial. Breathed in too large quantity, it appears to be powerfully anæsthetic. It burns with a beautiful lambent flame, producing carbonic acid, sulphurous acid, and a little sulphur. Its density is rather greater than that of carbonic acid. It resists the cold of a refrigerating mixture of ice and salt. Water dissolves nearly its own volume of it, but it decomposes it very rapidly into sulphuretted hydrogen and carbonic oxide,



It is scarcely more soluble in alcohol or ether.

Protochloride of copper in solution does not dissolve it. A solution of acetate of lead is not blackened by it immediately, but after a contact of some hours, the liquor turns black ; after several days the salt is completely transformed into carbonic oxide and sulphuret of lead. In contact with the alkaline solutions of potassa, soda, &c., its decomposition is rapid. With lime-water, for example, it reacts

by giving sulphuret of calcium and a volume of carbonic oxide equal to that of the gas employed. This remarkable reaction clearly establishes its composition,



No carbonate of lime is produced. At a red heat it is feebly decomposed; 1, by spongy platinum; 2, by steam into HS and CO; 3, more easily by hydrogen into HS and a carburetted hydrogen; 4, entirely by copper into graphitoid charcoal and sulphuret of copper; 5, finally, exposed to the sun with its own volume of chlorine, there is a reaction, partial condensation and formation of products, which I am now studying.

Analysed by oxygen in the eudiometer, it gives equal volumes of carbonic acid and sulphurous acid, from which CS is deduced as its composition. Moreover, this composition is clearly established by the action exerted on it by lime-water, which, as I have said, produces the gas CO, and sulphuret of calcium, and no carbonate of lime. The same conclusion is also arrived at by the determination of the quantities of carbon and sulphuret of copper which it furnishes in acting on that metal.

I here limit the narration of my researches, intending soon to present to the Academy a complete memoir on this interesting body, contenting myself with announcing its existence for the present.

Several chemists have attempted the discovery of this product, and if it has hitherto escaped their researches, it is doubtless because of its reaction on water, and on the alkaline solutions which convert it into carbonic oxide and sulphuretted hydrogen.

Comptes Rendus, No. 19, May 11, 1857.

ANALYTICAL CHEMISTRY.

NEW PROCESS for DETECTING IODINE and BROMINE; PRESENCE of IODINE in the WATER of VICHY.

By MM. OSSIAN HENRY, jun., and E. HUMBERT.

THE question of the presence of iodine in the waters of Vichy, has, of late years, given rise to a controversy which is not yet definitively settled. On one hand, several chemists* maintain that iodine is always to be found in this mineral water, whilst others hold a contrary opinion. Feeling persuaded that this difference of opinion is owing solely to the insufficiency of the reactions brought into play in researches of this nature, we have proposed, with the view of definitively clearing up this contested point, a new analytical method which has enabled us to demonstrate the existence of iodine in Vichy water, so as to place it beyond all doubt.

Our process presents a great analogy to that which we have re-

* M. O. Henry, sen., who first detected iodine at Vichy (1848-4); and subsequently, MM. Chevalier, Gobley, Lefort, and Poirier.

cently recommended for detecting traces of hydrocyanic acid in cases of poisoning: although it is applicable to every possible case, we will suppose that we are acting on a mineral water.

The mineral water, or the more or less concentrated residue of its evaporation, is treated with acid nitrate of silver. The precipitate which is formed, will contain, in the state of salts of silver, the chlorine, bromine, and iodine contained in water. It is washed and carefully dried. In this state, it is intimately mixed with a small quantity of cyanide of silver, and then introduced into a tube, at one end of which are fixed two small pledgets of tow or asbestos. It only remains to pass slowly over the mixture a current of very dry chlorine, at the same time gently heating the corresponding portion of the tube. The iodine, bromine, and cyanogen are displaced, they combine and condense in the colder parts of the tubes, under the form of white and crystalline rings of iodide and bromide of cyanogen. The tube is then closed at both ends, and may be used when required for the purpose of arriving at a conclusion.

The iodide and bromide of cyanogen possess physical and chemical properties which prevent them from being confounded with other compounds: the iodide sublimates at 45°C . (113°F .), and the bromide at 15°C . (59°F .) This difference in their point of volatilisation enables us to separate them mechanically, by immersing the tube which contains them into water at 30°C . (86°F .); the bromide alone ascends to the upper part of the tube, which should be first suitably cooled.

We will not here dwell on the chemical characters of these combinations; suffice it to say, that it is easy to determine their nature clearly by obtaining with them the principal reactions which characterise iodine and bromine. Care must be taken to prepare the chlorine necessary for this operation with pure substances, and especially free from chlorine: besides this precaution, the apparatus should also be made to operate empty for a short time. For this purpose, a little pure cyanide of silver is introduced into the condensing tube; if no trace of iodide or bromide of cyanogen sublimates, the operation may be proceeded with.

We may remark, in concluding this note, that, by following the experimental method which we propose, no matter is caused to enter into the mineral water to which can be attributed the iodine and bromine discovered during the experiment.

Comptes Rendus, No. 12, March 23, 1857.

INORGANIC CHEMISTRY.

OBSERVATIONS on some CHEMICAL DECOMPOSITIONS by means of the BATTERY. By M. SCHLAGDENHAUFFEN.

I.—DECOMPOSITION OF NITRIC ACID AND THE METALS.

1. ONE of the causes of the constancy of double fluid batteries is the absorption of hydrogen, in proportion as it is produced.

In Bunsen's battery this gas, in the nascent state, reacts on nitric acid, and generates water and red vapors, which remain dissolved in the undecomposed acid. While the latter is gradually weakened by the successive formation of an equivalent of water, the red vapors are decomposed in this new liquid, and are converted into nitrous and nitric acids. The presence of nitrous acid is easily proved; it is sufficient to saturate the acid mixture with carbonate of potassa and to treat the salt obtained with concentrated sulphuric acid to see an immediate disengagement of red vapors; moreover, with iodide of potassium and starch, in presence of a little sulphuric acid, there is an intense blue coloration.

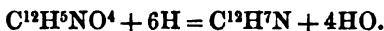
The nitrous acid, once liberated, is decomposed in presence of nascent hydrogen, and forms, under the influence of the current, water and ammonia. I have demonstrated the presence of ammonia in various cases of decomposition, either by taking more or less concentrated nitric acid, or by using alkaline or metallic nitrates. To detect it, I took three Bunsen's elements, in order to increase the intensity of the current; two in porous vessels were charged in the ordinary way; the third received the liquids intended to be decomposed. In each case the current was continued for six hours. At the end of this time the concentrated nitric acid, the acid diluted with half or one-fourth of its bulk of water, as well as the nitrates of potassa, baryta, chrome, and mercury, were decomposed. It was sufficient, to show the presence of ammonia, to saturate the liquor with potassa in excess, and to heat it in a tube to recognise the volatile base by its characters. The nitrates were rapidly decomposed; indeed, it appears that when once the separation of the base from the acid is effected under the influence of the current, the acid which has become free is attacked by the nascent hydrogen and converted into ammonia.

2. The decomposition of the alkaline and metallic nitrates being made without difficulty, I wished to investigate whether that of the nitrates of organic bases would occur in an analogous manner. I arrived at the same result; only instead of making use of only three Bunsen's elements, I increased the number of cells. I charged the first five porous vessels with nitric acid; into the sixth, I poured a solution of nitrate of ethyle in alcohol; then I closed the air out, as in the experiment already mentioned. After six hours I proved the presence of ammonia resulting from the decomposition of the acid of the organic nitrate. Nitrite of ethyle, heated in the same way, likewise furnished ammonia. The nitrates and nitrites of amyle gave the same result. From these experiments it may be concluded that, under the influence of the current and of the nascent hydrogen of the battery, inorganic and organic nitrates are in general decomposed, and that the free acid is rapidly converted into ammonia.

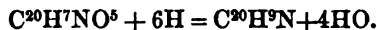
3. Other organic compounds, which instead of containing nitric acid in the state of NO^5 contain only the molecule NO^4 , are likewise reduced: we obtain in this case an amide body. These reductions of nitrated compounds are, then, the same as those which may be ef-

fects with sulphuretted hydrogen, sulphuret of ammonium, or the proto-salts of iron.

I submitted nitro-benzine and nitro-naphthaline to the influence of the current. The battery was composed, as before, of six Bunsen's elements, five of which were charged with nitric acid, and into the diaphragm of the sixth I poured 20 grammes of nitro-benzine dissolved in water. At the end of 12 hours, the nitro-benzine had lost its odor. I distilled the liquid of the porous vessel, in order to collect the unaltered nitro-benzine; then, treating the product with lime, I collected in the receiver a few grammes of aniline, well characterised by its odor and its coloration in presence of the hypochlorites. The crude product of this distillation was converted into sulphate, then the sulphate was decomposed with lime, in order a second time to recognise in the base its characters, which have already been indicated. The action of nascent hydrogen on nitro-benzine may be expressed by the formula—



Nitro-naphthaline submitted to the action of the battery and of nascent hydrogen gave an analogous result. The following formula accounts for this reaction:—



Napthalidam, which was obtained in this reaction, was characterised by its peculiar odor, its violet coloration in the air, its volatility, and the decomposition of its sulphate by means of lime.

4. The examination of the liquor surrounding the porous vessel charged with nitric acid proves that there are considerable proportions of nitric and nitrous acids in the free state. Indeed, poured on a mixture of sulphate of iron, the solution gives to this salt the brown coloration which characterises both the nitrates and the nitrites; moreover, in presence of iodide of potassium there is an intense blue coloration, which indicates the presence of nitrous acid.

When the porous vessel has been charged with nitrate of potassa or a metallic nitrate, nitric acid filters through this vessel, on the side of the positive pole, but scarcely any nitrous acid was found in it.

The passage of the acids through the porous vessel has the effect of polarising the current. This polarisation should be in the direct ratio of the quantity of acid which goes from the negative pole to the positive pole, and, consequently, it should be less in the decomposition of a nitrate than in that of free nitric acid.

5. When a Bunsen's battery has been in full activity for some days—and especially when it has been charged with concentrated nitric acid—there is observed round the charcoal a white residue which fills up the pores of the porous vessel, and greatly diminishes the intensity of the current. Under this white spongy mass is found a grey crust adhering strongly to the carbon. The greater part of the white residue is a mixture of nitrite and basic sulphate of zinc; the grey crust is oxide of zinc mixed with a certain quantity of metallic

zinc. I ascertained this in the following manner: I washed the residue with warm water; then, after having dried it, I boiled it with caustic potassa. The filtered liquor was evaporated to dryness, and heated with concentrated sulphuric acid. The red vapors, which were abundantly disengaged, proved the presence of a nitrite. This done, the latter residue was evaporated on a sand-bath for five hours, in order to expel the last portions of red vapors, and then heated in the open fire in a retort, with an excess of sulphuric acid. Red vapors were no longer disengaged, consequently there was no nitric acid in the residue.

The presence of sulphuric acid in the spongy mass was easily demonstrated by the addition of a salt of baryta to the hydrochloric solution of the deposit.

The formation of these insoluble salts is not difficult to explain. Indeed, let us imagine that the sulphate of zinc passed from the exterior vessel into the porous vessel; when it has arrived there it is decomposed. The oxide of zinc is not formed immediately, for the nitrous acid dissolves the excess of it. When once the nitrate of zinc is formed, the current decomposes it, still leaving the oxide in the inner vessel, and removing the nitrous acid which goes towards the positive pole. At a certain period, the acid and oxide form together the basic salt.

The formation of the basic sulphate must occur absolutely in the same manner.

The grey residue adhering strongly to the carbon, contains metallic zinc, which is protected from the solvent action of the nitric acid by the layer of basic salts which cover it.

When the spongy mass is boiled with hydrate of baryta, instead of taking potassa, we obtain, by double decomposition, soluble nitrite of baryta, oxide of zinc, and sulphate of baryta. If we are careful not to take an excess of baryta, we obtain pure nitrite of baryta, which serves for the preparation of other nitrites. This method of preparing the nitrites has the advantage of obtaining them free from nitrate.

6. It is to be remarked that when the decomposition of the nitric acid of the battery commences, there is an increase of temperature, which arises from the combination of the nascent hydrogen with the oxygen of the nitric acid. It is easily proved, by means of a thermometer placed inside the porous vessel which contains the acid. By measuring the intensity of the current by means of a *compass with tangents*, I ascertained that the maximum of temperature corresponds with the maximum of intensity, but the thermometer remains longer at the maximum than the compass. The readings of the compass and the thermometer were made simultaneously. If the result of these observations is represented by curves, the abscissæ of which designate the time and the ordinates the intensity of the current and the temperature, two curves are obtained nearly parallel to a very great distance. After about two hours' observation, the temperature again became what it was at the commencement, whilst the intensity went on decreasing.

The parallelism between the curb of the temperatures and that of the intensities was established in the cases of the decomposition of the nitrate, chromate, hypermanganate, and other salts mentioned further on.

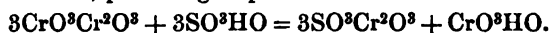
II.—DECOMPOSITION OF CHROMIC ACID AND THE CHROMATES.

1. I charged the porous cell of a Bunsen's element with chromic acid dissolved in water, and the outer vessel with sulphuric acid diluted to one-twentieth in volume. It must be foreseen that the nascent hydrogen, under the influence of the current, would decompose the chromic acid. No chromic salt was formed. At the end of three hours, a red brown solution of chromate of chromium was obtained according to the formula—



As soon as this salt is formed, the current decomposes it; the base goes to the carbon pole, and the free acid goes to the positive pole.

It is not the same when, instead of charging the porous vessel with chromic acid alone, a little sulphuric acid is added. This done, as soon as the chromate of chromium is formed, the sulphuric acid reacts on the salt, producing sulphate of chromium and chromic acid.



The sulphate of chromium is decomposed in its turn, and the oxide of chromium is deposited on the carbon.

In the two cases mentioned, it is observed that the liquor of the positive vessel becomes yellow on account of the filtration of the chromic acid. After one day's contact the chromic acid is reduced in presence of the sulphuric acid and zinc; there is therefore formation of sulphate of chromium in the exterior vessel.

2. The neutral chromates and the bichromates give rise to the same remarks; when they are alone without the addition of sulphuric acid, there is no formation of chromic salts in the interior of the diaphragm. The chromic acid goes to the positive pole, and the liquor becomes alkaline.

When the bichromate of potassa is employed, the latter passes to the same degree of reduction, as chromic acid submitted to the reducing influence; thus we have in this case,—



If we acidify the liquor by means of sulphuric acid, sulphate of chromium is formed in the interior vessel, and much less chromic acid passes towards the zinc pole than in the two preceding cases. When the quantity of sulphuric acid is more than sufficient for forming sulphate of chromium with the equivalent of bichromate, no chromic acid passes towards the positive pole. Indeed, if we take—



we remark that as soon as the chromic acid meets with the nascent hydrogen, it is decomposed and forms oxide; this latter, in presence

of sulphuric acid, gives green sulphate of chrome, with all the oxide of chromium resulting from the reduction :—



The proportionality between the variations of temperature, and of the intensity of the current relative to the decomposition of nitric acid and the nitrates is also true in this case. It must be so with all the acids which can be reduced by hydrogen to a lower degree of oxidation.

Journal de Pharmacie, June, 1857.

(To be continued in our next.)

INDUSTRIAL CHEMISTRY.

On SOME CEMENTS.

By EDMUND DAVY, F.R.S., M.R.I.A., F.C.S.L., &c.

MY attention has been directed to make some simple cements, which may admit of useful and ready application.

Gutta percha, as is well known, is itself an admirable cement for certain purposes, when softened by hot water or by a moderate degree of heat; and it has been used in making other cements;* but I am not aware of any cement described in which it forms a part: its high commercial value is an obstacle to its application in many cases where it could be employed with advantage.

Gutta percha, though readily adapted to an almost endless variety of uses, is, however, not easily rendered fluid when alone, and hence is not quite manageable enough for certain purposes.

I have made many experiments, using different proportions of gutta percha with pitch, rosin, wax, &c., with a view to form useful cements. In the present communication I purpose to notice only one cement, which I made by melting in an iron saucepan.

Two parts by weight of common pitch, and adding to it one part by weight of gutta percha, stirring and mixing them well together until they were completely incorporated with or united with each other, formed a homogeneous fluid, which may be used in this state for many purposes, but which, on account of the facility and tenacity with which it adheres to metals, stones, glass, &c., I found convenient to pour into a large basin of cold water, in a thinner or thicker stream, or as a cake. In this state, while warm, it is quite soft, but may be soon taken up out of the water and drawn out into longer, or pressed into shorter, pieces, or cut or twisted into fragments, which may again be readily re-united by pressure.

When the cement is cold, or before, it may be removed from the water and wiped dry, when it is fit for use.

From a rough experiment I made, there appeared to be a loss of about $\frac{1}{4}$ th of the weight of the materials in making this cement,

* Brande's "Chemistry," vol. ii.

arising from volatile matter and impurities in the pitch and gutta percha.

Properties.—This cement is of a black color; when cold, it is hard. It is not brittle, but has some degree of elasticity, which is increased by a slight increase of heat. It appears to be not so tough as gutta percha, but more elastic. Its tenacity is very considerable, but inferior, if I mistake not, to gutta percha. It softens when put into water at about 100° Fahr.; and if the heat is gradually increased, it passes through intermediate states of softness, becomes viscous like bird-lime, and may be extended into threads of indefinite length: it remains in this state, even when exposed for some time in a crucible, to the heat of boiling water, at 212° Fahr. When heated to about 100° Fahr., it becomes a thin fluid. Water appears to have no other action upon it but that of softening it when warm or hot, and slowly hardening it when cold. The cement adheres strongly, if pressed on metal or other surfaces, though water be present, provided such surfaces be warm.

My first trials with this cement put it to a very severe test. I used it as a substitute for plumbers' solder in repairing the lead gutters on the roof of my house, which were cracked in several places, and admitted water freely in different places, and also to staunch the leaks in an old common and forcing-pump attached for yielding a supply of water for the use of two houses, and raising it about thirty feet. For these purposes I found it quite effectual. All that was necessary in the case of the gutters was to remove with a brush all loose earthy matters from the cracked lead, slightly warm it with a hot iron, then pour the cement in a fluid state on the cracks, so as to cover them on both sides; then a hot iron was drawn along each edge of the cement so as to soften and bevil it down to the lead, as the cement has intermediate degrees of fluidity, and is thicker or thinner as it is exposed to more or less heat. In its thicker state it may, perhaps, be better adapted to repair cracks in lead or other gutters; but a crack in such gutters may be readily filled up by taking a piece of the dry cold cement and applying a warm, but not too* hot a soldering iron, so as to soften the cement on the crack, then melt it on each side and cover it with the cement. The cement will adhere with great ease to the lead, and is far more manageable than any of our common solders. A hole in a gutter could be readily stopped with the cement and a piece of lead of sufficient size to overlap the hole, say about one-half of an inch; cover the lead on both sides with a surface of the cement; press it on the hole; then cover the lead and its edges with the cement, as in puttying a pane of glass.

In the case of the common and forcing-pump, it was only necessary to have every part that leaked quite dry, and slightly warm, when a good coating of the cement, in its thick state, was applied so as completely to cover the cracks or apertures. The cement used in this instance did not exceed in bulk the plumber's solder which would have

* When the soldering iron is too hot, and applied to the cement, it decomposes a portion of it, or raises it in a white vapor. When it is of the proper temperature, which is about 130° Fahr., it is merely softened or partially melted.

been used. The warm soldering iron was lastly applied to fill up any interstices, and produce throughout a uniform surface.

I entertain no apprehension that the warmth of our climate at any time will impair the efficacy of this cement when applied to repair lead, zinc, or iron gutters; for, though it softens at a comparatively low temperature, it still adheres most tenaciously to metals and other substances, and does not allow water to pass through it. My gutters were repaired with the cement before the very hot weather we had last summer, and not the least appearance of a leak has been since observed in the gutters.

In a similar way the cement may be readily applied to repair holes in tin cans, garden watering-pots, iron or other metal vessels which are used only for cold water. Vessels thus repaired should be left a few hours before they are used, as the cement takes some little time to set or harden.

That the presence of water does not interfere with the action of the cement was shown in cases where I put a large hammer, also a seven-pound weight, into hot water, for a few minutes. I then removed them from the water, and, without wiping them, pressed the end of a small stick of the cold cement on the surface of each, when it softened and strongly adhered to the metals. I then poured a stream of cold water on each of the articles, when they could be raised from the table and carried about, being firmly supported by the cement, and considerable force was necessary to separate the articles from the cement.

This cement is applicable to many useful purposes. It adheres with great tenacity to metals, wood, stones, glass, porcelain, ivory, leather, parchment, paper, hair, feathers, silk, woollen, cotton, linen fabrics, &c. It is well adapted for glazing windows, as a cement for aquariums. As far as my experience has yet extended, this cement does not appear to affect water, and will apparently be found applicable for coating metal tanks; to secure the joints of stone tanks; to make a glue for joining wood, which will not be affected by damp; to prevent the depredations of insects on wood. The heavy oak beams and rafters in the roof of the Royal Dublin Society are attacked to a considerable extent by insects, as the weevil, &c. As this cement is soluble to a considerable extent in volatile oils, as turpentine, naphtha, &c., an application of the solution of the cement in turpentine, &c., might be beneficial, and arrest the ravages of those insects. It may be highly deserving of inquiry whether the cement may not be applied to preserve surfaces of metal and wood exposed to the atmosphere and to fresh water; also to protect anchors, chain-cables, &c., from the corroding agencies of sea water.

Journal of the Royal Dublin Society.

NOTE on a PIECE of ANCIENT WOOD from the QUAY of CARTHAGE.

By M. E. PELIGOT.

MARSHALL VAILLANT sent me, some weeks ago, a piece of ancient wood from the Quay of Carthage, taken by Dr. Guyon, Inspecting Physician at present in Algiers. It was accompanied by the following note from Dr. Guyon.

"The quay of Carthage is still well preserved in many places, to the height of fifty or sixty centimetres and more; and wherever it exists in this state are seen, at equal distances, the stakes which entered into its construction, and most of which jut out now from the masonry in which they are embedded, the latter being so much detached.

"The fragment which forms the subject of my note, gives an idea of the portion of the stakes thus deprived of the masonry which enveloped it. It was bathed on all sides by the sea water, and a kick sufficed for detaching it from the stake. The latter was in the finest state of preservation, to judge from its extreme hardness; appreciated from top to bottom, at the surface of its fracture with the detached fragment.

"This fragment bears the marks of a bituminous preparation—a kind of preparation which we find in many stones dispersed among the ruins of Carthage. It was known, moreover, to Pliny, that the Carthaginians, like the Babylonians, endued their stones with a bituminous product; which stones, owing to their nature, without that provision, would not long have resisted the action of external agents.

"The celebrated aqueduct which, from the Zowan, carried water to Carthage, is constructed here with stones, then in clay, according as the locality which it traversed had or had not stone proper for its construction. Now, in constructions in clay, we find pieces of wood in perfect preservation. I possess also specimens likewise taken by me on the spot, but at the moment I cannot lay my hand upon them; and I regret this so much the more, as they should naturally accompany the pieces of the stake of the quay of Carthage. This preservation of wood, in the clay of the aqueduct of Carthage, has already been noticed by Sir Granville Temple, in the account of his interesting voyage to Tunis.

" 'Some of the girders, encased in the layers of clay,' says that traveller, 'are well preserved: the wood of them is still sound; but it would be difficult to say, whether they have formed part of the scaffolding, or whether they have been employed for consolidating the edifice.' "—*Excursion to Tunis*.

At the request of Marshall Vaillant, I have subjected this piece of wood to an attentive examination. M. De Caisne was kind enough to determine the nature of the wood.

At first sight, this wood does not differ from ordinary wood, which it resembles in color and texture; however, its existence, even under the form of the stake, dates as far back as twenty-six centuries: it is

believed, indeed, that Carthage was founded about the year 860 before the Christian era.

Conformably to Dr. Guyon's observation, this piece of wood presents very great solidity in the direction of its fibres; but when the latter are detached, they are easily divided and crumbled by slight pressure between the fingers.

This wood burns with difficulty, and produces no flame; it leaves an enormous quantity of ash. It contains, indeed, from sixty to seventy per cent. of mineral substances.

In contact with dilute hydrochloric acid, it gives rise to a brisk effervescence, for it contains a very large quantity of carbonate of lime. By this treatment, it loses about two thirds of its weight.

The presence of mineral substances was already recognised by the gravity of this wood, which is perceptibly greater than that of ordinary wood. Besides, on examining attentively a piece cut in the direction of the fibres, we found in it white veins, which are formed by calcareous incrustations; these leave, in the ligneous tissue, a hollow furrow when the wood is placed in contact with hydrochloric acid for some hours.

It was interesting to ascertain whether this wood still presented any vestige of the bituminous preparation mentioned by Dr. Guyon. On treating it with ether and alcohol, the evaporated liquor gave only a small quantity of crystallised common salt.

A hot concentrated solution of potassa acted on this wood only very imperfectly; the boiling liquor was scarcely colored brown. A microscopical examination bore testimony to its good preservation as an organised product; the botanical family to which it belongs was easily determined by M. Decaisne, who has sent me, on this subject, the following interesting note.

"I have examined with scrupulous attention the wood from Carthage which you have sent to me; it is a resinous wood, of the family Coniferae, and probably of a species of pine or fir. The absence of vessels, the tenuity of the medullary radii formed of nearly square cellules, the very clear and characteristic punctuation of the ligneous fibres, leave no doubt in this respect. Unless this wood belongs to the pine of Aleppo (*Pinus Aleppenses*), or to the cedar, we are warranted in concluding that the Carthaginians derived a portion of their building woods from other countries, where, doubtless, they had establishments. The mountains of Bætica (Andalusia), those of Corsica, Sardinia, the Pyrenees, the Apennines, &c., may have furnished these woods.

"Moreover, the Carthaginians were not the first to make extensive use of the wood of the Coniferae. The Tyrians, their ancestors, could scarcely construct their vessels with any other material. We know, on one hand, that an immense number of cedars was cut down to enter into the construction of Solomon's Temple. But, even before this, the employment of the wood of the Coniferae (cedars, pines, or firs) was common among the Egyptians, as is testified by the objects sculptured in wood which we see in our Egyptian Museum. I have had

occasion to examine a charming statuette of the beautiful period of Egyptian art, and which I found to be made of another wood than that of the cypress. Various kinds of Egyptian toys, in wood of the same nature, exist in the Museum I have mentioned; which proves that, many centuries before Christ, Nuremberg toys were made as now. We may well say with Solomon—*Nihil sub sole novum!*"

The elementary analysis of this fragment of wood, almost completely freed by means of hydrochloric acid, from the mineral substances which accompany it, shows that its preservation, with regard to its chemical nature, is more apparent than real: this wood, dried at 120° C. (248° F.), presents indeed, the ashes being deducted, the following composition:—

Carbon	.	.	60.0	58.6
Hydrogen	.	.	5.9	5.8
Oxygen	.	.	33.5	35.0
Nitrogen	.	.	0.6	0.6
			100.0	100.0

It is known that dried wood does not contain more than 50 per cent. of carbon. With regard to cellulose, it contains from 42 to 44 per cent.; and hydrogen and oxygen exist in it, in the proportions which constitute water.

The 8 or 10 per cent. extra of carbon, which this wood contains, leads us to infer that, under the prolonged influence of water, it has been partially transformed into that variety of lignites which are designated by the name of *imperfect lignites*; and which contain, according to M. Regnault's analyses, from 57 to 66 per cent. of carbon, and 5.5 to 5.8 of hydrogen.

To sum up, the composition of this piece of ancient wood is represented by the following numbers:—

Water	.	.	.	.	9.5
Carbonate of lime	.	.	.	.	47.2
Carbonate of magnesia	.	.	.	.	2.5
Common salt	.	.	.	.	7.2
Sulphate of lime	.	.	.	.	1.6
Alumina and oxide of iron	.	.	.	.	0.3
Silica	.	.	.	.	0.1
Organic matter (lignite)	.	.	.	.	31.6
					100.0

I have not found in this product the least trace of salts of potassa, which are ordinarily found in the ashes of wood.

The very nature and abundance of the mineral matters which this wood contains, bear witness to its very ancient origin, and to its long sojourn in sea water. It is remarkable to see this water, which contains, as is known, earthy carbonates, dissolved by means of carbonic acid, incrust the fibres of the wood so completely that the latter is almost metamorphosed into a kind of stone. It is especially inte-

resting to be able to follow the very considerable alteration which time and sea water have caused the vegetable matter to undergo, as regards its chemical constitution. This alteration is not the same as that which results from the prolonged stay of woods in certain stagnant fresh waters. According to a recent work by M. Hervé-Mangon, a stake, from a bridge constructed about seventy-five years ago, had been profoundly altered by the water; the ligneous matter was disaggregated; it was enriched with mineral substances, but these were very rich in silica, and in much smaller proportion than that found in the wood from the quay of Carthage; it presented nearly the appearance and composition of turf, or 8 to 10 per cent. of carbon less than in ordinary wood. The alteration had proceeded, consequently, in the opposite direction to that which I have shown above.

Dr. Guyon mentions pieces of wood, in perfect preservation, which are met with in the constructions in clay of the Carthaginians: he would add still more to the interest which his note presents, by subjecting to a comparative examination these fragments of wood, as well as the clay in which they are encrusted. It may be asked, indeed, if these masses of clay are not themselves calcareous matter, formed, or at least profoundly modified, by the encrusting action of the sea water. In a very recent publication, "On the Destruction of Hydraulic Compounds by Sea Water," one of our most illustrious correspondents, M. Vicat, notices, in the following terms, the formation of products of this nature: "The presence of the bicarbonate of lime in sea water may alone account for certain phenomena of incrustation observed on the debris of ancient piers disseminated along the bank of the Pouzsoles. These incrustations, which consist in a thick layer of calcareous loam, could only have been formed by the carbonate of lime deposited in proportion as the surrounding bicarbonates gave up carbonic acid to the lime of the clay."

I may add that, in a time when modern constructions have much to suffer from the destructive action of this water, the establishment of facts connected with the preservation or alteration of materials employed by the ancients cannot fail to present great interest.

Comptes Rendus, No. 19, May 11, 1857.

*On the APPLICATION of HEAT to DOMESTIC PURPOSES, and to
MILITARY COOKERY.**

By CAPTAIN JOHN GRANT, late R.A.

THE science of heat is more studied, and probably better understood in England than in any other country; but there is a deficiency in the practical application of heat to domestic and other useful purposes, which deprives all classes of much comfort and benefit. For economy

* The discourse was illustrated by models and diagrams of the cooking apparatus invented and described by Capt. Grant.

in the consumption of fuel is a matter of great importance to the mass.

Domestic Fire Places.—The universal practice of fixing grates, and surrounding them with masonry, is defective in principle; for the masonry not only absorbs a large portion of the heat which is required to warm the room, but throws a still larger portion up the chimney.

The heat which is thrown out from grates set in this manner does not depend so much upon the quantity of fuel they contain, as upon the amount of radiating and reflecting surfaces with which that fuel is surrounded. The highly polished steel surfaces which adorn the grates of those who can afford them, reflect and radiate a very large amount of heat in proportion to the fuel they contain; but as the mass of the people can ill afford this costly mode of obtaining heat, the object should be to secure the maximum of heat with the minimum of cost.

The detached or portable grate, similar to the Brussels stove, will secure these advantages; and if properly constructed, combines all that can be desired for a domestic fireplace. It presents an open cheerful fire, easy to regulate,—affords a large radiating surface,—facilitates the operation of sweeping the chimney,—and is the best security against a smoky one; consequently dispenses with those countless varieties of infallible curatives, called chimney tops, which so disfigure all our houses. It acts also as a hot-air stove.

Domestic Cooking Apparatus.—The objection to casing our domestic grates with masonry applies still stronger to cooking ranges, for the wasteful expenditure of fuel is most apparent in all our cooking arrangements. The remedy here is equally simple, by the substitution of a detached cooking apparatus placed in the same recess which receives the ordinary fixed kitchen range; and a portable kitchen stove of this construction may be seen in daily operation at that interesting establishment, the North-West Reformatory Institution, in the New Road, where they are manufactured by the inmates. Eighty-two persons are daily cooked for at a cost of sixpence per day.

It may not be out of place to suggest to those who take an interest in the reformatory movement, a visit to that establishment, and see what has been accomplished through the exertions of one man (Mr. Bowyer), one of the most useful self-sacrificing philanthropists of the day.

The Cottager's Stove.—There is a large and important class of persons among the industrious and working classes, and also the indigent poor, whose domestic wants demand our consideration. For the purpose of at the same time warming their dwellings and cooking their food in a simple and economical manner, "*The Cottager's Stove*" was designed, which has found its way to every quarter of the globe. It requires no fixing, is extremely simple in its construction, and all the operations of cooking may be carried on with any description of fuel. The fact of 100 lbs. of meat and 115 lbs. of vegetables having been cooked in one of these stoves, with less than 20 lbs. of coal, will

suffice to prove how economically it may be worked, and it is peculiarly well suited to the rural districts. These stoves are manufactured by the Messrs. Bailey, of High Holborn.

Military Cookery.—The events of the late war have given rise to a variety of inventions and improvements, and amongst them that of cooking for large masses of troops, with convenience and economy, has been under consideration.

Field Cooking.—A system of field cooking was introduced at Aldershot, by cutting a trench in the ground and covering it with thin iron plates having a central hole in each to receive the ordinary camp kettle. A chimney is formed of sods, piled up to the height of three feet at one end of the trench, and a fire made at the other, and by this simple arrangement several regiments cooked for some months. -

Battalion Cooking Apparatus for Troops, adopted at Aldershot.—This principle of cooking was adapted to the requirements of the troops in the hut barracks at Aldershot, and extended to the battalion cooking kitchens throughout the whole of that encampment, where it has continued in successful operation for the service of between 12,000 and 14,000 men, for these last twenty months. From April to August, in the last year, it was subjected to the severe test of cooking for 92,000 men, who marched in and out of the encampment during that period. The consumption of fuel requisite for this system of cooking is one half-pound of coal per man per day, and the official report states the cost to be one half-penny per man per week for the three daily meals.

Battalion Cooking Apparatus for Permanent Barracks, with Oven.—The soldier's cooking is confined to the boiling process; and as he can only obtain a change by availing himself of a public bake-house at his own cost, an oven has been introduced into the chimney of the apparatus, so constructed that it is surrounded by the heated products of the two flues: thus profiting by what in ordinary cases is wasted up the chimney. 560° of heat were obtained by this arrangement in an oven capable of baking 250 lbs. of meat, without any additional consumption of fuel. *

By the introduction of this oven, other improvements are effected: the flues are shortened, the fires brought nearer together, and the whole of the kettles boil simultaneously. The advantages of this system of cooking could only have been obtained by the application of the principle of horizontal draft; and the same principle may be

* This principle of military cooking admits of so much variation in its details that it may be adapted to all the requirements of an army, either in the field or in permanent barracks, from a single soldier to a battalion of a thousand strong, for which provision is made.

The battalion cooking apparatus is constructed for that number of men, who may be efficiently cooked for by the aid of two small fires of 18 inches square and 6 inches deep, with any description of fuel, if the principle is properly carried out.

A school for the instruction of cooking might be added with advantage to the many excellent ones already established for the improvement and comfort of the British soldier.

carried out with equal advantage for warming the dwellings of the working classes. The subject is worthy the consideration of those interested in the philanthropic undertaking of erecting blocks of dwellings for our houseless*poor. This principle will also be found applicable in many cases, where the perpendicular shaft is adopted in our large factories, pouring out its column of fire and wasted heat.

An apparatus, upon the principle of cooking for troops in permanent barracks, has lately been erected at the North-West Reformatory Institution, in the New Road, for the use of its inmates, which combines the means of cooking, baking, washing, drying the linen, and supplying hot water for baths. It might be extended with advantage to our prisons, unions, and all large establishments requiring such conveniences.

Public bakehouses might also avail themselves of this principle. By the aid of two small fires, heating the oven from without, and lining the interior with brick or tile, the baking might be carried on with some advantages over the present system.

Read before the Royal Institution.

On the Use of SULPHATE of LEAD, as a SUBSTITUTE for CERUSE, in the MANUFACTURE of LACE;—Use of the same SALT, to RENDER FABRICS UNINFLAMMABLE;—Use of a NEW CHEMICAL AGENT, for the same purpose. By M. H. MASSON.

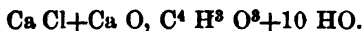
THE manufacturers of lace make use of ceruse, either to give soiled lace the appearance of new, or to cause the disappearance of finger marks, and thus concealing the traces of the designs (especially in the kind of lace called *Brussels appliqué*). In this last operation especially, the workwomen, when they have finished one application, sprinkle their work with carbonate of lead, which they must necessarily breathe. This practice is repeated every few minutes, and their health soon suffers. This is so well known that the manufacturers find a difficulty in obtaining workwomen, notwithstanding the high remuneration given to this kind of work. It is evident that the use of carbonate of lead must be given up, but for this purpose it is necessary to find a substitute for it. Now, *sulphate of lead* I have found to fill all the conditions admirably. Its action on the animal economy is extremely slight. One great proof of this is the use of *sulphates* of potassa, soda, or magnesia, as an antidote in cases of poisoning with salts of lead; finally, to prevent saturnine illnesses, the workmen who make white lead are advised to wash their hands and rinse out their mouths with water slightly acidulated with sulphuric acid.

Wöhler mentioned some years ago that sulphate of lead is soluble in a solution of neutral tartrate of ammonia; and, indeed, experiment has proved that neutral tartrate of ammonia will dissolve a very large quantity of sulphate of lead, if the temperature of the liquid is 100° C. (212° F.) A fabric steeped in this hot solution of sulphate of lead becomes difficultly inflammable if we expose it for some time to a very

high temperature; the organic matter burns completely, diffusing a sharp odor, and leaving only a small quantity of ash. This result not satisfying me thoroughly, I thought that chloride of calcium—the excellent effects of which in extinguishing fires have been already proved—would give me a means of preserving fabrics and wood from fire. The property which it possesses of being deliquescent in the air having prevented its use as a preservative substance, I have endeavored to remove its hygroscopic faculty.

The method of obtaining this result was already traced. Observation has proved that the double salts are generally less soluble than the most soluble of their constituents; they are sometimes even less soluble than their least soluble constituent. For this reason, when we mix concentrated solutions of two salts which will combine, the result is almost always a crystalline precipitate of a double salt. I discovered the principle; it still remained for me to seek its application to chloride of calcium. After several fruitless endeavors, I obtained a satisfactory result.

If we dissolve equal parts by weight of acetate of lime and chloride of calcium, and let the solution evaporate slowly, the two salts combine and form a hydrated compound, which crystallises in beautiful crystals. These crystals contain ten equivalents of water, and may be represented by the following formula:



Neither dry nor humid air causes any change in them. If we heat these crystals to more than 100° C. (212° F.), they lose their water of crystallisation; in the anhydrous state, they do not alter in the air, and are in no degree hygroscopic.

Having obtained this compound, I considered the problem solved; but in practice I was met by an unexpected difficulty. The water which I naturally, at first, tried as a solvent, partly decomposed the salt formed. Alcohol, which I tried afterwards, and which is moreover a very dear solvent, did not succeed any better.

My attention was then attracted to ammonia, and I obtained a perfect result. Ammonia perfectly dissolves the crystals of $\text{Ca Cl} + \text{Ca O}, \text{C}^4 \text{H}^8 \text{O}^8 + 10 \text{HO}$, at the boiling temperature.

To render a fabric *incombustible*, it is sufficient to steep it in the liquor and dry it; not only will it perfectly resist the action of burning bodies, but it has the advantage of not being in the least hygroscopic.

Although the best solvent is ammonia, the most economical is water. I could not give it up at the first difficulty; and I have found that, notwithstanding the partial decomposition which it produces, we can have recourse to it; but great care is necessary in using it.

Comptes Rendus.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.***On a LITTLE KNOWN FUNCTION of the PANCREAS, the DIGESTION of NITROGENOUS MATTERS.* By M. L. CORVISAET.**

PURKINJC and Pappenheim affirmed (1839) that they had extracted from the pancreas a liquid endowed, like the gastric juice, with the property of dissolving nitrogenous aliments; hitherto it had not been demonstrated, that a true digestive transformation was effected.

The pancreatic juice in digesting the albuminoid aliments, operates on them a transformation identical with, or analogous to, that which the stomach produces. But the liquid of the pancreas acts only on that part of the food which has escaped the gastric digestion. The portion of the food transformed by the juice of the stomach, is a *definitive product*, on which the pancreas has no action.

When the two digestive liquids are separated, they exercise their function to the utmost, and thus double the product of the digestion. If they are found in the pure state, the two digestions are no longer exerted; so far from the digested product being doubled, it is reduced to nothing. The two ferments (pepsine, pancreatine) are mutually destroyed. In the normal state, nature prevents this conflict by three means. 1. The pylorus, which separates the two ferments. 2. The gastric digestion itself, during which the pepsine is destroyed. 3. The bile, which annihilates in the passage the activity of the latter. It is this conflict, doubtless, which has prevented me when I tried pancreatine against disturbances of the second digestion, as I administered pepsine against those of the first. During a digestive period, nearly as much pepsine as pancreatine is formed; if the gastric juice appears more abundant, it is because it is more aqueous; if the pancreatic juice appears stronger, it is because it is less dilute. The bile does not precipitate the product which has been digested by the stomach, so that its digestion be destroyed or repeated; on the contrary, it is the acid of this last which precipitates the bile itself. Indeed, the precipitate is not formed, or is destroyed in an alkalised medium, indeed, by the bile itself.

Comptes Rendus, No. 14, April 6, 1857.

On the PHYSIOLOGICAL MECHANISM of the FORMATION of SUGAR in the LIVER.

By M. CLAUDE BERNARD.

THE physiological formation of sugar in animals should be considered as I have already said, not as a phenomenon of direct chemical separation of the sanguineous elements at the moment of the passage of the blood through the liver, but as a function performed by the succession and combination of two essentially distinct acts.

The first action is purely vital, for it cannot take place without the influence of life, it consists in the creation of the glucogenic matter in the living hepatic tissue.

The second action is chemical, and may take place without the vital influence, it consists in the transformation of the glucogenic matter into sugar with the aid of a ferment.

The reunion of these two orders of conditions is necessary for the appearance of sugar in the liver. The glucogenic matter must be created by the vital activity of the organ; it is then necessary that this matter should be brought in contact with the ferment which is to convert it into sugar.

The glucogenic matter is formed like all the products of organic creation, by means of the phenomena of slow circulation which accompany the acts of nutrition. As for deciding if, amid the numerous blood vessels with which the liver is provided there be any which are peculiarly charged with this nutritive circulation and others peculiarly connected with the phenomena of the chemical transformation of the glucogenic matter, that is a physiological question upon which it is at this moment unnecessary to enter. It will suffice for us to indicate in a general manner how the contact between the glucogenic matter and its ferment may take place in the living animal.

I at first thought that the ferment was peculiar to the liver, like the glucogenic matter itself; I had even succeeded in obtaining it in an isolated state. But, seeing afterwards that the sanguineous fluid possesses the property of transforming this glucogenic matter into sugar with the greatest readiness, it became impossible to think of a localisation of the ferment; that extracted from the liver coming, very probably, from the blood itself. So that if out of the organism we have several ferments which operate the transformation of the glucogenic matter into sugar, in the living animal it is only necessary to admit one represented by the blood, which moreover possesses the property of rapidly changing hydrated vegetable starch into dextrine and sugar. Without entering into the intimate mechanism of this contact and into the explanation of the physiological causes which vary its intensity, which would lead us into descriptions of microscopic anatomy and phenomena of capillary circulation which will be developed elsewhere; we shall confine ourselves to saying that the observation of physiological phenomena teach us that in the liver, parallel with this slow and nutritive circulation, there must be considered another, intermittent, variable, and whose superactivity coincides with the appearance of a larger quantity of sugar in the tissue of the organ.

In digesting animals the circulation in the *vena porta* is super-excited, and then the transformation of the glucogenic substance is much more active, although the formation of this matter does not appear to correspond with it at that moment. This circulatory super-activity may likewise be awakened without digestion; and then the same phenomena of transformation of the matter and appearance of the sugar take place. Among hibernating or benumbed animals, such as frogs for instance, the slackening of the circulation, which is connected with a lowering of the temperature, causes a diminution, and sometimes the almost entire disappearance of the sugar from the

liver. But the glucogenic matter is there all the same, as may be proved by extracting it. It is then only necessary to put the benumbed frogs in the warmth to render their circulation active, and the sugar then soon appears in the liver. On placing the animals again in a low temperature we find that the sugar diminishes, or disappears to shew itself again when the frogs are again placed in a warmer place. I must add that these singular alternations of the appearance and disappearance of the sugar may be reproduced several times without the animals having any food, and by acting solely on the phenomena of the circulation by the intermediation of the temperature.

In warm blooded animals we can also act, by means of the nervous system, on the phenomena of the abdominal circulation, and afterwards secondarily on the transformation of the glucogenic matter in the liver. I have shown that if we cut or wound the spinal marrow in the region of the neck, below the origin of the phrenic nerves, we considerably diminish the activity of the hepatic circulation, so much so that after four or five hours there are no traces of sugar in the liver of the animal, the tissue of which still remains charged with glucogenic matter. It is remarkable that after this operation the temperature of the abdominal organs falls considerably, at the same time many other perturbations are produced of which I cannot now stop to speak.

I have likewise proved that by wounding the cerebro-spinal axis in the region of the fourth ventricle, we produce exactly contrary phenomena; the abdominal circulation is very much accelerated, and consequently the renewal of the contact of the glucogenic matter with its ferment considerably extended. The transformation of the glucogenic matter moreover becomes so active, and the quantity of sugar removed by the blood becomes so considerable, that the animal, as is well known, becomes diabetic in this case, that is to say that the excess of sugar poured into the blood by the super-excited liver passes into the urine.

In these two cases the nervous system acts evidently on the purely chemical manifestation of a physiological phenomenon. But when we analyse its mode of action we find that its effects are only mechanical, and are brought first to the motor organs of the capillary circulation, the effect of which has been to lessen or hinder, or else to extend and augment the contact of two substances capable by their properties, of reacting one on the other; they thus produce a chemical phenomenon which the nervous system regulates indirectly, but upon which it has no direct and primitive action. This view is not peculiar to the liver, and I shall prove further on that the chemical influences which are recognised in the nervous system in general are most commonly purely mechanical.

As for the conclusions which we can at this moment deduce, in a general physiological point of view, from the mechanism which we have indicated for the formation of sugar in the liver, it is impossible not to be struck with the similarity which exists in this respect between the glucogenic function of the liver and the production of the

sugar in certain acts of the vegetable organism. In a seed, for instance, which produces sugar during germination, we have likewise to consider two series of very distinct phenomena; the first primitive, entirely vital, is constituted by the formation of starch under the influence of the life of the vegetable; the other consecutive, entirely chemical, which may take place without vegetable life, is the transformation of the starch into dextrine and sugar by the action of diastase. When a liver taken from a living animal continues for a certain time to produce sugar, it is evident that the vital phenomenon of the creation or secretion of the glucogenic matter has ceased; but the chemical phenomenon continues to be produced if the conditions of humidity and heat necessary for its accomplishment are realised. In the same way, in the seed separated from the plant, the vital phenomenon of the secretion of starch has ceased with the vegetable life; but, under the influence of favorable physico-chemical conditions, its transformation into dextrine and sugar by means of diastase may be produced. Finally, it is easily seen from these parallel observations, that the formation of sugar in the liver of animals passes through three series of successive transformations in every respect analogous to that of the formation of starch, dextrine and sugar in the seed of vegetables.

According to all the facts contained in this work, we may conclude that the question of the formation of sugar in animals has made an important step in advance, in consequence of the isolation of the glucogenic matter which always exists previously to the sugar in the tissue of the liver.

We still have to determine the organic form of this glucogenic matter, as well as the exact anatomical and physiological conditions of its formation in relation to the phenomena of development and the various physiological state of the liver. Some experiments which I have already commenced on this subject lead me to hope that it may be possible to go still further into the glucogenic question, and to localise the formation of the glucogenic matter in the peculiar elements of the hepatic organ.

Comptes Rendus.

PHOTOGRAPHY.

On the APPLICATION of LIGHT and ELECTRICITY to the PRODUCTION of ENGRAVINGS—PHOTO GALVANOGRAPHY.

By THOMAS A. MALONE, F.C.S.,

Director of the Laboratory in the London Institution.

THE subject of this discourse is one with which the speaker has been for some years practically acquainted. In 1844 he experimented for many months upon the engraving process of M. Fizeau of Paris, in conjunction with M. Claudet and M. Fizeau. Since that time he has closely watched all the steps of improvement that have been taken, down to the latest investigations of Talbot, Niepce de St. Victor,

Pretsch, and Poitevin. He ventured thus to think himself fairly entitled to lay before the auditory the numerous remarkable and beautiful specimens he had gathered, or kindly been furnished with, accompanied by such commentaries and notices of processes as the time admitted.

The various methods hitherto devised for the accomplishment of that important problem, the certain perpetuation and cheap multiplication—by means of printer's ink and the ordinary printing presses—of the images of natural objects, as obtained in the camera obscura by the processes of ordinary photography, may be arranged under three great divisions.

The *first* method, in which light was used to aid the engraver's art, was almost coëval with the first attempts made to produce sun-drawn pictures. Indeed it has been asserted that photography and photographic engravings were invented between the years 1813 and 1827, by one man, Nicéphore Niepce, of Chalon on the Saône. A reference, however, to the Journal of the Royal Institution* would show that photography really sprang from the labors of Thomas Wedgwood and Humphry Davy, as far back as the year 1802.

Although we cannot accord to Nicéphore Niepce the merit of originating photography, we must give him the undivided title of founder of the art of photographic engraving, and, moreover, acknowledge that he was the first to fix not only a *direct positive* photograph, but also to secure on metal and glass plates the images of the camera, and this long before Daguerre produced his wonderful plates. Of this there can remain no doubt, after a study of the remarkable specimens which Dr. Robert Brown has so kindly enabled photographers now for the first time publicly to examine. It was not generally known that Niepce's images of 1827 had so much that is beautiful, in common with the daguerreotype of a later date. Daguerre's pictures may be said to be only exalted examples of the same phenomenon; yet the processes are widely different. Niepce's method was beautifully simple, and as it gives us the ground-work of his etching process, must be briefly described. He took a bituminous substance called jew's pitch or asphaltum; upon this he poured oil of lavender to resolve the bitumen into a varnish with which he could coat plates of metal or glass. He used chiefly pewter and copper plated with silver. A plate coated and dried was exposed to the light with an engraving superimposed, or it was placed in the field of the camera obscura just as Wedgwood and Davy placed their prepared papers; and with a certain similarity of result, inasmuch as a photographic image was obtained on the varnished plate. This image, however, unlike that of Wedgwood and Davy, was *not visible*. The plate had to be submitted to the solvent action of a mixed liquid, composed of one part of oil of lavender, and ten parts by measure of *white oil of petroleum*, or mineral naphtha. On immersion in this fluid the remarkable fact revealed itself, that wherever the light had acted the

* Journal of the Royal Institution, vol. I. p. 170.

varnish had become insoluble, and in a certain degree proportionately so to the intensity of the light. There were not only lights and shadows, but half tints. The picture, as soon as developed by the solvent, was removed, drained, and washed with water to check all further action. The shadows of the picture were now represented by the parts of the white metal, or glass plate laid bare; the lights were given by the film of varnish which the light had hardened, and the solvent had left untouched. The plate now finished was capable of being etched by simply pouring engraver's acid upon its surface. The varnish would protect the metal from the acid over the lights of the picture; while the shadows, represented by the bare metal, would be bitten in the manner common to all etching processes. On now removing the protecting varnish, the plate could be inked and printed from by the common copper-plate printing press. Such are the essential details of the first of the photographic engraving processes. The specimens on the table were presented in 1827 to Mr. Bauer, late of Kew, by Nicéphore Niepce, who for a short time resided at Kew, on a visit to a brother in infirm health. Niepce prepared a statement regarding his invention, for presentation to the Royal Society; but as he at that time kept his process secret, his manuscript was not published. Niepce appears to have returned to France, disappointed at his ill-fortune.

Niepce's bitumen process was improved by his nephew, M. Niepce de St. Victor, who has published a treatise* on it, giving the necessary minute instructions. The main features do not differ from those above given, though greater sensitiveness and perfection have been obtained. MM. Mante, Belloc, and Nègre, MM. Barreswil Davanne, Lerebours, and Lemer cier have also advanced the bitumen process; the latter gentleman having applied it to lithographic purposes. The process is still under trial; but the difficulties of obtaining a constantly uniform result at present stand in the way of its general adoption. It still deserves a thorough investigation.

The *second* method of producing photographic engravings is founded upon certain properties possessed by the Daguerrean image. It is found that a daguerreotype unfixed by gold is acted upon by nitric acid in its shadows, while the lights long resist the biting action of the acid. This is explained by assuming that the shadows are of pure silver, and that the lights consist of mercury—the acid attacking the silver by preference. The fact is, that an etching is obtained by merely leaving diluted nitric acid in contact with the plate. The etched plate is then inked and printed from as in Niepce's case. Dr. Donné, of Paris, appears to have been the first to devise this method. Dr. Berres, of Vienna, also used nitric acid for this purpose; but the action is not easily controlled, and this form of the process has fallen into disuse. In 1842, Mr. Grove published in the Philosophical Magazine a method by which Daguerre's images can be engraved by

* *Traité Partique de Gravure Héliographique*, par M. Niepce de Saint Victor. Paris, Juin 1856.

the chlorine evolved by voltaic action, when the daguerreotype plate is made the positive terminal of the battery, and immersed in diluted hydrochloric acid; the negative wire being terminated by a plate of platinum, which was placed opposite and parallel to the photographic image. This process is much more under control than the last. [Prints from plates so engraved in 1842 were on the table.] This process is also worthy of further investigation. Here the image is truly drawn by light, and *engraved by electricity*.

M. Fizeau, about the year 1844, also patented in this country, in conjunction with M. Claudet, a process for engraving the daguerreotype image. The speaker was instructed in this process by M. Fizeau, and worked for many months at its perfection. Results obtained both in France and England were upon the table, and showed that in cases where great delicacy of delineation was required, as in certain anatomical subjects, this process had not been surpassed. It quite justified the formation of a second division of the available photographic engraving processes.

M. Fizeau, like Mr. Grove, availed himself of the affinity of chlorine for silver, but relied on chemical action for its application. He (M. Fizeau) made a solution of common salt and nitrate of potash in water, to which he added nitric acid. This mixed acid acted immediately when aided by warmth, upon the silver of Daguerre's plate, and left untouched the parts supposed to be completely covered by mercury. Chloride of silver was thus at once formed in the shadows of the images, and after some time in the half tints also. A very faint etching was thus produced. A prolonged application of the acid would not further deepen the etching, since the insoluble chloride of silver at first formed protected the faintly etched parts from a further deepening corrosion. It was therefore necessary to remove the chloride of silver by washing with a solution of ammonia. This effected, the plate was ready for a second application of the acid, when chloride of silver would be again formed, to be once more removed by ammonia; and this alternation of solutions could be repeated a certain number of times, the etching increasing in depth at each operation. But in practice it was found that after a few applications of the acid the lights of the image also gave way, and thus the engraving came to an untimely end. To remedy this circumstance was M. Fizeau's great aim; and he succeeded in a marked degree by heating the etched plate in a strong and boiling solution of caustic potash, after which treatment the lights resisted well the injurious action they had before suffered from. It is not clear how the potash acts. M. Fizeau has supposed, and the speaker was inclined to support the view, that the potash acts merely as a hot bath, possessing a proper and a regular temperature which might restore the continuity of the amalgamated surface of mercury and silver as often as it was weakened to the point of breaking by the *under-biting* of the acid liquid. The heating in potash is an important feature in M. Fizeau's process. As soon as the etching has been carried as far as possible by the acid mixture, the plate is dried and inked with fine printer's ink,

and an impression may be immediately taken; but M. Fizeau prefers that the ink should be allowed to dry in the hollows of the plate, the unetched parts being wiped clean, so that gold may be deposited only upon the bright parts by the electrotype process. On now removing the ink, ordinary diluted nitric acid may be safely applied to the plate, to deepen still more the shadows without any danger of destroying the lights of the picture. This last step causes M. Fizeau's etchings to possess greater vigor than those obtained by Donné's or Grove's processes. The danger is, that under-biting may remove the half tints. However, some beautiful results obtained by the late Mr. Hurliman, a skilful engraver of Paris, attest the worth of this method. M. Fizeau, foreseeing that the wear and tear of the silver plates might be considerable, thought to use the electrotype process to produce fac-similes of the engraved plates, reserving the original plate unworn to supply any further demands, thus allowing any number of impressions to be struck off. Plates so electrotyped by the speaker two years ago, some of which were afterwards worked upon by an engraver, were placed upon the table. The patent right in this process will soon expire.

The processes of the *third* and last division were, it must be confessed, very desirable, notwithstanding the numerous satisfactory specimens obtained, and still to be obtained, by the processes previously described. The truth seemed to be that none of the processes gave uniformly satisfactory results; hence the necessity of being acquainted with the capabilities of all the chief known methods, and of impartially comparing them with a view to produce any special required result.

Mr. Henry Fox Talbot opens the third division by his method, known as the gelatine and bichromate of potash process, in which a steel plate is covered with a liquified jelly, containing bichromate of potash in solution. This jelly was allowed to dry upon the plate after the manner of Niepce's varnish; and the gelatine plate might be used in a similar way to reproduce engravings or the images of the camera; the light, as in Niepce's case, doing its work by altering and hardening the gelatine whenever it fell with sufficient intensity. On removing the plate from the light, and immersing it in water, it was found that the gelatine had become comparatively insoluble where the light had acted, but it retained its usual solubility over those parts which were in shadow. Thus the metal could be partially laid bare, as we have seen was the case in the bitumen process; the lights now would consist of the altered gelatine, and the shades be represented by the bare metal; it is evident we have only to pour an acid upon the plate to obtain an etching; but here some care and ingenuity will be required. Nitric acid acts so energetically and so uncertainly on the steel plate, that but little success would attend its employment. Accordingly, Mr. Talbot was led to seek a better engraving liquid. This was found in a solution of bichloride of platinum, which appeared to act in the desired manner. The advantage of any process on steel plates would be obvious, from the great number of impressions, that

so hard a body would yield under the wearing action of the printing press. It might here be observed that the bitumen process had also been applied to steel plates by M. Mante, in a series of natural history plates, published in Paris, and also by M. Niepce de Saint Victor, in the frontispiece in his treatise.

In 1854, Herr Paul Pretsch, of the Imperial printing office of Vienna, patented in this country, and subsequently in France, a process which he has called *Photogalvanography*. He uses Mr. Talbot's materials, but with certain additions, and avails himself of a property of the gelatine which allows of his dispensing with the acid etching altogether. We are unable to speak with certainty of the exact comparative methods and capabilities of the two processes. Mr. Talbot's results are on steel; Herr Pretsch's on copper. If other things be equal, the steel would possess the advantage of greater durability. Herr Pretsch takes one part of clear gelatine or glue, and about ten parts of water to form a jelly, which he mixes with a strong solution of bichromate of potash; to this mixture he adds a fresh portion of jelly, containing nitrate of silver in solution; the whole being warmed and thoroughly mixed for about ten minutes. He next adds a third portion of jelly, containing a comparatively small quantity of iodide of potassium; then the whole mixture is strained, and is ready to coat the glass plates which are at first used in this process. A plate being coated and dried, is applicable to all the purposes enumerated in the early bitumen process. It can be used to copy engravings by super position, or be made to receive the images of the camera. However, it is found that the most practical way to make use of the bitumen and gelatine processes, is to copy from a positive photograph which has resulted from a collodion or a talbotype negative. We have only to place the positive print upon the dried orange-colored jelly, press it in contact by a plate of glass, and expose the whole to the light for some time, when we shall find upon removal that we have obtained upon the dried jelly a photographic representation of the positive print. Wherever the light has acted strongly the plate will have changed from its bright orange-red color to a more tawny hue, this latter shade of color gradually passing in the half tints into the unaltered red of the parts completely shielded from the light. The parts acted upon by the light have now become, as in Mr. Talbot's case, comparatively insoluble in water. So far, Herr Pretsch's process has much in common with Mr. Talbot's, but the two experimenters now *diverge* widely. Herr Pretsch, instead of dissolving away the unaltered jelly, merely soaks the plate in water long enough to cause the unaltered gelatine to swell, and so to rise above the surface in such a way that we obtain a picture in relief resembling the condition of an ordinary cut wood-block. The tawny colored parts do not swell, and so they remain depressed, representing the sunken portions of the wood-block. If the swelled gelatine were hard enough, we might at once ink the raised parts by a roller, and print in the usual way. This, however, is impracticable; and, moreover, surface printing is not in this art deemed to be the best mode of

procedure. A device, analogous to one used in type printing, is therefore adopted; a sort of stereotype process is gone through. A mould in softened gutta-percha, or other suitable moulding material, —possibly a composition of wax or stearine, is made; this mould will of course have the raised lines or dots of the original gelatine, represented by grooves and cavities, apparently graven in the surface; and here, again, if the mould were firm enough, we might ink it as if it were an engraved copper-plate, and print by the copper-plate printing press. From these considerations, it will be evident that we have only to seek to convert these yielding surfaces into enduring ones, and we shall end our labors successfully. This the electrotype art enables us to do. We have simply to render the mould a conductor of electricity, by black lead, or finely divided metal, and we can deposit in it copper to any amount. We shall thus get in copper a fac-simile of the original swelled gelatine plate. But since this requires surface printing, and that is not to be preferred, we must once more apply our electrotyping process, using this first obtained and raised copper-plate as a *matrix*, to produce as many engraved or sunken plates, ready to be printed from, as we may desire. The original matrix remains, as in Fizeau's case, unworn.

The above is an outline of the more important features of Herr Pretsch's invention. There is one more point that deserves attention. In all the engraving processes hitherto described, there is a difficulty in obtaining a granular surface over the etched parts necessary to hold the amount of ink required by the printer. In Pretsch's process this difficulty, remarkably enough, does not present itself; the swelled surface breaks up in a direction vertical to its surface into little masses which are just what is desired; this result is quite characteristic. It has been attributed to the presence of particles of chromate of silver, or of iodide of silver. Would it be too far-fetched to suppose that it is another beautiful instance of the slaty cleavage action demonstrated by Dr. Tyndall? However this may be, the fact is very important for the success of the invention. The chemistry of the processes of the first and third divisions of this subject is but little advanced. M. Niepce de St. Victor has found, what M. Chevreul had anticipated, that the oxygen of the atmosphere is essential in the bitumen process. In an illuminated vacuum the result could not be obtained, although ordinary photographic action went on quite as well as in air. With reference to the gelatine processes, it might be observed that Mr. Ponton, who first used bichromate of potash as a photographic agent, and M. Edmond Becquerel, who extended its use on paper, both found that the sizing materials became more insoluble by the photographic action. It was believed that chromic acid was liberated by the sun's rays, since simple mono-chromate of potash produced no effect. On mentioning these facts to a friend (Dr. Hugo Müller), the speaker learnt that solutions of chromium had been employed in Germany, in experiments on tanning skins; and it therefore suggested itself that the chromic acid set free might, in re-acting on part of the gelatine, liberate an oxide of chromium, which, when

combined with the rest of the gelatine, would form a species of artificial leather; thus rationally accounting for the comparative insolubility of the altered and tawny colored portions of the jelly. The subject, however, requires and deserves a more thorough investigation.

M. Poitevin, of Paris, has applied the gelatine and bichromate of potash process to lithographic stone, and his results, placed on the table, would well bear a comparison with those obtained by the other methods described in this division.

The speaker, in conclusion, expressed his opinion that these engraving processes would greatly advance the art of photography itself, particularly in its applications to the delineation of colored objects, in which it is still very imperfect, although some progress has been made.

Read before the Royal Institution.

On the DRY COLLODION PROCESSES. By Mr. G. R. BERRY.

In the investigation of the dry collodion processes I purpose to glance, first at the ordinary wet process, its manipulations and the chemical changes involved, and then to detail the different methods by which various experimenters have endeavored to produce a dry collodion process, and at the outset I would mention that the more the whole subject of photography is investigated, the more palpable is the evidence that the ordinary laws of chemistry are capable of explaining all the phenomena observed in the different photographic processes, albeit our new science has revealed to us subtle chemical changes which our ordinary tests have failed to appreciate, illustrations of which will arise in the course of our subject this evening.

We will take first the ordinary collodion process, which is thus divided:—

1. Preparation of the collodion.
2. Preparation of the nitrate of silver bath.
3. Preparation of the developing solution, positive and negative.
4. Removal of the surplus chemicals.
5. Varnishing.

Collodion is, as all are aware, a solution of one of the pyroxyline compounds, in a mixture of alcohol and ether, and holding also in solution an iodide, or a mixture of iodide and bromide of potassium, ammonium, cadmium, or other bases. The number of these is not very extensive, as the case will admit only those soluble in the menstruum before-mentioned, but if the ingredients are few in number, the variations in quantity and relative adjustment are almost infinite, as every photographer has his series of pet fancies, and to this fact nearly all the disheartening failures of amateurs are attributable, and almost every instance of success has been attained by those who, adopting one formula, have adjusted their silver bath and developer in accordance; and, therefore, when the impressions fail to be successful, have

only these three items to examine or replace. The silver bath varies in its strength from 20 to 60 grains per oz. of water, and it is on the proper reaction between the collodionised plate and the nitrate of silver in solution all success depends. The most approved formula for the silver bath for the negative and dry processes, I believe to be the following:—

Nitrate of silver, that has been fused and probably containing a portion of nitrite	1 oz.
Dissolve in 4 oz. water, to which must be added iodide of potassium	20 grs.

This mixture must be well agitated at intervals for one hour. By this means the strong solution of nitrate of silver dissolves a portion of the precipitated iodide, and lets fall another portion on the addition of water to make up 14 fluid oz. The solution is then filtered, and is ready for use. The collodion plates used will present an even primrose-colored surface, without *striae* or other markings, and the developed impressions will be clear and free from any irregularity of coating. The collodion process negative has, in itself, many advantages. Its great advantage is—exquisite sensibility, inasmuch as under favorable circumstances instantaneous impressions may be obtained; at the same time the delicacy of definition is all that can be desired, and if not equal to albumen, it is the fault in the preparation of the pyroxyline from which the collodion is made. It is facile in manipulation and speedy in its perfected results. Its disadvantage is the necessity of obtaining the impressions in the camera in the first few minutes from the excitation of the plate; it is therefore impossible to work far away from the dark closet or the tent in which the plates are prepared and developed. It is this that has prompted experimenters to devise some plan by which the sensitiveness of the plate might be indefinitely prolonged, and I lay before you the various plans proposed, in the order of their publication, as correctly as my means will allow.

If we excite a collodion plate in the usual way, and leave it in the dark slide until dry, we shall find that the nitrate of silver film, as it concentrates by evaporation, dissolves out the iodide from the collodion surface, and eventually crystallises in minute stellar groups, completely destroying the utility of the surface as a photographic medium. The prevention of this result was the first problem to be solved, if the plates were to be preserved any length of time after preparation. At first, various plans were tried to prevent the evaporation altogether, as, for instance, laying a second glass plate directly upon the collodionised surface. Thus Messrs. Spiller and Crooks attempted the same object, by steeping the prepared plate in a strong solution of a deliquescent nitrate. At first zinc, and subsequently magnesia, were employed. At a later period, Messrs. Shadbolt and Lyte partially washed off the excess of nitrate of silver from the plate, and poured over it a solution of honey, and I do not think there is, even now, a better process extant.

Simple oxymel, glycerine, treacle, and various other substances have also been employed. During this period the somewhat remarkable process of M. Taupenot was published, and was as follows:—The plate was first coated with collodion and sensitised in the usual way, the surface washed, and a mixture of albumen and honey slightly iodised was poured over it; it was then allowed to dry, and again plunged into the silver bath and rinsed, when it might be used, either wet or dry. The theory of the process, at the time it came out, was imperfectly understood; and, as it involved very operose manipulations, it was not so generally used as it deserved. The main point was the coating the excited collodion surface with the albumen, thus filling its pores and preserving it in some measure from the action of the air; and it being ascertained that even if the whole of the free nitrate were removed by washing, and the plate dried in the dark chamber, it was yet sensitive, although in an inferior degree than when recently prepared.

Mr. Barnes, who has been eminently successful upon dry plates, has published a pamphlet on the subject; contemporaneously, Dr. Hill Norris proposed to cover the washed plates with gelatine, and I believe his dried plates preserve their sensibility for many months. I may add that Dr. Norris has patented his process, and that a company has been formed at Birmingham to sell the prepared plates.

Mr. Sparling employed a solution of dextrine mixed with honey, and iodized in the same way as Taupenot, also following him in two immersions in the silver bath.

Mr. Maxwell Lyte has yet more recently advocated the use of what he terms *meta-gelatine*, produced by boiling a solution of gelatine with sulphuric acid until it ceases to gelatinise on cooling, then removing the acid by digestion upon carbonate of lime, filtering, and adding a portion of clarified honey. This is poured over the plate, without previously washing, and stored away until required for use. This is certainly the quickest, and, as far as I have seen it tried, the most satisfactory process, as it may be used indifferently, either wet or dry.

One word more on the peculiarities of the *meta-gelatine*. While the Taupenot, Norris, and Sparling plates are hard and horny, the *meta-gelatine* is as it were porous, and to this I attribute the fact of the non-necessity of washing away the free nitrate, and from a series of experiments I have now in progress, I think I may safely say that before long I shall, by a modification, be enabled to dispense with the collodion altogether, and at the same time preserve the rapidity of action. This would, with large-sized plates, be a great pecuniary boon to photographers, as well as obviating the uncertain way in which collodion sets on large surfaces. Of course it is not to be expected that I shall divulge my mode of procedure until I have achieved my object.

Read before the Liverpool Chemists' Association.

PHYSICS.

On the RELATION of GOLD to LIGHT.

By Professor FARADAY, D.C.L., F.R.S.

THIS subject was brought forward on the 13th June of last year, and in the account of that evening, at page 310, vol. ii. of the Proceedings of the Royal Institution, will be found a description of some of the proofs and effects then referred to and illustrated; the following additional remarks will complete the account up to this time. The general relations of *gold leaf* to light were described in the former report. Since then, pure gold leaf has been obtained, through the kindness of Mr. Smirke, and the former observations verified. This was the more important in regard to the effect of heat in taking away the green color of the transmitted light, and destroying to a large extent the power of reflexion. The temperature of boiling oil, if continued long enough, is sufficient for this effect; but a higher temperature (far short of fusion) produces it more rapidly. Whether it is the result of a mere breaking up by retraction of a corrugated film, or an allotropic change, is uncertain. Pressure restores the green color; but it also has the like effect upon films obtained by other processes than beating. Corresponding results are produced with other metals.

As before stated, *films* of gold may be obtained on a weak solution of the metal, by bringing an atmosphere containing vapors of phosphorus into contact with it. They are produced also when small particles of phosphorus are placed floating on such a solution; and then, as a film differing in thickness is formed, the concentric rings due to Newton's thin plates are produced. These films transmit light of various colors. When heated they become amethystine or ruby; and then when pressed, become green, just as heated gold leaf. This effect of pressure is characteristic of metallic gold, whether it is in leaf, or film, or dust.

Gold wire, separated in very fine particles by the electric *deflagration*, produces a deposit on glass, which, being examined, either chemically or physically, proves to be pure metallic gold. This deposit transmits various colored rays: some parts are grey, others green or amethystine, or even a bright ruby. In order to remove any possibility of a compound of gold, as an oxide, being present, the deflagrations were made upon topaz, mica, and rock crystal, as well as glass, and also in atmospheres of carbonic acid and of hydrogen. Still the results were the same, and ruby gold appeared in one case as much as in another. Being heated, all parts of the deposit became of an amethystine or ruby color; and by pressure these parts could be changed so as to transmit the green ray.

The production of *fluids*, consisting of very finely divided particles of gold diffused through water, was spoken of before. These fluids may be of various colors by transmitted light from ruby to blue; the effects being produced only by diffused particles of metallic gold. If a drop of solution of phosphorus in bisulphide of carbon be put into a bottle containing a quart or more of very weak solution of gold, and the

whole be agitated, the change is brought about sooner than by the process formerly described; or if a solution of phosphorus in ether be employed, very quickly indeed; so that a few hours' standing completes the action. All the preparations have the same qualities as those before described. The differently colored fluids may have the colored particles partially removed by filtration; and so long as the particles are kept by the filter from aggregation, they preserve their ruby or other color unchanged, even though salt be present. If fine isinglass be soaked in water, then warmed to melt it, and one of these rich fluids be added, with agitation, a ruby jelly fluid will be obtained, which, when sufficiently concentrated and cold, supplies a tremulous jelly; and this, when dried, yields a *hard ruby gelatine*, which being soaked in water, becomes tremulous again, and by heat and more water yields a ruby fluid. The dry hard ruby jelly is perfectly analogous to the well known ruby glass, though often finer in color; and both owe the color to particles of metallic gold. Animal membranes may in like manner have ruby particles diffused through them, and then are perfectly analogous in their action on light to the gold ruby glass, and from the same cause.

When a leaf of beaten gold is held obliquely across a ray of common light, it *polarizes* a portion of it; and the light transmitted is polarized in the same direction as that transmitted by a bundle of thin plates of glass; the effect is produced by the heated leaf as well as by the green leaf, and does not appear to be due to any condition brought on by the heating or to internal structure. When a polarised ray is employed, and the inclined leaf held across it, the ray is affected, and a part passes the analyser, provided the gold film is inclined in a plane forming an angle of 45° with the plane of polarisation. Like effects are produced by the films of gold produced from solution and phosphorus, and also by the deposited dust of gold due to the electric discharge. The same effects are produced by the other deflagrated metals, so long as the dusty films are in the metallic state. As these finer preparations could be held in place only on glass or some such substance, and as glass itself had an effect, it was necessary to find a medium in which the power of glass was nothing; and this was obtained in the bisulphide of carbon. Here the effect of gold upon a ray of light which was unaffected by the glass supporting it, was rendered very manifest, not only to a single observer, but also to a large audience.

The object of these investigations was to ascertain the varied powers of a substance acting upon light, when its particles were extremely divided, to the exclusion of every other change of constitution. It was hoped that some of the very important differences in the action upon the rays, might in this way be referred to the relation in size, or in number of the vibrations of the light and the particles of the body, and also to the distance of the latter from each other; and as many of the effects are novel in this point of view, it is hoped that they will be of service to the physical philosopher.

Read before the Royal Institution of Great Britain.

TOXICOLOGY.

SUSPECTED CASES of POISONING of CALVES by YEW LEAVES (*TAXUS BACCATÆ*).

On the 3rd of April last, I was summoned to attend three yearling calves, the property of Major T. E., which were said by the messenger to be vomiting very much.

On my arrival, at noon, I found my patients in a pasture field, and was told they had not been perceived unwell previous to 7 a.m.

They shewed the following symptoms:—Loss of power in the hind extremities, evidenced by a staggering gait; great dulness; eyes prominent and watery: a little frothing at the mouth; irritation of the rectum, manifested by frequent straining; pulse slightly accelerated; breathing nearly natural.

My impression on observing these symptoms was, that the calves had eaten some plants of a poisonous nature. I therefore asked whether they had been in any plantation, or had had access to any branches of the yew-tree; and was answered in the negative. My attention was next directed to the herbage of the field, but I could not discover anything to account for their illness. Being told that some of the vomited matter was still to be seen, I examined it, and found it to contain several leaves of the rhododendron, and, as I believed, laurel, but no yew. On inquiring as to where they had obtained the rhododendron, I was shown a heap of cuttings which had been carried from the garden and placed in one corner of the field, and was further informed that the calves were seen eating of these the previous evening. On searching the heap I found it to consist chiefly of the leaves, &c., of the rhododendron, but it also contained some fir cuttings, and *a very few sprigs of green yew*.

I had not yet discovered any traces of yew in the vomited matter; but on my expressing my conviction that the calves were poisoned by it, an attendant told me that he and another of the servants had seen some in other vomited matter in another portion of the field. On going to look at this, I found that the yew, if there had been any, had been taken away. I may here state, that it seemed to be with great reluctance that the servants gave me any information, and it was only on my going away that one of them showed me a large branch of yew which he said had been found in the heap before I searched it. This branch, however, did not appear to have been much browsed upon by the calves; and I still remained in ignorance of the quantity of yew they had eaten, and, indeed, as to whether they had eaten any.

Treatment.—Having had my patients placed in a loose box, I administered to each a saline purgative containing carbonate of ammonia. I then directed a sinapism to be applied to the loins of each; the calves be well rubbed with wisps of straw, and have enemata of warm water thrown up hourly, with a plentiful allowance of thin gruel and warm water, but no solid food.

At 6 p.m. I gave to each a dose of linseed oil, with

Spt. Ether. Nit. et Spt. Ammon. Arom.,

and ordered the continuation of treatment as before directed. On the next day I found the calves much better; the purgative had operated freely, and the animals were very desirous of food. The following day they were all convalescent.

W. L.

[Does it follow that the yew was the only poisoning vegetable in the above cases? May not the rhododendron have had something to do with it? We are not told what variety of this plant it was, the leaves of which were found by W. L. Burnett places many of them among suspicious plants, and states some of them to be deleterious. The *Rhododendron ponticum* is said by him to exude a nectareous juice, from which honey being made by bees, it possesses intoxicating and poisonous properties of a phrenetic kind, which is not only formidable in its symptoms, but also very lengthened in its duration. This plant is often found associated with the *Azalea pontica*, believed to be the *Egolethron* of the ancients; and it was to many of the soldiers eating honey collected from such plants that the plague was attributed which afflicted the army of Xenophon, in the celebrated retreat of the 10,000.]—*Editor of Veterinarian*.

Veterinarian, June, 1857.

PUBLIC HEALTH.

TREATMENT in CASES of SUSPENDED RESPIRATION from DROWNING.

ROYAL NATIONAL LIFE-BOAT INSTITUTION.

14, John-street, Adelphi,
London, W. C., 29th May, 1857.

SIR,—I am directed to forward to you the annexed Circular, relative to the treatment of persons apparently drowned, and to say that the Committee of this Institution would esteem it a favor, if you would kindly allow the same to appear in *THE CHEMIST*.

I am, SIR,

Your very obedient servant,

RICHARD LEWIS,
Secretary.

To the Editor.

The attention of the Committee of the Royal National Life-Boat Institution, has been recently called to the proposition by Dr. Marshall Hall, of a new method of treating persons apparently drowned, which has been to some extent discussed in the columns of the medical journals, and is now known by the designation of "Marshall Hall's ready Method."

The subject is felt by the Committee to be one of great—indeed, national—importance, since the restoration to life of numberless persons, both on the coast and in the interior parts of this country, must, doubtless, depend on the adoption of a proper treatment, or the reverse.

But more especially is the subject one of vital importance to this institution, since at its numerous life-boat stations the printed rules for observance, which are posted up in its boat-houses, are the sole guide to those persons who are called on to minister to drowning persons before medical attendance can be procured.

It thence follows, that the issue of instructions by the institution, on this important subject, clothe it with a responsibility which may well make its Committee of Management anxious as to the character of those instructions.

Up to the present time, the institution has adopted the rules issued by the Royal Humane Society, whose extended sphere of operations might be supposed to give it all the advantages derivable from practical experience.

The promulgation of Dr. Marshall Hall's method, which the Committee are informed has already awakened much interest in the medical profession, has, however, led them to feel that it is their duty to obtain all the information on the subject, which either science or experience has revealed, with a view to the reconsideration of those rules which the institution distributes for information amongst our coast population, and on which the issues of life or death must so often depend.

The Committee solicit the opinion of all medical men on this important subject, especially that of those who have had opportunities for attending persons apparently drowned, in order that the institution may possess the advantage of such individual or collective information as the profession may be able to afford.

The Committee believe, that the above simple statement of the humane and important object which this Society has in view, will be sufficient to elicit a general response from a profession, whose whole sphere of duties is so intimately bound up with the welfare and happiness of mankind, and which has always largely extended its sympathy to humane and charitable undertakings in our land.

RICHARD LEWIS,

14, John-street, Adelphi,
May, 1857.

*Secretary to the Royal National Life-Boat
Institution.*

TO RESTORE PERSONS APPARENTLY DROWNED.

Royal Humane Society's Instructions.

Dr. Marshall Hall's Instructions.

SEND QUICKLY FOR MEDICAL ASSISTANCE.

Cautions.

1. Lose no time.
2. Avoid all rough usage.
3. Never hold up the body by the feet.
4. Nor roll the body on casks.
5. Nor rub the body with salt or spirits.
6. Nor inject tobacco smoke, or infusion of tobacco.

1. Treat the patient *instantly, on the spot, in the open air, exposing the face and chest to the breeze, except in severe weather.*

I.—TO CLEAR THE THROAT.

2. Place the patient gently on the face, with one *wrist* under the forehead;

[all fluids and the tongue itself then fall

I. Convey the body carefully, on its face, with the head and shoulders supported in a raised position, to the nearest house.

II. Strip the body, and rub it dry; then wrap it in hot blankets, and place it in a warm bed in a warm chamber, free from smoke.

III. Wipe and cleanse the mouth and nostrils.

IV. In order to restore the natural heat of the body,

— Move a heated covered warming-pan over the back and spine.

— Put bladders or bottles of hot water, or heated bricks, to the pit of the stomach, the arm-pits, between the thighs, and to the soles of the feet.

— Foment the body with hot flannels.

— Rub the body briskly with the hand; do not, however, suspend the use of the other means at the same time; but, if possible, immerse the body in a warm bath at blood heat, or 100° of the thermometer, as this is preferable to the other means for restoring warmth.

V. Volatile salts or hartshorn to be passed occasionally to and fro under the nostrils.

VI. No more persons to be admitted into the room than are absolutely necessary.

If apparently dead from intense cold.

Rub the body with snow, ice, or cold water. Restore warmth by slow degrees; and after some time, if necessary, employ the means recommended for the apparently drowned. In these accidents it is highly dangerous to apply heat too early.

GENERAL OBSERVATIONS.

On the restoration of life, a teaspoonful of warm water should be given; and then, if the power of swallowing be returned, small quantities of wine, or diluted brandy, warm; the patient should be kept in bed, and a disposition to sleep encouraged, except in cases of apoplexy, intoxication, and *coup de soleil*. Great care is requisite to maintain the restored vital actions, and at the same time to prevent undue excitement.

The treatment recommended by the Society to be persevered in for THREE or

forwards, leaving the entrance into the windpipe FREE.]

If there be breathing—wait and watch; if not, or if it fail.

II.—TO EXCITE RESPIRATION.

3. Turn the patient well and *instantly* on his side, and—

4. Excite the nostrils with snuff, or the throat with a feather, &c., and dash cold water on the face previously rubbed warm.

If there be no success, *lose not a moment*, but *instantly*—

III.—TO IMITATE RESPIRATION.

5. Replace the patient on his face, raising and supporting the chest well on a folded coat, or other article of dress.

6. Turn the body very gently on the side and a little beyond, and then briskly on the face, alternately; repeating these measures deliberately, efficiently, and perseveringly fifteen times in the minute, occasionally varying the side;

[when the patient reposes on the chest, this cavity is compressed by the weight of the body, and EXPIRATION takes place; when he is turned on the side, this pressure is removed, and INSPIRATION occurs.]

7. When the prone position is resumed, make equable but efficient pressure, with brisk movement, along the back of the chest; removing it immediately before rotation on the side;

[the first measure augments the expiration, the second commences inspiration.]

* * THE RESULT IS,—RESPIRATION;
—AND, IF NOT TOO LATE,—LIFE!

IV.—TO INDUCE CIRCULATION AND WARMTH.

8. Meantime rub the limbs upwards, with firm grasping pressure and with energy, using handkerchiefs, &c.

[by this measure the blood is propelled along the veins towards the heart.]

9. Let the limbs be thus warmed and dried, and then clothed, the bystanders supplying the requisite garments.

FOUR HOURS, as it is an erroneous opinion that persons are irrecoverable because life does not soon make its appearance, cases having come under the notice of the Society of successful results, even after five hours; and it is also absurd to suppose, that a body must not be meddled with or removed, without the previous permission of a Coroner.

10. Avoid the continuous warm bath, and the position on, or inclined to, the back.

BIBLIOGRAPHY.

A MANUAL OF PHOTOGRAPHIC CHEMISTRY, INCLUDING THE PRACTICE OF THE COLLODION PROCESS. By T. FREDERICK HARDWICH, Lecturer on Photography in King's College, London; late Demonstrator of Chemistry in King's College. Fourth Edition. London: Churchill, 1857.

THE large sale of this work says more in its favor than anything else could do. It is scarcely a year since the third edition was published.

The author informs us that "since the publication of the third edition, a series of experiments have been made on the manufacture of collodion, the results of which have thrown further light upon the condition affecting the sensitiveness of the excited film, and have enabled the writer to introduce an organic substance, 'glycirrhizine' which will be found of service in making photographic copies of engravings and similar works of art."

"Dr. Norris, of Birmingham, has within the last few months communicated a paper on dry collodion, which places the theory of that subject upon a better footing than before. The oxymel preservative process is now also thoroughly understood, and may be considered certain."

"The albuminised collodion of M. Taupenot* which experience proves to be one of the best dry processes at present known, is included in this edition."

We have quoted the preface for these additions; besides these, alterations have been made in the style and general arrangement of the work, and additional matter has been inserted.

The work fully merits the encouragement it has received.

CARLO MINASI'S GUIDE to his PATENT ARTIFICIAL HATCHING MACHINE.

M. MINASI, (the well known composer of music) does not, while worshipping the Muse, despise the more prosaic matters of ordinary utility.

The little work before us is a guide to the use of one of his numerous mechanical inventions—the hydro-incubator; and from it we select the following brief—

* See THE CHEMIST for December, 1855.

"DESCRIPTION OF THE MACHINE."

"The Patent Hydro-incubator consists of a centre and two wings. The central portion is formed of a strong zinc square case, about two to four inches in depth, the upper part being like a tray. The tray part is that the eggs are first laid upon for hatching, and the wings, or outside compartments, are in reality, the miniature poultry yards of the new hatched chicks; this zinc case is water tight all round, top and bottom; the lower surface is corrugated and covered over with fine sand. In order to keep up the requisite heat, a constant stream of hot water is kept flowing through the corrugated channels of the under part, which communicates its warmth to the part above it. This hot water is obtained from a small vessel, under which a lamp is constantly burning. The eggs to be hatched are then placed upon the layer of sand, and covered with glass or some other suitable article. In order to compensate for the watery evaporation from the eggs, and which, if allowed to proceed to any great extent without a counteracting process, would destroy the germ *in ovo*, a number of short tubes, passing from the under to the upper side of the case and reaching above the larger part alluded to, are so arranged that the moisture from the atmosphere can rise up from below and distribute itself over the whole surface of the eggs. The lamp is placed upon a very easily managed stand, so as to be lowered or raised by an end-screw in a moment. This lamp possesses certain improvements, effected by the inventor, in the combination of cotton and asbestos to conduct the spirit to the burner, and which will enable it to burn naphtha, without a wick, for six months consecutively, without any change of conducting material. In order to economise the heat from the lamp, the caloric is made to traverse a spiral flue, which detains a portion on the sides—a very small quantity only passing away through a pipe into the atmosphere. This heat from the sides of the flue acts upon the water, and the Incubator having been constructed so that one end is higher than the other, a continual circulation of the heated water is secured. The under side of the zinc case is corrugated, in order that the chicks which are reared in a chamber, of which it forms the upper part, may nestle against it. There is this further advantage with respect to the lamp, namely, that when it is burning, it is in a kind of small closet, closed by a wire gauze, or perforated door, and can be locked up from the interference of any person but the holder of the key; accidents, therefore, from this source, are wholly out of the question."

M. Minasi gives minute directions for the use of his Incubator. He also supplies a very interesting chapter on the structure of eggs, and on the formation of the chick, tracing its progress from day to day, with illustrations. The work concludes with a few words on the leading characteristics of our principal domestic poultry.

M. Minasi's Incubator appears to possess every desirable quality, and in the hands of any person capable of following the instructions conveyed in his guide, it will be found a most valuable apparatus.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

On the MECHANICAL EFFECT PRODUCED by the EXPLOSION of GUN-COTTON.

By JOHN S. BLOCKEY.

HAVING recently been engaged in a series of experiments, with the view of determining the mechanical force manifested by the explosion of gun-cotton, I think possibly some of your readers may find something in this communication worthy of notice.

I had a strong cylinder constructed, with a piston fitting air-tight in its interior, and fitted on a horizontal beam, in an upright position. To the piston was fixed a table, on which weights were fixed; and a pencil pressing by a spring against a stretched paper index, marking the length the piston had travelled in each experiment, with the weight on the table; this weight, added to the weight of the table and piston itself, and the weight equivalent to the friction of the piston in the cylinder, would give the sum of the weight capable of being lifted through the distance marked on the paper index, by the quantity of cotton introduced into the cylinder. The cotton was introduced into a small cavity in the bottom of the cylinder, in such a manner as to avoid as much as possible the chance of the effect being modified by the presence of atmospheric air beneath the piston, and fired by several platinum wires, disposed at several parts of the little pledget of cotton, heated simultaneously by the voltaic battery. After several trials, I found the maximum effect producible by one grain of cotton was 21lbs., lifted a distance of one foot, by the outward explosion of the material; besides a certain effect, for the succeeding vacuum, repeated experiments with the same cotton, gave the same effect. The cotton here was prepared with 1.5 nitric acid, and ordinary sulphuric acid, 3oz. of sulphuric to 1 of nitric.

Cotton prepared in different ways, gave greatly different results; in some experiments I could only lift 7lbs. through 9 inches. To determine the vacuum power after the explosion, I had an apparatus constructed, so that the weight on the table was balanced or removed directly the piston had ended its outward journey. This was effected by a spring and catch bringing a wheel into operation, loaded with a weight just equal to that on the table, plus the weight of the piston and table itself, minus the equivalent of the friction; and thus exactly balancing the weight tending to depress the piston. A small additional weight was then attached, and the distance through which this could be pulled, indicated the force of the vacuum. But the rapidity with which the vacuum succeeds the explosion, has prevented me

from accurately measuring its amount, the piston returning towards the lower end of the cylinder before the mechanism has effected its object, and balanced the weight on the table. So rapid is this indeed that I have not thought it worth while even to endeavor to estimate the effect by a column of water and valve, the piston being capable of moving in one direction only, as theory might possibly suggest.

Any apparatus of the kind must be of a complex nature, and it is of maximum importance, that with such a material as gun-cotton, it must be of most simple construction.

Trusting, however, that it may at least be worth while to know the explosive power of gun-cotton in one way at all events, I have forwarded you this short communication. I am still busy with the mechanical effects of several other explosive compounds; among others, of a mixture of water-gas (H and CO) and atmospheric air, and with oxygen, and when my investigation is complete will forward you the results.

On CHINESE BOTANY and PHARMACOLOGY, and the LATEST RUSSIAN RESEARCHES THEREON. By Dr. LOTSKY.

It is necessary to premise, on the present occasion, that the Russian Government is the only one which, since the last century, entertains an especial Diplomatic Mission at Peking, located in buildings of great extent. Amongst the staff of the mission is also a Medical Officer, whose term of residence in China was formerly fourteen years, but has been lately shortened to seven. Several of them have occupied their leisure in researches on the different departments of Chinese medical science.

It was thus that Dr. Kiriloff had collected, so far back as the year 1841, 127 different Chinese drugs, which he brought with him to Russia, and where they were first deposited in the Foreign Office of St. Petersburg. In the year 1847 these specimens were transferred to the Museum of the Medical and Surgical Society of St. Petersburg, and Mr. A. Horaninoff, member of the Academy, and Actual Privy Councillor, was commissioned, by an especial order of Nicholas I. to study them and to make a clinical examination thereof. As a preliminary, Dr. Kiriloff had made extensive researches into a large number of Chinese medical works,—a kind of Eastern literature, but very little known hitherto in Europe; but by the knowledge of which alone a clue to the usage of unknown Chinese drugs could be obtained. In the meantime the members of the Medical and Surgical Academy had examined them chemically and physiologically. Finally, forty Chinese drugs (unknown in Europe), were tried on thirty-seven hospital patients with the most satisfactory results.

While these researches were going on in St. Petersburg, Dr. Tatarinow, who had remained ten years in China, returned home in 1851. The doctor had studied and translated Chinese medical works while in China; he had made collections in Peking, and in a radius of 300 kilometres around that capital, so far as Mongolia, the results of which

were 800 species of plants in a number of dried specimens for the herbarium, and a vast collection of 500 sorts of drugs, some in such quantities as to allow a large scope for experiments and *exchange* with other medical establishments in Europe.* At the same time most of these plants had been drawn in a superior way, in folio, by a Chinese artist, but as Dr. Tatarinow superintended the painting, most detailed analyses of the plants are added, which greatly enhance the value of the collection. Very interesting in these portfolios are the many varieties of the genus, *prunus*, *pyrus*, *malus* (plums, pears, and apples), exhibiting most curious and monstrous forms of these trees and their fruit, originally brought here from *other* parts of Asia. If the accuracy of Chinese chronology, with its vast series of years, could obtain any additional evidence, it might obtain it from these forms of cultivated fruit. Ages must have passed by, until the very nature of organisms could have become thus changed, and, as it were, transmuted. This portion of Dr. Horaninoff's intended work will not be the least interesting for phytology and horticulture, as it is known that the Chinese, who can rear an oak-tree with acorns only a span high, are very expert in the latter branch of science.

From the collections of Drs. Koriloff and Tatarinow, it would appear that the Chinese possess drugs and medicines innumerable. Some of them, as derived from Dr. Horaninoff's note-book, are *radix Sophoræ flaviscentis*, *radix ginseng*, *caules Ephedræ*, *radix Pupalizæ*, *Ptilolycodonis*, *Epimedii*, *Ari tryphilli*, &c. These and the other remedies were found very efficacious in chronic diseases, which yielded in the usual proportion of cure and failure, and *only* in a few cases the additional aid of European remedies was to be called in. As, however, the Chinese therapeutics prescribe *larger doses*, taken several times a day, the Commission waived this, and conformed more to the rules of European medicine. Privy Councillor Horaninoff, known as a deserving author on botany and materia medica, and whose Russian works have much advanced medicine in his native country, has been commissioned to compile the volumes on Chinese botany and pharmacology, which, as they will be printed in the Offices of the Academy of Sciences of St. Petersburg, will probably be as splendid as the "*Antiquités du Bosphore Cimmerien*,"—one of the finest productions issued from any Press in modern times.† Dr. Horaninoff had visited, last autumn, Berlin and London, to exhibit his *Icones Sinicas* and his drugs to the learned of both these cities. Amongst the specimens of his collection was the root of an orchidean (?) plant, which, resembling the human form, is sold, by the superstitious Chinese, at the weight of gold. It appears that they know sublimate and arsenic, but make no use of it in medicine. We may judge from their preparation of Indian ink, that they must be in possession of most curious chemical contrivances. They have no medical schools; abhor anatomy; but

* The Councillor of State, Horaninoff, of St. Petersburg, will, we are sure, afford any information on this interesting subject.

† *Vide* 'Notes and Queries,' 1856, No. 47, by Dr. Lotsky.

their medical men must, when employed by the Government or their towns, undergo an examination from their old medical authors and text books.

It cannot be doubted that the knowledge of about 600 sorts of new drugs, collected in a country extending from the tropic to the snow of the Altai, will have an influence on the condition of *materia medica* and medicine in general. The analogy between such an expanse of country, and the strip of tropical America, whence we have derived the Peruvian bark and balsam, sarsaparilla, &c., is still increased, if we consider that in China the experience of thousands of years is to be thrown into the scale. On the other hand, the "National Chinese Party," the offspring of European influence and initiation, are panting after European instruction and teachers. Thus, while the work of Dr. Horaninoff is in preparation, the medical men of Europe, visiting China under present emergencies, may know that they are on a spot where great sources of scientific and commercial importance are yet to be discovered.

15, Gower Street North, London, July, 1857.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

On INVERSE SUBSTITUTIONS. By M. BERTHELOT.

FIRST PART.

CHEMISTS have learnt to replace hydrogen by chlorine, bromine, or iodine in organic substances; but they have hitherto been able only in a few peculiar cases to solve the inverse problem, which consists in regenerating the original compound by means of the transformed compound.

Four processes have been employed with this view:—

1. M. Melens changed chloracetic acid, $C^4HCl^3O^4$, into acetic acid, $C^4H^4O^4$, and perchloride of carbon, C^2Cl^4 , into marsh gas, C^2H^4 , by the simultaneous action of water and of the amalgam of potassium; but an analogous transformation did not succeed with the chloruretted derivatives of hydrochloric ether. The employment of the amalgam of potassium appears to be suitable only for chloruretted bodies very easy of decomposition; in other cases its action is exerted exclusively on the water.

2. M. Kolbe likewise replaced by hydrogen the chlorine of chloracetic acid; he operated by means of the battery, zinc being employed as the electrode. By the same process he effected a similar substitution in a very curious series of peculiar acids derived from the action of chlorine on sulphuret of carbon. We may observe that the battery can act only on compounds soluble in water or in a conducting liquid.

3. The hydriodic ethers, C^4H^5I , C^3H^3I , and C^6H^5I , attacked by

zinc or by sodium, at a high temperature, lose their iodine without substitution, and furnish the carburets designated under the name *ethyle*, C^4H^5 , *methyle*, C^3H^3 , *allyle* C^6H^5 , &c. When we operate with zinc, in presence of water, peculiar carburets are formed, in which the iodine of the hydriodic ether is replaced by hydrogen; hydroguret of ethyle, C^4H^6 ; marsh gas, C^2H^4 ; propylene, C^6H^6 . This is the most extended example of inverse substitution known; it is due to the labors of Dr. Frankland.

4. In the researches on ioduretted propylene which I carried out in conjunction with M. De Luca, I replaced iodine by hydrogen, by means of a peculiar process which has become the germ of the present work.

This process consists in making mercury and hydrochloric acid employed simultaneously react on ioduretted propylene, C^6H^5I ; whence results, even cold, the formation of propylene, C^6H^6 , of iodide of mercury and chloride of mercury, all bodies none of which are produced without heat under the influence of the above mentioned reagents employed two by two; but they are produced by the simultaneous concurrence of several affinities supporting each other, nearly in the same way as the chlorides of silicium and boron are produced in the simultaneous reaction of chlorine, charcoal, and silicic or boracic acid, which, taken two by two, do not exert any neutral action.

The foregoing facts comprise all the known observations of inverse substitutions; we may judge how they are limited and restricted to almost always individual cases.

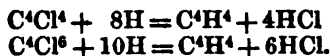
My researches relative to the synthesis of the carburets of hydrogen have led me to study inverse substitutions in a more general manner; in all the cases in which I have tried the experiment I have succeeded, by various means, either in substituting hydrogen for chlorine, iodine, and particularly bromine, in carburets modified by substitution, or in regenerating the original carburets after they have undergone the action of the haloid bodies.

The processes which I have put in operation rest, sometimes on the employment of free hydrogen at a high temperature, and sometimes on the concurrence of two simultaneous affinities, equivalent to the employment of nascent hydrogen. In the last case, time is an essential element of the phenomenon.

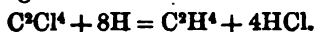
I. Free Hydrogen.—Free hydrogen combines with the chlorine of the chlorides of carbon towards the temperature of nascent redness; at the same time, the carburet of hydrogen corresponding to the chloride of carbon under experiment is regenerated. A considerable proportion is destroyed under the influence of the heat; but a portion resists and may be collected. This process is applicable only to very stable substances, but for that very reason it is suitable for the compounds in which all the hydrogen may be replaced by chlorine—a phenomenon which proves great stability, both in the primitive carburet and in the chloride of carbon derived from it.

In these conditions, the protochloride of carbon, C^4Cl^4 , and the

sesqui-chloride of carbon, C^4Cl^6 , furnish a considerable proportion of olefant gas, C^4H^4 .



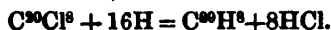
Perchloride of carbon, C^2Cl^4 , produced marsh gas, C^2H^4 , and olefant gas. Marsh gas results from an inverse substitution:—



With regard to olefant gas, it appears to derive its origin from the well-known decomposition by virtue of which perchloride of carbon, heated to redness, is separated into chlorine and protochloride:—



The chloride of carbon, $C^{20}Cl^{18}$ (perchloruretted naphthaline) re-produced at a bright red heat, $C^{20}H^{18}$:—



The chloride of Julin (C^2Cl) reproduced, in large quantity, a crystallised body presenting the characters of naphthaline; no gaseous carburet was formed simultaneously in sensible proportion. In this property, as well as in its odor and relative fixity, the chloride of Julin, as it appears to me, should be removed from the series of olefant gas, in which it is included at present, and brought into that of naphthaline. It is probably a perchloruretted chloride of naphthaline:— $C^{20}Cl^{10} = C^{20}Cl^{18} + Cl^2$; a very curious result, when we look at the origin of Julin's chloride. This body, indeed, endowed with great stability, appears to be one of the ultimate products of the decomposition of the chlorides of carbon, almost as naphthaline is one of the ultimate products of the decomposition of the hydrogurets of carbon. This accords with the ideas of substitution which imply a certain analogy of grouping between these two series of compounds.

The Julin's chloride, the perchloride, the sesquichloride, and the protochloride of carbon employed in the foregoing experiments, were prepared by Kolbe's process, by means of chlorine and sulphuret of carbon. These results furnish, then, a new means of producing olefant gas, marsh gas, and naphthaline, by means of the simple bodies which constitute them.

XX. Nascent Hydrogen.—I shall first detail the facts relative to the bromides of ethylene, propylene, &c.; then I shall pass to various other compounds: these are the first bodies which led me to the studies whose results I give here.

Having isolated the alcoholic carburets of hydrogen, collected in the most complex gaseous mixtures, under the form of bromides, I made a great variety of experiments for regenerating each of the carburets engaged in the bromuretted combination, in order to confirm its existence by studying it separately. The succinct description of these experiments will throw some light on the nature of the actions which should be employed with regard to organic matters.

I first tried the employment of the isolated metals, such as sodium,

iron, zinc, copper and mercury. But these bodies, heated to 100° 200° and 300° C. (212°, 392°, and 572° F.) with bromide of ethylene, $C^4H^4Br^2$, do not regenerate olefiant gas, C^4H^4 , in considerable proportion; at most, they form mono-bromuretted ethylene, C^4H^3Br .

I then had recourse to nascent hydrogen. Zinc, heated with water and bromide of ethylene to 300° C. (572° F.), regenerates olefiant gas: but the substitution is ordinarily incomplete; and, moreover, this gas is mixed with a very large quantity of free hydrogen, which renders the opening of the tube in which the experiment has been performed a dangerous operation. The free hydrogen results from the decomposition of the water by the zinc—a decomposition produced at the same time as the reaction which we desire to obtain, and independently of this reaction itself. This independence of the two reactions is an unfavorable circumstance; it is most frequently opposed to a complete substitution; the decomposition of the water being completed before the decomposition of the bromuretted compound. Thus, I addressed myself in preference to the metals, which do not decompose water by themselves, but which seemed to me proper for decomposing it by complex affinity with the simultaneous concurrence of bromide of ethylene.

Mercury, tin, lead, and copper, in presence of water, potassa, and hydrochloric acid, have been tried by turns; copper, in presence of pure water, gave the best results. Indeed, bromide of ethylene, heated to 275° C. (527° F.), with water and copper, loses its bromine, and furnishes olefiant gas mixed with a certain proportion of hydrogen, and with small quantities of carbonic oxide and hydroguret of ethyle.* But this reaction is extremely slow; it becomes complete only at the expiration of thirty or forty hours, in contact with matters at 275° C. (527° F.) I endeavored to render it more rapid by turning to account the well-known instability of iodide of ethylene. I thought that if we were placed in such conditions that the compound would have a tendency to be formed, we should more easily realise the regeneration of olefiant gas. For this purpose, I made bromide of ethylene, copper, water, and iodide of potassium, react simultaneously at 275° C. (527° F.); the special affinity of iodine for copper should contribute to the result.

In these conditions, the reaction is complete at the end of from twelve to fifteen hours. It gives rise to olefiant gas, mixed with a little hydroguret of ethyle, and more often with hydrogen, carbonic oxide, and even carbonic acid.

After having realised these experiments, I tried what result would be produced by suppressing the copper. I caused a mixture of bromide of ethylene, water, and iodide of potassium to react at 275° C. (527° F.), and I found that the bromide of ethylene was still decomposed, with liberation of a portion of the iodine of the iodide of potassium: only the gas produced consisted principally of hydroguret

* See *L'Institut*, April 22, 1857, for my note on the analysis of carburetted gases.

of ethyle, C^4H^6 , mixed with a variable proportion of olefant gas, carbonic acid, and often hydrogen and carbonic oxide. Thus, under the influence of water and iodide of potassium, the bromine of bromide of ethylene is replaced by hydrogen—a singular result, but which seems due to causes analogous to those which act in the foregoing reactions.

Comptes Rendus, No. 24, June 15th, 1857.

CONTRIBUTIONS to the HISTORY of some ORGANIC BODIES.

By M. V. DESSAIGNES.

I. Urea.

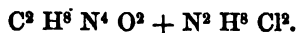
FOURCROY and Vauquelin have described under the name of urea, a substance crystallised in thin and colored square plates, which they obtained by treating the extract of urine with hot alcohol, filtering and allowing to cool. They were aware that this substance contained hydrochlorate of ammonia, but they left it undecided whether it existed as an impurity, or in the state of combination. It appeared natural to regard these crystals as formed by the union of hydrochlorate of ammonia and urea. M. Dumas holds, indeed, that these two bodies dissolved in water, combine equivalent for equivalent; on the other hand, M. Werther has not succeeded in effecting this combination. By evaporating urine in order to prepare creatine, I obtained a large quantity of crystals, the examination of which has enabled me to clear up these contradictions.

When we concentrate large quantities of urine, especially by boiling it, a portion of the urea which it contains is decomposed. In this case, the urine brought to the consistence of a syrup, was filled on cooling with rather brown crystalline plates, which are the urea of Fourcroy and Vauquelin. They are freed from their mother-liquor by decantation and pressing, and as they are deliquescent, they were purified by throwing them on a filter, and abandoning them to the damp air. It is thus easy, after a few crystallisations, to obtain them perfectly colorless, and purified from the common salt which at first accompanies them. They are then presented, sometimes under the form of square tables, whose thickness may reach to half a millimetre, and sometimes in long needles, very much resembling those of pure urea. I analysed these crystals by determining the hydrochloric acid with nitrate of silver, the urea with nitric acid, and the ammonia with chloride of platinum.

I thus obtained :—

	Tables.	Needles.	Calculation.
H Cl . . .	31·70	31·64	32·16
Urea . . .	49·76	50·60	52·86
Ammonia . .	14·53	15·18	14·98

The calculation was based on the formula—



We do not obtain this combination by dissolving equal equivalents of muriate of ammonia and urea. Hydrochlorate of ammonia crystallises at once; but if we remove these crystals, the urea containing hydrochlorate of ammonia, being in presence of an excess of urea, then crystallises. Thus, it is sufficient to obtain it, to dissolve together two equivalents of urea and one of hydrochlorate of ammonia. It is very stable in presence of an excess of urea, and may be recrystallised as often as desired. When it is pure, on the contrary, water partially decomposes it. When it is treated with a quantity of water insufficient for completely dissolving it, a crystalline powder of hydrochlorate of ammonia is seen to appear; the mother-liquor decanted and evaporated, reproduces the primary combination, which is again decomposed by water. By repeating this operation we ultimately obtain large square prisms, which are urea, whose solution is scarcely rendered turbid by nitrate of silver. I may add, that the most advantageous process for obtaining hydrochlorate of ammonia with urea, consists in evaporating urine, strongly acidulated with hydrochloric acid. This combination of urea, heated on a sand-bath, when it is dry, readily gives cyanuric acid.

Urea and benzoic acid dissolved in water do not combine. When we gradually add urea to a concentrated solution of this same acid in absolute alcohol, heating to solution and cooling each time, at a certain time the crystals of benzoic acid are replaced by heavy and hard agglomerations of lamellar and confused crystals of a very different appearance. Freed from their mother-liquor and dried *in vacuo*, between two porous plates, they were decomposed by water, and I contented myself with separating the acid and the urea by filtration. I thus obtained: benzoic acid 48.56 per cent. and urea 47.66. The formula $C^{14}H^{12}O^4 + 2(C^2H^8N^4O^2)$ requires,—acid 50.5, and urea 48.5.

I have not succeeded, by the same process, in preparing hippurate of urea; but hippuric acid readily dissolves in melted urea. The mixture dissolved hot in absolute alcohol, produced lamellar crystals which, pressed between folds of paper, dissolved entirely in water. Next day, the hippuric acid had crystallised in the liquor, which contained besides urea in solution.

I have also prepared the bimalate of urea, which is a salt in very fine crystals. The neutral malate does not crystallise.

Alloxane.

Alloxane, which may be regarded as mesoxalate of urea minus water, behaves with the salts of mercury in the same manner as urea, and as alloxane. It does not precipitate corrosive sublimate, but it precipitates nitrate of mercury. It also combines directly with red oxide of mercury. This also takes place with urea, as I showed some years before Liebig's publication on this subject. (See *L'Institut*, Jan. 4, 1849.)

If we gradually add oxide of mercury, prepared by the humid way, to a solution of alloxane, heated to about 60° C. (140° F.), it disap-

pears by agitation. The addition of oxide is continued until a little white powder is formed, which does not dissolve. The clear liquor is then decanted, and deposits at the end of 24 hours a white powder. This powder, washed and dried in the air remains white; but it turns yellow *in vacuo*, and especially when heated to 100° C. (212° F.) It is a combination of alloxane and oxide of mercury. Indeed, when it is incompletely decomposed in water by means of sulphuretted hydrogen, the filtered liquid evaporated gave large effervescent crystals, which gave a blue precipitate with sulphate of iron and potassa. When the current of sulphuretted hydrogen is continued, alloxantine is produced.

I analysed the compound of alloxane and mercury, by dissolving it in hydrochloric acid, and precipitating the mercury with chloride of tin. The mercury, estimated with all the precautions prescribed for avoiding loss, represented red oxide, 54·84 and 54·55 per cent. of the matter dried at 100° C. (212° F.) The formula $C^8 H^6 N^4 O^{10}, H^2 O^2, 2 (Hg O)$, requires 548 per cent. of mercury. This same matter dried at 100° C. (212° F.), then left to the damp air, rapidly becomes white, and in two days its weight increased 13·50 per cent., which corresponds to six equivalents.

It is also the same body that is produced when we precipitate alloxane with nitrate of mercury. We add the salt of mercury drop by drop to the warm solution of alloxane, until the precipitate ceases to be dissolved by agitation. It is filtered, and, by cooling, a granular precipitate is obtained, which is easily washed, and insoluble even with the aid of heat in its mother-liquor. Dried at 100° C. (212° F.), it turns yellow. It contains, according to my analysis, 55·13 per cent. of Hg O.

Alloxantine, in contact with oxide of mercury, is converted into alloxane, which ultimately combines with the metallic oxide.

Alloxane, like the neutral amides, may then play the part of a feeble acid; but it differs from urea in that it does not appear to combine with the acids or metallic salts. Thus, I have not obtained alloxane with chloride of mercury, either by direct combination, or by dissolving the compound of alloxane and mercury in hydrochloric acid and evaporating. In the same way, I dissolved alloxane in hydrochloric acid, and I abandoned the liquor under a bell-glass with lime. I obtained rather confused crystals, not containing hydrochloric acid, very acid to the taste, and whose reactions resemble those of alloxanic acid, and are no longer those of alloxane. Is it alloxanic acid? I propose to determine this by further investigation.

Creatine and Creatinine.

With urine concentrated and precipitated by chloride of zinc, we obtain a combination of creatinine and chloride of zinc, which, purified by crystallisation, and decomposed by known methods, gives a mixture of creatine and creatinine. Now, creatine does not produce with chloride of zinc a sparingly soluble combination. On the other hand, I prepared directly the salt of zinc with pure creatinine; this combi-

nation decomposed in small quantity only reproduces creatinine, but decomposed in the large way it gives, like the salt of urine, a mixture of creatine and creatinine. Creatinine may then be hydrated and converted into creatine. I have studied the circumstances of this transformation.

I enclosed creatinine in two flasks—first, with water; second, with ammonia—the two liquids being in sufficient quantity for dissolving it. After six months, there were formed crystals different from creatinine, rare in the first flask, and more numerous in the second. These crystals effloresce at 100°C . (212°F .), have no alkaline reaction, and do not precipitate chloride of zinc; they have all the characters of creatine.

This transformation also takes place in presence of the neutral salts, and it is irregularly expedited by the action of heat. When we decompose with ammonia very pure hydrochlorate of creatinine, as for preparing this latter body, if the whole is left for a fortnight, the creatinine is mixed with a large quantity of creatine. In preparing creatine by the reaction of ammonia and hydrosulphate of ammonia on creatinine and chloride of zinc, we have as a residue, colored mother-liquors which, evaporated, produce only thin yellow plates, which are impure creatine, and which cannot be purified under this form. By diluting this mother-liquor with liquid ammonia and heating, this creatinine is converted into creatine which is colored, but which is easily rendered pure.

It was already known, that creatine extracted from urine by Liebig's process, was contained in it in the state of creatinine. From the foregoing, moreover, it was rendered probable that, by the concentration of large masses of urine neutralised as Liebig prescribes, a portion of the creatinine should be changed into creatine, and thus escape the crystallisation which the chloride of zinc determines. This is indeed what happens. To ascertain the maximum of creatinine which could be extracted from urine, by a properly modified process, I made the following experiment with the urine which I had used in my various preparations, and which was a mixture of the urines of the morning, passed in winter by young people of 15 to 18 years of age. 200 cubic centimetres of this urine, precipitated by lime and chloride of calcium, then filtered, were acidulated with hydrochloric acid, then evaporated, *in vacuo*, to the consistence of a thick syrup. Uric acid, and its salts were separated by filtration, and after neutralisation by means of a little ammonia, chloride of zinc was added. The crude combination of creatinine with chloride of zinc, washed and dried in the air, which was obtained, weighed 0.655 gr., which represents 327 grammes of this small crude salt for 100 litres of urine. Now, in 1856, the mean of my preparation, on the large scale, by Liebig's process unmodified, gave me only 143 grammes of zinc salt from 100 litres of urine. In 1857, on the contrary, in concentrating urine on the open fire, after having precipitated it with lime and chloride of calcium, then rendered acid, I obtained in the large way a mean of 266 grammes per 100 litres of urine.

The pre-existence of creatine in urine has not hitherto been demonstrated. I think that I may say as much of the muscular liquid, which probably contains only creatine. Indeed, in order to extract creatine by simple crystallisation, it must be rendered neutral, must be concentrated by long operation of heat, which must convert into creatine a large portion of the creatinine which pre-existed in it. The presence of creatine in the animal liquid, is therefore wanting in proof for the moment, owing to the want of a characteristic reaction.

Calves' urine, which is acid, contains as is known, a considerable quantity of creatinine. Does this alkaloid exist in the urine of the cow, which is alkaline? This urine, strongly acidulated with hydrochloric acid, then concentrated, was freed from its hippuric acid, and then treated with chloride of zinc. Ten litres produced about ten grammes of crude compound of creatinine with chloride of zinc. But as it is sufficient to heat creatine for a long time in an acid liquor to convert it into creatinine, this experiment cannot decide which of these two bodies pre-existed in the urine of the cow. I will say on this occasion, that it is sufficient to boil creatine with chloride of zinc, in even not very concentrated solution, to see a white powder of creatinine with chloride of zinc.

Hydrochlorate of creatinine combines also with chloride of zinc, and forms large crystals, which are very soluble and acid to the taste. To obtain them, it suffices to dissolve the compound of creatinine and chloride of zinc in hydrochloric acid, and to evaporate to the consistence of a syrup. I ascertained that they contained zinc, and I estimated the chlorine, which weighed 32.44 per 100 parts of salt. The formula $C^8 H^{14} N^6 O^2, H^2 Cl^2, + Cl^2 Zn$ requires 32.64 per cent. of chlorine.

Trimethylamine.

Calves' blood, about 12 hours after its extraction from the animal, and not presenting any apparent alteration, disengages the odor of the crab when agitated with an excess of milk of lime. This odor characterises trimethylamine in very dilute solution. I coagulated in a sand-bath 12 litres of calves' blood, previously acidulated with hydrochloric acid; I pressed the coagulum, mixed the liquor with a slight excess of lime, and distilled; the product of the distillation, received in water containing a little hydrochloric acid, was evaporated to dryness. It consists almost entirely of sal ammoniac, which smells like cod-fish. I redissolved it in absolute alcohol to eliminate this salt. The alcoholic solution evaporated, left a very slight deliquescent residue, disengaging with potassa a powerful odor of sea water; this residue, treated with chloride of platinum, gave me three kinds of crystals; the less soluble were chloro-platinate of ammonia; I afterwards obtained more soluble yellow bractææ, which, decomposed by an alkali, smell powerfully of snuff; and finally, larger, red and very soluble crystals, which I think were chloro-platinate of trimethylamine; they occur, like the last named salt, under the form of triangular tables, whose angles are truncated. I had but a few milligrammes of them.

Calves' blood, before any indication of putrefaction, contains then two volatile alkaloids. Nevertheless, these bodies appear to me to be only products of decomposition. Indeed, I have ascertained that the blood of the same animal, on issuing from the vessels, does not disengage any odor of crab by the addition of lime. There exists then in the blood, as in the urine, a body which is easily decomposed, and one of whose products is trimethylamine. Is it trimethylurea?

Journal de Pharmacie, July, 1857.

(To be continued.)

ON PYROGALLIC ACID. By M. ANTON ROSING, of Christiania.

PERFECTLY pure pyrogallal acid has no influence on litmus paper; but the commercial acid has most frequently an acid reaction, due to the presence of foreign bodies. It cannot be sublimed without decomposition, whatever may be the precautions employed; a portion decomposes and forms metagallic acid. This circumstance has prevented me from taking the vapor density of this substance; it is well known that dry pyrogallal acid does not alter under the influence of the air, that, on the contrary, its solution undergoes a very rapid alteration, becoming of a darker and darker tint, and at length depositing a brown, amorphous matter. This decomposition must not be confounded with that which pyrogallal acid undergoes under the influence of alkalies; for, although I have not yet examined the product, I have been able to convince myself that this alteration took place in air washed by sulphuric acid. One fact, well worthy of remark is, that a few drops of hydrochloric acid will prevent all coloration of pyrogallal solutions.

Hydrochloric acid diluted with water, and that which is concentrated, have no reaction on pyrogallal acid, whether cold, or at the boiling temperature.

On pouring monohydrated sulphuric acid on dry pyrogallal acid, the crystals assume a yellow color and gradually dissolve; if the solution is heated it assumes a black tint; with *fuming sulphuric acid*, pyrogallal acid gives a black solution. This is not simply a phenomenon of solution, for the liquid diluted with water, neutralised with carbonate of baryta and filtered, gives a yellow liquor which contains sulphuric acid and baryta; but as there are formed at the same time some products of decomposition, the *sulphopyrogallate* has not yet been obtained in a fit state for analysis.

Fuming nitric acid acts powerfully on pyrogallal acid; the mixture heats and assumes a brown color, with a disengagement of rutilant vapors; by degrees the color becomes paler, and by evaporation crystals of oxalic acid are formed. I am now occupied in studying the intermediate products.

Chlorine gas acts powerfully on dry pyrogallal acid, producing a black coloration and disengaging hydrochloric acid. Several experiments which I have undertaken have led me to conclude, that there is a series of chloruretted products formed which I have been, as yet,

unable to separate; the maximum of chlorine which I have hitherto been able to introduce has been 27 per cent.

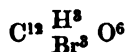
Dry iodine does not react on pyrogalllic acid, in ordinary circumstances; but at 200° C. (392° F.), in a tube sealed in the lamp and placed in an oil bath, it appeared to attack it.

When anhydrous bromine is poured on dry pyrogalllic acid a very powerful reaction is manifested, and hydrobromic acid gas is disengaged; if the excess of bromine is driven off there remains a heavy, yellowish mass, which, when heated on a platinum plate, lights, burns with a flame edged with green, and leaves a large quantity of charcoal.

Analysis gave:—

		I.	II.	Calculated.
C	=	20.022	19.772	20.00
H	=	0.860	0.845	0.80
Br	=	66.100		66.00
O	=	13.018		13.20
		100.000		100.00

which corresponds with the formula:—



This bromuretted product is almost insoluble in cold water; but if it is boiled with water it partly dissolves, and the remainder decomposes. It is soluble in alcohol and ether. The solution is brown, and possesses a very marked acid reaction. The alcoholic solution gives by spontaneous evaporation large magnificent crystals, of a pale brown, which appear to be oblique rhomboidal prisms: these crystals contain two equivalents of water. Hydrochloric and sulphuric acids do not appear to react on this bromuretted product; but concentrated nitric acid has a powerful action, and disengages rutilant vapors. With ammonia, a solution of the bromuretted product takes a very intense red coloration, which, under the influence of the air, becomes brown. Potassa and soda produce an analogous reaction.

With sulphate of protoxide of iron the solution of the bromuretted product takes a very rich blue coloration, which may be compared to Saxony blue or ammoniacal oxide of copper. The sensibility of this reagent is so great, that this substance may be substituted for ferrocyanide of potassium to ascertain the presence of protoxide of iron. The blue liquor may be left for a long time in contact with the air without undergoing any alteration, but at length the color becomes black, and it deposits a viscid mass which contains crystalline plates, insoluble in cold water, but soluble in alcohol.

If dry ammoniacal gas is put in contact with pyrogalllic acid out of the air, in a current of hydrogen, for instance, no reaction is manifested. When the excess of ammoniacal gas has been driven off by the hydrogen, about two per cent. is left; but this gas is gradually disengaged *in vacuo*, and the acid is found unaltered.

If an excess of ammonia is added to a solution of pyrogallie acid, there is produced, as is well known, under the influence of the air, an extremely dark brown coloration; after spontaneous evaporation an amorphous, black substance is left of a resinous appearance. This product possesses the following properties.

It disengages ammonia when heated with slaked lime, and also, although with difficulty, with a solution of potassa; it dissolves in water and alcohol, producing a brown liquor, which is so intense and so pure that it may be used like sepia. The solution is neutral to litmus paper, and gives precipitates with many of the metallic salts, &c.; such as acetate of lead, sulphate of copper, chloride of manganese, sulphate of iron, bichromate of potassa and lime water. The lead precipitate, at least, is a nitrogenous compound.

A great number of analyses and experiments have shown me that this product is of variable composition; for this reason I have not ventured to formulise it; but undoubtedly, the principal reaction consists in an oxidation with fixation of nitrogen, and at the same time, it appears to me, an elimination of hydrogen.

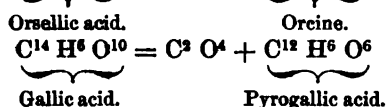
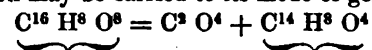
Many attempts at etherification with pyrogallie acid have given me negative results; contrary to what has been said, pyrogallie acid does not disengage carbonic acid from the carbonates of lime, ammonia, &c., nor from the bicarbonates; but, under the influence of the air, a solution of pyrogallie acid mixed with an alkaline carbonate, becomes slowly of a brown color.

Pyrogallie acid not only reduces the noble metals from their solutions, but likewise copper in the saccharimetrical liquor of Frommherz, with respect to which it behaves like glucose; it combines with various metallic oxides, whose powerful affinity for the oxygen of the air renders the preparation of them very difficult. It unites also with some organic substances, such as gelatine and caseine.

A mixture of pyrogallie and stearic acids when submitted for 36 hours to a temperature of 200° C. (392° F.), in a tube sealed in the lamp, gives a crystallised combination, from which we can nevertheless remove the excess of stearic acid.

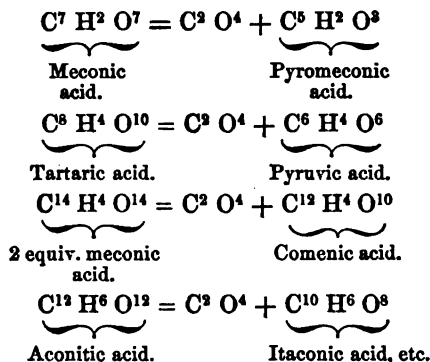
Although many of these attempts have hitherto been only introductory, I think that I may already deduce from a general conclusion, that pyrogallie acid is not a true acid; it approaches more nearly to orceine than to any other known body. It has the same rapid alterability, under the influence of air and the bases. In the same manner as orceine, it absorbs dry ammoniacal gas, but loses it again *in vacuo*, like it likewise, it gives with humid ammonia, under the influence of the air, a neutral nitrogenous compound, &c.

This comparison may be carried to its mode of generation, for—



It would perhaps be better to replace the word *pyrogallic acid* by that of *pyrogalline*, or simply *galline*. In this case the brown nitrogenous product might receive the name of galleine of pyrogalleine, the same as *orceine*.

But at the same time pyrogallic acid has some characters analogous to those of some pyrogenous acids, especially pyromeconic acid, and it would not be impossible to unite these bodies as a group. Thus we have—



These four pyrogenous acids, whose mode of production is analogous to that of pyrogallic acid, have several properties which are common to the latter, amongst others that of giving a red color to the salts of iron.

Comptes Rendus, No. 22, June 1st, 1857.

PREPARATION of PYROGALLIC ACID. By M. LIEBIG.

PYROGALLIC acid was discovered by Braconnot; being very reducible it will even easily remove oxygen from silver compounds, which is what has rendered it useful in photography. M. Liebig, who indicated this application, has now completed it by giving a process which is preferable to any other, both as to the yield and purity of the product.

This process consists, in principle, of the decomposition by heat of very highly dried crystallised gallic acid.

To succeed well with this reaction, the gallic acid is mixed with pumice stone and distilled in a current of carbonic acid gas.

The mixture should be placed in a tubulated retort, which should only be a quarter full, the retort is plunged up to the neck in a sand-bath. The tubulure receives a disengagement tube, which communicates with a source of carbonic acid gas. This tube goes deep into the retort and stops within a centimetre of the mixture.

The neck of the retort should be both very long and very spacious; it is put into a receiver, but in such a manner that the latter can be readily removed.

All these precautions are necessary, because, at an elevated temperature, dry gallic acid decomposes into water, and into pyrogallic, metagallic, melangallic, and carbonic acids.

Two atoms of gallic acid will produce two atoms of pyrogallic acid, one atom of metagallic acid, four atoms of carbonic acid and two atoms of water. According to this, 110 parts of dried gallic acid ought to give 39 of pyrogallic acid. Now, at a temperature near that at which gallic acid decomposes, pyrogallic itself is changed into water and metagallic acid; whence it results that the yield would be the greater, the more rapidly the pyrogallic acid is removed from the action of the heat. Another precaution which is indispensable to take, is to prevent the vapors of this acid from condensing in the upper part of the retort, for, in this case, the condensed product never fails, sooner or later, to fuse and consequently to return to the bottom of the retort, when its destruction would be certain. To obviate this inconvenience, the author causes a current of carbonic acid gas to intervene, which draws the pyrogallic vapors into the neck of the retort as soon as they are produced, at the same time that it prevents the steam from condensing and removing the crystals of pyrogallic acid which are formed.

In the receiver there condenses a syrupy liquor composed of an aqueous solution of pyrogallic acid, from which the latter may be separated by evaporation in the air. This solution proceeds from the simultaneous condensation of the steam and the vapors of pyrogallic acid.

When the operation is progressing, the neck of the retort is soon covered with elongated, flattened needles of a brilliant white, which may be removed with a feather. If the neck of the retort heats so as to attain the temperature at which pyrogallic acid fuses, this latter liquifies, and runs down the neck, and then becomes a solid mass which may be removed with a spatula.

Fused pyrogallic acid always assumes a persistent reddish tint, which cannot be removed even with charcoal.

By this process, from 31 to 32 per cent. of solid pyrogallic acid is obtained, 3 kilogrammes of dried gallic acid give a little less than a kilo. of pyrogallic acid; 8 or 9 per cent. is always lost in the carbonic acid, which it colors white. Doubtless, one part out of these 9 might be saved without much trouble by cooling the current of gas, or by passing it through water.

It would also be advantageous to perform the decomposition of the gallic acid in a bath of a constant temperature. This is not the case with a sand bath placed over a furnace. In this case the gaseous current has to be regulated according to the temperature, so that if drops of fused pyrogallic acid are deposited at the upper part of the retort, the fire must be augmented, and the disengagement of gas thus rendered more active.

Annalen der Chemie und Pharmacie.

INORGANIC CHEMISTRY.

On the REDUCTION of certain SALINE SOLUTIONS by ALUMINIUM.

By M. HENRI MASSON.

M. HENRI MASSON thinks, like M. Henri Deville, that aluminium should be ranked, by its properties, with iron and chromium. He makes known the result of his experiments on the reduction which aluminium produces in certain saline solutions, and especially in nitrate of silver, nitrate of protoxide of mercury, protochloride and cyanide of mercury, chloride of tin, acetate of lead, and the salts of copper; these various salts are almost all reduced by aluminium. The author indicates the conditions in which the experiment should be made, and the ulterior reactions which may supervene.

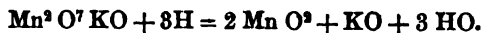
Comptes Rendus, No. 23, June 8th, 1857.

OBSERVATIONS on some CHEMICAL DECOMPOSITIONS by means of the BATTERY. By M. SCHLAGDENHAUFFEN.

(Concluded from page 609.)

III. DECOMPOSITION OF SOME OTHER METALLIC ACIDS AND THEIR SALTS.

1. HYPERMANGANIC acid is more easily reduced than chromic acid. It is sufficient to charge the porous vessel of a Bunsen's element with a solution of a hypermanganate of potassa, and to close the circuit, to remark at the end of an hour the transformation of the hypermanganic acid into peroxide deposited on the carbon. We must have



The interior vessel contains free potassa and the hypermanganic acid does not pass through the diaphragm.

The decomposition of this acid is more rapid when the liquor has been acidified by means of a little sulphuric acid. To decompose 4 grammes of hypermanganate dissolved in 50 c. of water, required only half an hour, whilst in the foregoing case it took more than two hours. Protoxide of manganese was deposited on the carbon pole; indeed,



the sulphate of protoxide of manganese is decomposed in its turn, and leaves MnO at the negative pole.

2. Molybdic acid undergoes the same transformation. I took 8.5 grs. of molybdate of ammonia dissolved in 50 c. of water, to charge the diaphragm of a Bunsen's element. The internal vessel contained sulphuric acid at a twentieth of the volume. After two hours the molybdate was changed into blue molybdic molybdate.

I repeated the same experiments with the tungstate and vanadate of ammonia, and I found that for the same weight of salt dissolved in

50 c. of water, two hours were required to produce the transformation into blue tungstate of tungstate, and vanadate of vanadium.

Finally, a solution of titanous acid gave me, by reduction, the beautiful violet liquor of titanate of titanium.

The transformations of these various acids under the influence of the battery and nascent hydrogen might easily have been foreseen, because we always use a plate of zinc in the presence of dilute sulphuric acid to make the qualitative analysis.

IV. DECOMPOSITION OF CHLORIC, BROMIC, AND IODIC ACIDS, AND THEIR SALTS.

These acids, as well as their salts, are decomposed under the influence of the nascent hydrogen of the battery.

1. In a first experiment I took three Bunsen's elements, of which two were charged as usual with nitric acid; into the third I had poured a dilute solution of chlorate of potassa. The circuit of the battery was closed. At the end of three hours I found that this salt was decomposed; the potassa had become free in the vessel which surrounded the charcoal.

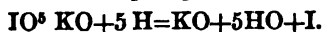
One portion of this liquor was treated with nitrate of silver, after saturation with pure nitric acid; the abundant precipitate of chloride of silver proves that the chloric acid had been reduced and changed, under the influence of the nascent hydrogen, into hydrochloric acid. One portion of the hydrochloric acid produced by this reduction of the chlorate, passed through the diaphragm towards the positive pole, whilst the nitrate of silver gave in the external vessel an abundant precipitate of chloride. The equation which gives an account of this experiment is consequently $\text{ClO}^6\text{KO} + 6\text{H} = \text{KO} + 5\text{HO} + \text{ClH}$.

2. Bromate of potassa decomposes in an analogous manner; at the end of four hours the liquor in the porous vessel becomes alkaline. A great deal of hydrobromic acid passes towards the positive pole. One singularity is observed here: when the bromic acid decomposes, there is a time at which the bromine becomes free; what proves this is the yellow coloration of the external liquor, a coloration which continues for about an hour and then disappears again. I ascertained the presence of free bromine by treating the liquor directly with starch. To obtain a more intense yellow coloration, I first removed the bromine with ether, then, after evaporating the latter, I treated it with dilute sulphuric acid and starch; the same yellow coloration. The bromine afterwards disappears in the positive vessel; it combines with the hydrogen. This fact appears quite natural when we consider that the hydrogen and bromine combine with less ease than the same gas with chlorine; it is therefore by virtue of this less affinity in these two first that bromine may remain free for some time during the reduction of a bromate.

3. Iodate of potassa does not behave like the preceding salts. Almost all the iodine is deposited in the interior of the porous cell. Only one hour was required for the complete reduction of the quan-

tity of iodate corresponding to the bromate and chlorate used in the above experiments.

The precipitation of the iodine is not surprising; it proves that the reduction of iodic acid is performed gradually by the fixation of the nascent hydrogen on oxygen, and that iodine, not being able to remain in solution in water, is precipitated.



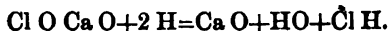
On the other hand, the affinity of hydrogen for iodine being less than that of the same gas for bromine and chlorine; we should expect not to find hydriodic acid, and scarcely any iodide in the external vessel.

The bromate of potassa is therefore more easily reduced than the iodate, and less easily than the chlorate; two equations will show the manner in which it is decomposed.



whereas the decomposition of the other two salts is explained by only one equation.

4. When we subject hypochlorous acid and free chlorine to the same experiment, we remark that these bodies soon combine with the hydrogen; after two or three hours a dilute solution of hypochlorite of lime no longer decolors vegetable colors. This proves well that there has been combination of hydrogen with oxygen and chlorine:—



Moreover, the line is free at the negative pole, and all the hypochloric acid has gone to the positive pole.

V. DECOMPOSITION OF SULPHUROUS, ARSENIOS, AND ARSENIC ACIDS AND THEIR SALTS.

By charging the diaphragm of a Bunsen's element with sulphurous acid, and by combining this element with three others charged with nitric acid, we obtain a very intense current, and such a disengagement of hydrogen that, at the end of an hour, the sulphurous acid is decomposed. The oxygen unites with the hydrogen, whereas the sulphur is deposited in a thick mass in the porous cell:—



Daniell had already made an experiment with sulphite of soda; the external vessel was charged a solution of sulphite; whereas in the present case the external vessel contained sulphuric acid. In this case the hydrogen is disengaged at the negative pole, and the oxygen in passing to the positive pole transforms the sulphurous acid into sulphuric acid. This nascent hydrogen of the negative pole not being sufficient to decompose the sulphurous acid, Daniell did not find a deposit of sulphur.

The reduction of the sulphurous acid is not confined to this deposit

of sulphur, as is indicated by the formula; there is, moreover, a combination of hydrogen with the sulphur, and consequently a formation of sulphuretted hydrogen which remains in solution in the liquor.

If, instead of using sulphurous acid, we take an alkaline sulphite, we obtain only a very slight deposit of sulphur. This small quantity appears to proceed from the decomposition of the sulphurous acid, which becomes free when the sulphuric acid of the positive vessel infiltrate the diaphragm and reacts on the sulphite. The greater part of the sulphurous acid of the salt passes through the vessel to go to the positive pole without being decomposed.

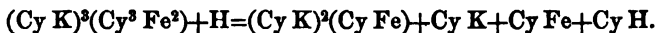
2. Arsenious acid is reduced still more easily than sulphurous acid. The arsenic is deposited round the charcoal, under the form of a black powder. Half an hour is sufficient to decompose a concentrated solution of this acid put into the diaphragm of a Bunsen's battery.

When, instead of taking the acid alone, we take an arsenite acidulated with sulphuric acid, we obtain the same result. Arsenite of potassa, without any addition of sulphuric acid, takes much more time in decomposing. In this last case, one portion of the acid filters through the diaphragm towards the positive pole; in the two preceding cases, on the contrary, the hydrogen decomposes all the acid in the negative vessel.

We may thus decompose arsenic acid or an arseniate acidulated with sulphuric acid so as to obtain metallic arsenic at the charcoal pole. With four elements the current was too weak to decompose arseniate of soda without the addition of sulphuric acid.

V. DECOMPOSITION OF THE RED CYANIDE.

A cell of the battery was charged with 5 grammes of the red cyanide dissolved in 50 c. c. of water. The other three diaphragms were charged with nitric acid; after having established the contacts, the circuit was closed. At the end of three hours I found that the red cyanide was decomposed. On distilling the liquor contained in the diaphragm I collected a very considerable quantity of hydrocyanic acid. There remained a green precipitate in the retort. After filtering the precipitate a yellow liquor passed which was yellow cyanide, for it gave with the ferrous salts a white precipitate which became blue, and with the ferric salts a precipitate of Prussian blue. This decomposition may be explained in the following manner by the formula:—



It might be expected that the red cyanide would, under the influence of nascent hydrogen be changed into yellow cyanide, since this latter passes to the state of red cyanide with oxidising agents, such as chlorine.

The experiments which I have just described prove that the nascent hydrogen of the battery acts in various ways on oxidised bodies. In the decomposition of the nitrates there is a formation of water, and, moreover, a combination of the hydrogen with the nascent nitrogen;

that is to say, a production of ammonia. When we subject the chromates, manganates, molybdates, and other salts, with metallic acids, to the influence of reducing bodies under the influence of the battery, we find that there is a transformation of their oxidised compounds into inferior oxides; for example, the reduction of chromic acid into oxide of chrome, of hypermanganic acid into peroxide of manganese. In other cases the oxygen of the acid subjected to decomposition is entirely burned by the oxygen, and the metalloid now combining readily with this latter is deposited at the negative pole; this is observed in the reduction of iodic, sulphurous, arsenious, and arsenic acids.

Finally, nitrobenzine and nitrophtaline subjected to this reducing agent, and to the action of the battery, are decomposed into aniline and naphthalidam. These reductions are performed in the same manner as that produced with sulphuretted hydrogen, sulphuret of ammonia, or the protosalts of iron.

I am continuing this kind of investigations, and hope soon to be able to describe some new results. I am attending especially to the decomposition of the nitrated bodies, and principally to that of the nitrated acids containing two or three equivalents of NO^4 , with the view of producing the corresponding compounds of ammonia.

Journal de Pharmacie et de Chemie, June, 1857.

INDUSTRIAL CHEMISTRY.

NOTE on the THEORY of CEMENTS. By the Marshall VAILLANT.

THE President of the Academy has been kind enough to communicate to me a note* addressed to the Academy by M. Vicat, correspondent of the institution, and in which are discussed, criticised and condemned these propositions enunciated by MM. Rivot and Chatoney, in their memoir *On the Materials employed in Marine Constructions*, (see THE CHEMIST, October, 1856, p. 40; and December, 1856, p. 163,) of which memoir I was charged to present an analysis to the Academy, and which it ordered to be inserted in the *Recueil des Travaux des Savants Etrangers*.

M. Rivot has asserted that pure cements, especially those which set slowly, should as much as possible be employed in chinks, and that they thus acquire more compactness than when they are mixed at the ordinary consistence.

M. Vicat denies this superiority of plashing with an excess of water, and he opposed to the assertion of MM. Rivot and Chatoney comparative experiments which he has made, of which he gives an analysis and the results, and which have led him to conclude that cements, whether setting slowly or quickly, plashed with an excess of water and employed in chinks, have less density, homogeneity and hardness than the same cements plashed firm.

* See the next Article.

In another part of their memoir, speaking of artificial pozzolanes, MM. Rivot and Chatoney expressed the opinion that the argils baked or not baked, cannot, in general, act like good pozzolanes, because the action exerted on them by lime in the presence of water is slow and partial, and must, consequently, give rise to molecular motions, the causes of disaggregation for mortar. On the contrary, they have advanced that powdered silex must be considered as a good pozzolane, provided that the mortar be submitted to a long previous digestion.

M. Vicat disputes this opinion; the experiments which he has made have led him to a diametrically opposite conclusion; according to him, the pure argils, and even certain ochrey argils, are, after slight baking, excellent pozzolanes, whilst, among known hydraulic compounds, there is not one which does not give incomparably better results than porphyrised silex.

Finally, M. Vicat reproaches MM. Rivot and Chatoney with having committed an error, in attributing to the Romans the habitual practice of a long digestion preparatory to the employment of the mortar. This process, which MM. Rivot and Chatoney recommend, was unknown at Rome, according to M. Vicat. He affirms besides, that the Romans made hydraulic constructions badly, and he concludes from this, that it is not to praise the efficacy of previous digestions, but to sustain that they made use of them.

Such are the contradictions set up by M. Vicat against MM. Rivot and Chatoney. The first two are serious, and the authority of the disputant will, doubtless, add great weight to them.

I join with M. Vicat in requesting the insertion of his note in the *Comptes Rendus*. It appears to me right that the points in dispute should be discussed openly. If MM. Rivot and Chatoney have committed errors, these must be rectified. If they are correct, proofs will not be wanting.

Come what may, the art of hydraulic constructions will only have gained by the discussion.

Comptes Rendus, No. 21, May 25th, 1857.

EXAMINATION of some PROPOSITIONS enunciated by MM. RIVOT and CHATONEY, in their MEMOIR on the MATERIALS EMPLOYED in MARINE CONSTRUCTIONS. By M. VICAT.

It is not long since I have been able to read this memoir in its entirety, and thus to form a much more accurate idea of it, than can be arrived at from reading the extract inserted in the *Comptes Rendus*. (See THE CHEMIST, October, 1856, p. 40; and December, 1856, p. 163.) The very learned considerations which MM. Rivot and Chatoney have developed in it, as to the manner in which they presume that the solidification of hydraulic compounds should be operated, and as to the consequences which they deduced from them, as to the manner of preparing the materials of them by what they call previous digestions, are at present only in the state of theories; we must therefore wait

until experience justifies or condemns them; but it is not so with certain propositions, which have no less a tendency than to upset completely that which we have professed for 30 years, without observation or practice having contradicted us. We find ourselves, therefore, not only in what is called a case of legitimate defence, but also under the obligation of maintaining sound doctrines. We will quote as we discuss them, the passages of the memoir which, at first sight, contain propositions the most contrary to the evidence of the best established facts. We commence with the plashing of cements:—

“We have hitherto supposed, say the authors, that cements were plashed with the quantity of water, simply sufficient for obtaining a consistence for use; but whenever it is possible it is well to employ the pure cement in the form of grout, that is to say, with a great excess of water; in solidifying, it rejects the water not required for hydration, and its texture is much more compact than if it had been plashed to the ordinary consistence; it might be said that, left to themselves in a more liquid medium, the molecules assimilate better; they are also better moistened and carry less air with them. For this two-fold reason, the mortars are less porous.”

In reading this new doctrine on the plashing of cements, we asked ourselves how it could be that the increase of volume, which is the necessary consequence of the employment of a great excess of water, not only for cements in general, but also for plaster and argil in the arts of the plasterer, potter and brick-maker, could aid in giving greater density to these matters, on the completion of their setting? Thousands of common, old and daily recurrent facts answer most expressly in the negative, and yet, in the face of the scientific position of the honorable engineers whom we combat, it is our duty to examine whether there is not in all this something misunderstood, or some of those easy mistakes into which every experimenter may at one time or other fall.

We therefore decided on recommencing new experiments, operating in the first instance on cements which set rapidly, and afterwards on those which set slowly; for that purpose, we procured some glass tubes of equal diameter (four to five centimetres) closed at one end with common corks, and we introduced into each of them cements of Grenoble, Paris, Vassy, and Valentine, plashed on one hand with 50 parts of water to 100 parts of cement (case of maximum of consistence), and on the other hand with 120 parts of water to the same quantity of cement (forming a much lighter mass than the ordinary grouts). Now the tubes being all of equal diameter, the volumes of the cements contained in them should be proportional to the heights which they occupied.

Results obtained with Quick-Setting Cements.

By taking the customary precautions for avoiding honey-combs, by introducing the cements plashed firm into the tubes and by agitating on the other hand, with a small glass rod, until the commencement of thickening, the fluid masses contained in them, we were enabled to

form with these columns twice the weight of the cements plashed firm.

By afterwards comparing with equal volumes the hardness, weight, and capacity of imbibition acquired after two months* by the cements disengaged from the tubes by carefully treating the latter, we arrived at the following mean results expressed in proportions one of the terms of which is taken as unity:—

	Hardness.	Weight.	Capacity Imbibition.
For cements plashed firm after } natural dessication in the air }	1,000	1,000	1,000
For the same cement plashed } with an excess of water in } the same conditions . }	0,075	0,376	2,570

These figures require no comment; it is easy to see that between these limits of 50 and 120 parts of water by weight to 100 parts of cements, similar to those which are designated *quick-setting cements* in constructions, the volumes, degrees of hardness, weights and degrees, permeability measured by capacity of imbibition, would pass through all the degrees comprised between the unity and the corresponding numbers of the second line.

MM. Rivot and Chatoney having attributed to the facility which the particles of the cements have of moving and arranging themselves at their ease in an excess of water, which is afterwards rejected, the more compact texture which under this hypothesis they assign to pure cements thus treated, we considered it necessary to follow them into this question, and to turn our experiments into this direction, by allowing the fluid grouts to precipitate to the bottom of the tubes, instead of agitating them until the commencement of thickening. It resulted, indeed, that a considerable quantity of water floated, and that the volumes of the grouts precipitated did not amount, as before, to double that of the cements plashed firm. But these volumes did not descend to equality; the proportions found varied from 1 to 1.36 and 1.46, and all homogeneity in point of hardness and density disappeared from their mass. The results, in the circumstances above specified, were found as follows:—

	Hardness measured by the Foret.		
1. Cement of Valentine plashed to good consistence	{	Above the cylinder	21
		Below	21
2. The same precipitated spontaneously from a fluid grout	{	Above	00
		Below	5
3. Cement of Grenoble plashed to a good consistence	{	Above	33
		Below	33

* The cements remained one month under water in their tubes, and then another month in the air after being disengaged from their tubes; some of the grouts were so light after dessication, that in placing them in water they floated for two or three seconds, after which the imbibition caused them to sink. It was necessary to employ tubes of very thin glass, and to break them with great care to obtain the grout cements entire, and we did not always succeed.

Hardness measured by the Foret.

4. The same cement precipitated spon- { Above the cylinder 14
taneously from a fluid grout { Below " 9

The allegation of MM. Rivot and Chatoney, as to the useful effect of a great excess of water in the plashing of cements is then, as regards those which are called quick-setting, directly contrary to what occurs, in whatever way they may be manipulated, and that with differences in hardness so great that not the least uncertainty can remain.

Inspector-General Reibell had already made this remark at Cherbourg, in 1852, on the old cement of Boulogne; he complained loudly of it, and wrote to us, on the 28th of March and on the 14th of April of the same year, that this cement employed pure in grouts between the stones of his blocks, did not become hard; he sent us at the same time a box of it, to try it contradictorily, and the following are the results which we obtained after 90 days' immersion, viz. :—

	Tenacity per square centimeter.
For 100 parts of cement plashed to a strong consistency with 50 parts of water	kil. 8.20
For the same quantity plashed with 57 parts of water	6.45
For the same quantity plashed with 80 parts of water	3.75

Beyond this last quantity of water, the tenacity was too feeble to be appreciated. These results were found to be in accordance with those obtained at Cherbourg.

Results obtained with Slow-Setting Cements.

Before entering upon these cements, we may remark that, if we operated on an ordinary inert sand, in grains of equal size and weight, it would be perfectly indifferent as regards the volume, density, and weight of the mass tempered, whether we gave it at once all the water necessary for filling the pores, or caused it to take it up either by agitation, or by precipitating it through a more or less abundant quantity of liquid. It would be sufficient to shake a little the vessel containing the whole, for the grains of sand in being heaped up to range themselves in the same order, occupying the same spaces, after having rejected the water which their spaces could not contain; there would also be homogeneousness in all points of the mass.

But all these points being supposed to remain the same for the volumes,* there would no longer be homogeneousness if the sand was composed of grains of all sizes, even to the fineness of dust. We comprehend then, that, if we agitate the mass in an excess of water, the heaviest grains will go to the bottom, whilst on the contrary, they will remain intermixed with the lighter ones, if the quantity of water be regulated in such a manner as to prevent all precipitation.

Well, that which would occur in this last case is very nearly the

* It might also happen that the volumes might no longer be equal, the spaces between the large grains being no longer filled by the finer.

same as takes place in the first instance when, instead of sand, we employ a slow-setting cement (two or three hours) such as we find in commerce. We will give an example:—

We submitted a Portland cement, of this kind, to the same proofs as ordinary cements; this cement, plashed to a good consistence of mortar, was moulded into its tube, allowing a layer of water of scarcely 1 millimetre in depth to float. But with the same quantity of cement, and a great excess of water, and after two minutes' agitation in a second tube, the coarse and heavier parts were precipitated, leaving the others suspended in the order of their fineness and lightness. The precipitation of the latter was gradually finished, leaving a considerable quantity of water floating on the top. The same quantity of cement was afterwards projected by small portions, into a third tube, two-thirds full of water, leaving it to precipitate naturally through the liquid column, without the least agitation. The three tubes thus charged were left to themselves for three or four days, after which the heights of the columns of cement contained in them were found to be as follows:—

	C.	Simple Proportions.
For that plashed to a good consistence . .	13.50	1.00
For that agitated with excess of water . .	18.66	1.38
For that precipitated through water . .	16.80	1.25

The volumes differed little, as we see, from those which we found in analogous cases with quick cements.

The tubes having been broken after an immersion of one month, and the hardness of the cements contained in them measured immediately by the foret, we found:*

1. Plashed to a good consistence	{ Upper part of the cylinder	80
	{ Base of the cylinder	80
2. Plashing with large quantity of water	{ Upper part of the cylinder	8
	{ Base of the cylinder	28
3. Cement precipitated through water	{ Upper part of the cylinder	10
	{ Base of the cylinder	54

The degrees of permeability followed the order of the volumes, and in neither of the last two cases did the maximum density amount to the uniform density of the cement plashed from. These results were easy to foresee; every one may repeat them without having a laboratory at his disposal, and will arrive, if not at identically the same figures, at least at proportions following the same direction.

We have to learn how such striking facts can have escaped the sagacity of the learned authors whom we are combating, to such an extent as to make them believe that, when left to themselves in a liquid medium, the particles of pure cements approach each other

* The figures express the number of turns which a steel rod cut in biseau, and charged with an uniform weight must make to penetrate 6 millimetres into the cement.

better than when we compel them to do so by a good ordinary manipulation.

It would be in vain to object that if we had employed cements whose particles were exactly equal in weight and size, things would have occurred differently; that is more than doubtful, since in this case even the lower layers of the precipitates are always more or less compressed by the weight of the upper layers; but this hypothetical state of cements does not exist, and cannot be realised in commerce; we must take the materials as they necessarily exist, and not as they ought to be to satisfy theoretical conceptions.

The entire importance in marine constructions of the part which the quantity of water employed in plashing cements play will be appreciated when it is known that such among them as, by manipulation at a strong consistence, may resist the action of the sea water for ten months, when plashed in grouts, yielded in ten days. A mass of cement, of unequal density, would therefore be attacked first at the lightest and most permeable parts, long before the preservative crusts which the sea deposits could cover them.

Comptes Rendus, No. 21, May 25, 1857.

(To be continued).

On M. CHEVREUL'S LAWS of COLOR.*

By F. CRACE CALVERT, ESQ., F.C.S., M.R.A., Turin,

Honorary Professor of Chemistry, Royal Institution, Manchester.

MR. CRACE CALVERT stated that he had three objects in view in this discourse. The first was to make known the laws of colors, as discovered by his learned master, M. Chevreul; secondly, to explain their importance in a scientific point of view; and, thirdly, their value to arts and manufactures.

To understand the laws of colors, it is necessary to know the composition of light; Newton was the first person who gave to the world any statement relative to the components of light, which he said consisted of seven colors—red, orange, yellow, green, blue, indigo, and violet. It is now distinctly proved that four of those seven colors of the spectrum, are the result of the combinations of the three colors, now known as the primitive colors, viz., red, blue, and yellow. Thus blue and red combined produce purple or indigo; blue and yellow, green; while red and yellow, produce orange: these facts being known, it is easy to prove that there are not seven, but three primitive, and four secondary, called complementary colors.

Several proofs can be given that light is composed of three colors only. One of the most simple consists in placing pieces of blue, red, and yellow papers on a circular disc, and rotating it rapidly; the effect to the eye being to produce a disc of white light. If, therefore, the eye can be deceived so readily, while the disc travels at so slow a rate,

* Read before the Royal Institution of Great Britain.

what must necessarily be the case when it is remembered that light proceeds at the rate of 190,000 miles per second?

The rapidity with which light travels is such, that the eye is not able to perceive either the blue, red, or yellow, the nerves of the retina not being sensitive enough to receive and convey successively to the mind the three or seven colors of which the light is composed.

Before entering into the laws of color, Mr. Crace Calvert stated, that it might be interesting to know what scientific minds had devoted attention to the laws of colors.

Buffon followed Newton, and his researches had special reference to what M. Chevreul had called, the "successive contrasts" of colors.

Father Scherffer, a monk, also wrote on the laws of color. Goëthe, the poet, also brought his mind to bear upon the subject, and studied it to a great extent. Count Rumford, about the end of the eighteenth century, published several memoirs on the laws of colors. He explained, very satisfactorily, the "successive" contrast, and arrived at some insight into the "simultaneous" one; still he did not lay down its real laws.

Prieur, Leblanc, Harris, and Field, were also writers of most interesting works on this subject. The reason that they did not arrive at the definite laws of color, was because they had not divided those laws into successive, simultaneous, and mixed contrasts. These form the basis of the practical laws of color, and the honor of their discovery is due to M. Chevreul.

The reason why a surface appears white or brilliant is, that a large portion of the light which falls on its surface is reflected on the retina, and in such a quantity as gives to the surface a brilliant aspect; whilst in plain white surfaces, the rays of light being diffused in all directions, and a small portion only arriving to the eye, the surface does not appear brilliant. The influence of colors on these two kinds of surfaces is very different, as may be perceived by the examples round the room, showing the influence of different colors on gold ornaments. When rays of light, instead of being reflected, are absorbed by a surface or substance, it appears black; therefore white and black are not colors, as they are due to the reflection or absorption of undecomposed light. It is easy to understand why a surface appears blue; it is due to the property which the surface has to reflect only blue rays, whilst it absorbs the yellow and red rays; and if a certain portion of light is reflected with one of the colored rays, it will decrease its intensity; thus red rays with white ones produce pink. On the contrary, if a quantity of undecomposed light is absorbed, black is produced, which, by tarnishing the color and making it appear darker, generates dark reds, blues, or yellows. The secondary colors are produced by one of the primitive colors being absorbed and the two others reflected; for example, if red be absorbed, and blue and yellow reflected, the surface appears green. There are two reasons why a perfect blue, yellow, red, cannot be seen, &c. The

first is, that surfaces cannot entirely absorb one or two rays and reflect the others. The second is, that when the retina receives the impression of one color, immediately its complementary color is generated; thus, if a blue circle is placed on a perfectly grey surface, an orange hue will be perceived round it; if an orange circle, round it will be noticed a bluish tint; if a red circle, a green; if a greenish yellow circle, a violet; if an orange yellow circle, an indigo; and so on.

The "successive" contrast has long been known; and it consists in the fact, that on looking stedfastly for a few minutes on a red surface fixed on a white sheet of paper, and then carrying the eye to another white sheet, there will be perceived on it not a red, but a *green* one; if green, *red*; if purple, *yellow*; if blue, *orange*.

The "simultaneous" contrast is the most interesting and useful to be acquainted with. When two colored surfaces are in juxtaposition they mutually influence each other,—favorably, if harmonising colors, or in a contrary manner if discordant; and in such proportion in either case as to be in exact ratio with the quantity of complementary color which is generated in the eye: for example, if two half-sheets of plain tinted paper, one dark green, the other of a brilliant red, are placed side by side on a grey piece of cloth, the colors will be mutually improved in consequence of the green generated by the red surface, adding itself to the green of the juxtaposed surface, thus increasing its intensity, the green, in its turn, augmenting the beauty of the red. This effect can easily be appreciated if two other pieces of paper of the same colors are placed at a short distance from the corresponding influenced ones, as below:—

Red. Red Green. Green.

It is not sufficient merely to place complementary colors side by side to produce harmony of color, since the respective intensities have a most decided influence: thus pink and light green agree, red and dark green also; but light green and dark red, pink and dark green, do not; and thus to obtain the maximum of effect and perfect harmony the following colors must be placed side by side, taking into account their exact intensity of shade and tint.

HARMONISING COLORS.

Primitive Colors.	Complementary Colors.	
Red . . .	Green . . .	{ Light blue Yellow Red } White light.
Blue . . .	Orange . . .	{ Red Yellow Blue } White light.
Yellow-orange .	Indigo . . .	{ Blue Red Yellow } White light.

Primitive Colors.		Complementary Colors.	
Greenish Yellow.	Violet . . .	{ Red Blue Yellow }	White light.
Black. . . .	White . . .	{ Yellow Blue Red }	White light.

If attention is not paid to the arrangement of colors according to the above diagram, instead of their mutually improving each other, they will, on the contrary, lose in beauty; thus if blue and purple are placed side by side, the blue throwing its complementary color, orange, upon the purple, will give it a faded appearance; and the blue, receiving the orange yellow of the purple, will assume a greenish tinge. The same may be said of yellow and red, if placed in juxtaposition. The red, by throwing its complementary color, green, on the yellow, communicates to it a greenish tinge; the yellow, by throwing its purple hue, imparts to the red a disagreeable purple appearance. The very great importance of these principles to every one who intends to display or arrange colored goods or fabrics was convincingly shown by Mr. Crace Calvert, from a great variety of embroidered silks (kindly lent by Mr. Henry Houldsworth), calicos, and paper-hangings, which demonstrated, that if these laws are neglected, not only will the labor and talent expended by the manufacturer to produce on a given piece of goods the greatest effect possible, be neutralised, but perhaps lost. It was clearly demonstrated that these effects are not only produced by highly colored surfaces, but also by those whose colors are exceedingly pale, as, for example, light greens, or light blues with buffs, and that even in grey surfaces, as pencil drawings, the contrast of tone between two shades was distinctly visible. The contrast of tone or tint was most marked when two tints of the same color were juxtaposed, and it was, therefore, the interest of an artist to pay attention to this principle when employing two tints of the same scale of color. From the "mixed contrast" arises the rule that a brilliant color should never be looked at for any length of time, if its true tint or brilliancy is to be appreciated; for if a piece of red cloth is looked at for a few minutes, green, its complementary color, is generated in the eye, and adding itself to a portion of the red, produces black, which tarnishes the beauty of the red. This contrast explains, too, why the tone of a color is modified, either favorably or otherwise, according to the color which the eye has previously looked at. Favorably, when, for instance, the eye first looks to a yellow surface, and then to a purple one; and unfavorably, when it looks at a blue, and then at a purple.

Mr. Crace Calvert also showed that black and white surfaces assume different hues according to the colors placed in juxtaposition with them; for example, black acquires an orange or purple tint if the colors placed beside it are blue or orange; but these effects can be overcome, in the case of these or any colors, by giving to the influ-

enced color a tint similar to that influencing it. Thus, to prevent black becoming orange by its contact with blue, it is merely necessary that the black should be blued, and in such proportion that the amount of blue will neutralise the orange thrown on it by influence, thus producing black. As an instance, to prevent a grey design acquiring a pinkish shade through working it with green, give the grey a greenish hue, which, by neutralising the pink, will generate white light, and thus preserve the grey.

Mr. Crace Calvert, after explaining the chromatic table of M. Chevreul, which enabled any person at a glance to ascertain what was the complementary color of any of the 13,480 colors which M. Chevreul had distinctly classed in his table, stated that it was of the highest importance to artists to be acquainted with these laws, in order to know at once the exact color, shade, and tint, which would produce the greatest effect when placed beside another color, and that they could save the great length of time which no doubt the great masters lost in ascertaining by experiment those laws, which they could now learn in a few hours by consulting M. Chevreul's work.

AGRICULTURAL CHEMISTRY.

On the INFLUENCE which the ASSIMILABLE NITROGEN of MANURES exerts on the PRODUCTION of VEGETABLE MATTER.

By M. BOUSSINGAULT.

In a former Memoir (see THE CHEMIST, New Series, vol. iii. p. 219), I showed how the nitrates aided vegetation. In the same meteoric conditions, in soils of the same nature, sunflowers grown with nitrate of potassa obtained a considerable development; they elaborated six decigrammes of albumen in producing 108 times as much vegetable matter as the seed contained. In the absence of nitre, on the contrary, when the nitrogenous principles assimilable from the atmosphere were alone assailable, the growth of the plant was much less; after three months' cultivation it had scarcely formed three centigrammes of albumen, and the dry sunflowers weighed only three or four times as much as the seed.

The experiments on cress led to analogous and perhaps more certain results, because, in our comparative experiments, the plants had both at their disposal, in the ash of manure added, beyond what they could absorb of mineral substances. But was it the same with the sunflowers? We ought to ask, for example, whether, in consequence of the rapidity of the growth, that which had the nitrate had really met, in the soil, with a sufficiency of phosphate of lime; and even admitting that it were so, we should still be in the right to maintain that the development of the sunflower grown without nitrate would have been greater; that the carbon, nitrogen, and elements of water would have been assimilated in larger proportions if the plant had found in the soil as much potassa as the saltpetre had given to the sunflower which had been cultivated as a parallel.

To set all doubts at rest, I have undertaken a fresh series of experiments. I expected, moreover, to see reproduced certain facts which had appeared unexpectedly in my former works. I would speak of the decisive action of the assimilable nitrogenous matters in the formation of the organs and proximate principles of the vegetables, so evident in action, that the weight of the organism elaborated by a plant gives in some sort the measure of the nitrogenous manure of which it has disposed. This is so true that a seed, so small that the albumen in it may be considered as imponderable, as the *mimulus speciosa*, tobacco, &c., produces in a sterile soil a plant whose development will not advance beyond the appearance of the primordeal leaves, and which will preserve this imperfect form for three entire months, waiting for the manure indispensable to constitute the nitrogenous tissue, without which it cannot grow, because the functions cannot be performed. It was this stationary state, this persistent germination, which I observed for the first time in 1854, in several seeds whose weight was comprised between $\frac{1}{17}$ th and $\frac{1}{8}$ th of a milligramme.

I found, moreover, that extremely light seeds, weighing from two to three milligrammes, such as cress, &c., produce, when sown in an absolutely sterile soil, small delicate plants, but still possessing complete organs; but then, as that has always been the case in all my experiments without any exception, after several months of existence in the open air, and, of course, still more in a confined atmosphere, the plant does not weigh much more than the seed from which it grew, as if the extension of its organism was limited by the quantity of nitrogenous principles contained in the seed.

Thus, there are seeds which contain in themselves sufficient of the nitrogenous element to produce, without any manure, a plant which is excessively reduced in all its dimensions, but perfectly organised, which I have called a *limited plant*, because it represents the vegetable formed of the smallest quantity of matter; we find in it about the same quantity of nitrogen as in the seed, and diminutive as it is, it flourishes and bears fruit which only requires a fertile soil to produce the normal plant.

The experiments which I have just described were intended in the first instance to ascertain the action of phosphate of lime on vegetation, with and without the concurrence of saltpetre.

I have followed the development of the *helianthus argophyllus* in the open air, sheltered from the rain, in a soil formed of burnt clay broken up, and quartzose sand. These matters, as well as the flower-pot containing them, had been calcined after having been washed in distilled water. Three experiments were prepared, A, B, C.

In experiment A, nothing was introduced into the soil.

In experiment B, were incorporated with the burnt clay and sand, basic phosphate of lime, vegetable ash, and nitrate of potassa.

In experiment C, the soil received phosphate of lime, vegetable ash, and a quantity of bicarbonate of potassa containing precisely the same quantity of alkali as the nitrate used in experiment B.

The phosphate of lime was extracted from calcined bones, using as

pure agents as possible because of the presence of magnesia; notwithstanding this precaution, the phosphate, precipitated by potassa, was not free from nitrogen, 2.445 grs. of the basic salt containing 0.00022 gr. in the state of ammoniaco-magnesian phosphate. The phosphate was always put into the soil in the gelatinous state, as collected on a filter after washing.

The *bicarbonate of potassa* was prepared with very pure carbonate.

The *vegetable ash* proceeded from the combustion of hay; it was white, very rich in silica, and without a trace of cyanides.

The plants grew in the open air, at one metre above the turf, near a vine planted on the border of a large forest.

The water used for irrigation was free from ammonia, and contained about the fourth of its volume of carbonic acid gas.

	Gr.
The pots weighed on an average	600
The crushed clay	400
The quartzose sand	1026

Earthy matters used in each experiment	2026
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Experiment A.—*Vegetation in a soil containing nothing but burnt clay and sand.*

Two *helianthus* seeds, weighing 0.107 gr. were planted on the 5th July.

20th August.—The first normal leaves had withered.

	Length.	Width.	
2nd normal leaves	25 millim.	10 millim.	
3rd normal leaves	15 „	8 „	of a pale green.

Height of the plants, 11 centimetres; diameter of the stalks, 2 millimetres.

20th September.—The second normal leaves had withered.

	Length.	Width.	
3rd normal leaves	18 millim.	5 millim.	
4th normal leaves	7 „	3 „	of a pale green.

Signs of a flower-bud; height of the stalks, 11 and 18 centimetres.

30th September.—The appearance of the plants has not changed since the 20th. The bud has expanded into a small yellow flower, the corolla of which is not more than three millimetres in diameter. This flower in miniature is surrounded with several nascent leaves.

We had obtained a limited plant.

	Gr.
The dried plants weighed	0.392
The dried seeds had weighed	0.107

Organic matter developed	0.285
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It was observed that the plants were very strong up to the 20th of August. From that time the older leaves died off as others appeared, and the vigor of the vegetation gradually declined until the flowering.

Analysis gave for the entire dried plant :—

	Gr.
Nitrogen	0·0084
In the soil	0·0020
	0·0054
In the seeds	0·0081
Nitrogen acquired in 8 months' vegetation in open air	0·0028

Determination of the Carbon fixed during the Vegetation.—

The matter organised during the vegetation weighed 0·285 gr. According to the analysis made on plants grown in the same conditions, it contained at the degree of dessiccation to which it was brought, 0·40 of carbon, or 0·114 gr. This carbon, which could only come from the carbonic acid, represents 0·418 gr., or 211 cubic centimetres of carbonic acid gas.

As the vegetation continued 86 days, we arrive at the conclusion that, on an average, the *helianthus* appropriated the carbon of 2·45 c.c. of carbonic acid gas each day.

Experiment II.—Vegetation of the *helianthus* under the influence of phosphate of lime, ashes, and saltpetre.

The soil was formed exactly in weight and nature as in the previous experiment. There was put into it :—

	Gr.	
Phosphate of lime	10·0	
Ash	0·5	
Nitrate of potassa added		Gr.
successively	1·4 containing	{ Assimilable nit. 0·1969
		{ Potassa 0·6525

On the 5th July, two seeds of *helianthus*, weighing 0·107 gr., were planted in the soil, properly moistened.

20th August.—The first leaves were withered.

	Length.	Width.	
2nd normal leaves	90 millim.	60 millim.	
3rd normal leaves	95 „	70 „	
4th normal leaves	95 „	70 „	of a fine green.
5th normal leaves	well developed.		

Height of the plants, 25 and 30 centimetres ; diameter of the stalks, 8 millimetres.

10th September.—Height of the plants, 49 and 59 centimetres ; diameter of the stalks, 9 millimetres.

20th September.—Height of the plants, 64 and 74 centimetres ; diameter of the stalks, 1 centimetre.

30th September.—Height of the plants, 64 and 74 centimetres ; diameter of the stalks, 1 centimetre. The largest sunflower had a beautiful yellow flower, the diameter of whose corolla was 9 centimetres. Since the 20th August the appearance of the leaves had not apparently altered. The largest presented the appearance of the sunflower as grown in gardens.

The dried plants weighed :—

	Gr.
Stalks	8·655
Leaves and flowers	7·028
Roots	5·535

21·218

The seeds weighed 0·107

Organic matter developed 21·111

Analyses made on three grammes of matter showed that the 21·218 grs. of dried plants should contain :—

	Gr.
Nitrogen	0·1697
The two seeds contained	0·0081

Nitrogen acquired in three months' vegetation . . 0·1666

1·4 gr. of nitrate of potassa added to the soil, contained, of nitrogen 0·1969

Difference 0·0303

There were consequently 0·0303 gr. of nitrogen of the manure which the helianthus had not fixed, nitrogen representing 0·219 gr. of nitrate of potassa, a part of which was found in the soil; the other portion had furnished carbonate of potassa, in consequence of the action of the organic matter of the roots, an action already proved by M. Schloessing, and which is explained by the facts lately published by M. Pelouze.

The examination of the ash showed that the plants had taken 0·265 gr. of phosphate of lime from the soil.

Carbon fixed during Vegetation.—The 21·111 grs. of organised matter contained 8·444 grs. of carbon, evidently derived from 30·961 of carbonic acid, say 15·637 litres. The vegetation had lasted 86 days, the helianthus had consequently, under the influence of saltpetre and phosphate of lime, taken every twenty-four hours, on an average, the carbon of 182 cubic centimetres of carbonic acid gas.

Experiment C.—*Vegetation of the helianthus under the influence of phosphate of lime, ashes, and bicarbonate of potassa.*

We have just seen that the introduction into the soil of saltpetre united to phosphate of lime, determined a considerable development of organised manure, and the assimilation of more than eight grammes of carbon. The sunflowers thus grown had the same appearance and vigor as those cultivated in the garden. The association of nitre with phosphate and ashes consequently forms a complete manure in which the plants found all that they required.

The experiment C was undertaken to ascertain what part of the influence on vegetable production should be attributed to the phosphate of lime. For this purpose the saltpetre was omitted; but as

this omission likewise occasioned a great loss of alkali, the nitre which had been used in experiment B was replaced by its equivalent of bicarbonate of potassa, which is a much less alkaline salt than the carbonate; it is, moreover, the bicarbonate which is found in farm-yard manure, as well as in the urine which the herbivora diffuse on the meadows.

The soil in the two experiments B and C were found as follows:—

	Experiment B.	Experiment C.
	Gr.	Gr.
Vessel of baked earth	600	600
Crushed clay	400	400
Quartzose sand	1026	1026
	2026	2026
	Gr.	Gr.
Nitrate of potassa { Potassa	0·652	Bicarb. of { 0·652
1·4 gr. containing { Assimilable nitrog. 0·197		Pot., 1·26 gr. } 0·000
Ash	0·500	0·500
Phosphate of lime	10·000	10·000

The two soils were therefore alike, with the exception of the assimilable nitrogen of the nitric acid, which was wanting in experiment C.

Two helianthus seeds, weighing 0·107 gr., were planted on the 5th July.

20th August.—The first normal leaves were withered.

	Length.	Width.
2nd normal leaves	27 millim.	10 millim. pale green.

Height of the plant, 9·6 and 11 centimetres; diameter of the stalks, 2 millimetres.

20th September.—The second normal leaves were withered.

	Length.	Width.	
3rd normal leaves	10 millim.	3 millim.	} of a very pale green.
4th normal leaves	9 "	8 "	
5th normal leaves	5 "	2 "	

Each plant had a bud. Height, 13·6 and 14 centimetres; diameter of the stalks, 2 millimetres.

30th September.—The plants had not changed in appearance since the 20th; both bore an extremely small yellow flower, well formed. As in experiment A, when nothing had been added to the soil, *limited plants* had been obtained.

	Gr.
The two plants dried weighed	0·498
The seeds	0·107

Organised matter developed	0·391
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In the same way as in experiment A, the plants appeared vigorous for two months; after that, the leaves withered towards the bottom of the stalk, and the strength of the vegetation decreased rapidly.

Analysis gave, in the dried plants, and in the remains of the soil:—

	Gr.
Nitrogen	0·0058
In the seeds	0·0081

Nitrogen acquired in three months' vegetation . 0·0027

Carbon fixed during the Vegetation.—The organised matter weighed 0·391 gr.; admitting 0·40 for the amount of carbon, we have 0·1564 gr. for the weight of this combustible taken at 0·573 gr., or 286 cubic centimetres of carbonic acid gas.

The vegetation having lasted 86 days, the plants must have assimilated, on an average, in each 24 hours, the carbon of 3·3 c.c. of carbonic acid gas; this is within 1 cubic centimetre of what the helianthus had assimilated in experiment A.

(To be continued.)

On the PRESERVATION of WHEAT by means of QUICK LIME.

By M. J. PERSOZ.

IN the note, of which this is an abridgment, after having mentioned a method of preserving seeds which obtained for its author, M. Petitot, a gold medal from the *Concours regionale de l'Est*, and, pointed out the circumstances which led me to examine this same question, I show the contradictory opinions which have been expressed about the efficacy of the various methods of preservation in our climate; contradictions which must be attributed to the insufficiently defined sense of the word *dry corn*. I have shown, in fact, that the proportion of water may vary from 8·5 to 18·5 per cent.

These experiments on dessiccation have proved a fact which should be laid before the Academy. Until seven per cent. of water, the wheat experimented on contracts, abandoning water, and, as might be expected, increasing in specific density. But from this point, it loses water without changing its volume, and consequently its density diminishes continually. Consequently, two specimens of wheat of low density may be met with, the lightness of which may be due, in one, to an excess of moisture, in the other to excessive dryness.

From my results we find that corn which is considered dry may contain quantities of moisture which differ as much as ten per cent. These differences are accompanied by properties which enable us to class them in two categories; those containing more than nine per cent. of water, and those containing nine per cent. and less.

Corn of both these categories being introduced into bottles with ground stoppers, and these being exposed to the radiating action of surrounding bodies, a phenomenon takes place which can only be considered as a species of **TRANSPIRATION**. On those sides directly opposite to the calorific action, we see the water condense in small drops, so as to render the grains of wheat which they moisten adherent at some points.

The calorific source being constant and acting constantly in the same direction, by leaving the bottle in one place, or varying its position, we can at will give all the power of the phenomenon to bear on one point, or make it appear and disappear successively on various parts of the vessel.

The corn of the first category *transpires* at low temperature; and all the more easily as it contains more water. We thus see that it cannot be preserved intact, except at very low temperatures and in equal calorific conditions; the least variation of temperature inducing, as its inevitable result, the removal and accumulation of a portion of the water on one point; when it finishes by inducing phenomena of alteration, which are soon propagated throughout the whole mass. Thus we have seen corn, contained in stoppered bottles, and which contained only fifteen per cent. of water, destroyed in a few weeks.

As for corn of the second category, its *transpiration* is never manifested at low temperatures; it requires the action of the solar rays,—and then, instead of small drops of water, it is a slight mistiness which appears in the sides of the vessel; but the corn never becomes adherent.

The consequence to be drawn from these experiments, as regards the preservation of grain, is very simple: it is to prevent this species of transpiration, and to oppose its effects by means of a powerful agent, easily procured and within the reach of all, from its reasonable price; and capable of use in agriculture, after having been used in the preservation of the grain. Lime appears to unite all these advantages.

M. Vilmorin has lately been a witness of my experiments with lime, and he tells me that for some years past he has instructed his travelling botanists to send him their seeds in very dry, stoppered bottles, with the precaution of introducing into each a little quicklime, wrapped in paper.

It is sufficient to say—

1.—That with quicklime we have preserved wheat unchanged in circumstances which were so favorable to its alteration, that the same corn could scarcely be preserved for a month when contained in stoppered bottles; and that, on the contrary, after nearly twenty-nine months, the same corn, preserved with quicklime, had lost none of its qualities, and still possessed all its germinative properties.

2.—That corn which had been made to germinate, having been mixed with quicklime, the germination was instantly stopped; and yet, when sifted and winnowed, it possessed no perceptible flavor.

3.—Finally, that corn in a state of decomposition, having been treated in the same way with quicklime, all fermentation soon ceased; and that the corn, sifted, winnowed, washed, and dried, might, up to a certain point, be mixed with ordinary wheat, although it might have lost 25 per cent. of its weight by fermentation.

We may observe, in conclusion, that sifting and winnowing will always remove the lime from the corn. The only inconvenience offered by corn which has been thus preserved, is that of being ex-

tremely hard and dry, and consequently of *pulverising* under the mill-stones, instead of *flattening*; now, as this flattening is necessary for the easy separation of the bran from the flour, this inconvenience will easily be remedied by swelling the grain by the addition of a certain quantity of water, before subjecting it to the action of the millstones.

Comptes Rendus, No. 22, June 1st, 1857.

ANALYTICAL RESEARCHES ON HAY TEA, and on some of the ALTERATIONS which take place in HAY from NATURAL MEADOWS, when treated either with HOT or COLD WATER. By M. ISIDORE PIERRE.

IN many of the works lately published on the rearing of cattle, mention has been made of a preparation known by the name of *hay tea*, and which is given to calves when being weaned. This name shows sufficiently, that it is prepared in the same manner as common tea; but I am not acquainted with any work, in which have been shown the nature and proportions of the substances yielded by the hay to the water in this preparation, nor in which there has been any comparison of the alimentary value of the normal hay, and that which it retains after its treatment by water.

I have now the honor of presenting to the Academy the first part of a work which I have undertaken on this subject.

M. Legoux, Member of the Agricultural Society at Caen, an intelligent and well-known farmer, has placed at my disposal, for these investigations, several trusses of hay of excellent quality, from an elevated meadow, and which he had been in the habit of using for tea for his calves; M. Berjot, manufacturer of chemical and pharmaceutical products at Caen, has treated 16 or 17 kilogrammes of this hay in the ingenious apparatus, in which he now prepares extracts of a perfection which has been seldom attained and never surpassed.

Treatment of the Hay by INFUSION with Heat.—We infused at two separate times, for six hours each time, 8·220 kilogrammes of hay in distilled water, kept during the whole time at a temperature of about 80° to 90° C. (176° to 194° F.), and the hay was squeezed after each infusion.

On evaporating *in vacuo* the liquors proceeding from this double treatment, we obtained 13·10 of dry extract entirely soluble in water, or about 15·94 per cent. of the raw hay. If we add that each kilogramme of hay only contained in reality 801 grammes of dry matter, the real yield of dry extract rises to 19·9 or 20 per cent.

The hay after this treatment was still of a good color; when dry it diffused an agreeable odor, and might certainly have passed in the market for ordinary hay of good quality.

Before the treatment, it contained, in the normal state, 13·9 gr. of nitrogen per kilogramme, and 17·4 gr. when dry.

After the treatment, it contained 14·6 gr. of nitrogen in the dry

state, and 11.7 in the normal state (estimating that at 20 per cent. of water).

The treatment to which the hay had been exposed, had consequently removed 2.8 gr. of nitrogen per kilogramme, or about 16.1 per cent. of what it originally contained; but it is evident that this is not the only loss which it had sustained, seeing that it had lost 20 per cent. of its weight. These two losses together bring to 33 per cent. the diminution which the normal hay had really sustained from its original richness in nitrogen, which was thus reduced to *two-thirds* of what it contained before the treatment.

The extract thus obtained was of a pale chocolate color and of a very agreeable odor, containing 18 per cent. of nitrogen, which is rather more than the original hay. This extract is extremely greedy of water, and deliquescent; it would be difficult to keep, except in M. Berjot's beautifully stopped bottles.

Treatment of the Hay by DISPLACEMENT, without Heat.—8450 grammes of hay were put into a large displacement apparatus, and infused for 12 hours in distilled water, at a temperature of 20° to 25° C. (68° to 77° F.); the water was then suffered to run off and the hay was pressed; this treatment was repeated several times, until the water came from the apparatus limpid and colorless.

By evaporation *in vacuo*, the waters when united gave 1400 grammes of dry extract, or 16.57 per cent. of the weight of hay taken in the normal state, of 20.7 per cent. of the weight of this same hay if completely dry. After this operation the hay appeared much whiter than that which had been acted upon by hot water, and I can only compare it to the effect produced upon the outside of an uncovered haystack after long rains, without having undergone any other apparent damage than this repeated washing in the open air.

Its nitrogen was reduced to 13.9 per cent. in the dry state, or 11.1 in the marketable condition (containing 20 per cent. of water).

Under the influence of this treatment with cold water, the hay had lost in the first place 3.5 gr. of nitrogen per kilogramme, or 20 per cent., without reckoning its loss of weight; the combination of the two losses brings to 36.5 per cent. the diminution of the proportion of nitrogen originally contained in the hay.

This enormous loss will give us an idea of the kind of damage which is done to hay by the influence of rain alone, and that without taking any unpleasant taste or contracting any bad odor.

The dry extract given by this operation was very similar to that obtained by the previous operation, and its amount of nitrogen was 17.3 gr. per kilogramme, which differs but little from the extract produced by infusions with heat.

After having ascertained the amount of nitrogen in these various substances, I endeavored to ascertain what modifications there might have been produced in the constitution of the hay, under the influence of these various treatments. I shall at present only mention the most important mineral principles, silica, phosphoric acid, lime, magnesia, soda and potassa.

	Normal hay per kilo. gr.	Treated with heat per kilo. gr.	Treated cold per kilo. gr.
Ash . . .	69·011	39·591	35·155
Silica . . .	19·406	20·363	23·155
Phosphoric acid .	4·440	2·756	1·329
Lime . . .	12·637	9·859	8·681
Magnesia . . .	1·824	1·004	0·386
Soda . . .	15·956	3·931	1·153
Potassa . . .	12·527	0·900	1·395

We see immediately from these numbers that the loss has been chiefly in the potassa and soda, rather less in the phosphoric acid, and a little less again in the lime and magnesia.

Hay may thus lose nine-tenths of the potassa, almost the same of soda, from half to three-quarters of its phosphoric acid, from a third to a quarter of the lime. The apparent augmentation of the silica does not prove that the hay has not, under the influence of water, lost a certain quantity of this substance, for we shall very shortly see the contrary by the presence of silica in the ash of the extract; this apparent augmentation combined with the real diminution of the weight of the hay, is changed into a loss, comprised between 5 and 12 per cent. of the silica in the original hay.

The comparison of the ash of the extracts with that of the original hay, shows us, in a completely soluble state, the mineral principles removed from the hay by these different treatments.

These results are produced by one kilogramme of increased matter.

	Normal hay. gr.	Extract with heat. gr.	Without heat. gr.
Ash . . .	69·011	199·402	242·438
Silica . . .	19·406	18·118	9·180
Phosphoric acid	4·440	13·563	16·261
Lime . . .	12·637	24·557	37·199

That is to say, that extract of hay may contain, in the state of soluble compounds, from 9 to 18 thousandths of its weight of silica; from 13·5 to 16 thousandths of phosphoric acid in the state of phosphate, and from 25 to 36 grammes of lime per kilogramme of extract.

What is called *hay tea* appears, therefore, to form a very rational drink, which, independently of its aromatic, tonic, and stimulant properties, offers to young animals, under a form which pleases them, a food which is rich in nitrogenous principles, and containing, moreover, in very considerable proportions, the principles necessary for the development of their bony structure.

Another consequence which appears to me to result from this work, is, that under the influence of rain, even when uninjured by any fermentation, hay must lose a considerable proportion of the organic and mineral principles which are necessary to render a good aliment. We

have just seen, that water alone removes from it *more than a third* of the nitrogenous matters which it contains.

I shall shortly lay before the Academy, the result of an examination of the organic principles contained in hay tea, and of analogous results obtained by the treatment of clover, lucern and sainfoin.

Comptes Rendus.

On a SUBSTANCE called PHOSPHATIC GUANO.

By M. A. BOBIERRE.

I HAVE lately been analysing a substance which is worthy of serious attention—*phosphatic guano*. The partial vitrification of this matter, the surface of which represents a true porcelain, the texture of the mass, and, finally, the nature of the schistous rock on which the deposit rests, give rise to some curious hypotheses on the subject of the phases through which its formation has passed.

The six analyses which have been made of this *phosphatic guano*, from the Caribbee Islands, at New York, Boston, Philadelphia, Hull, Havre, at Paris by M. Payen, and finally at Nantes, by myself, prove that very remarkable differences have existed between the various samples. I have myself obtained very dissimilar figures according to whether I operated on the vitrified fragments or on the dust from the bottom of the cargo. The following was the average composition of six samples; 100 parts of matter dried at 105° C. (221° F.) gave—

Nitrogenous organic matter	.	.	.	.	7.60
Insoluble siliceous residue	.	.	.	.	2.00
Sulphate of lime	.	.	.	.	8.32
Phosphates of lime and magnesia	.	.	.	.	70.00
Alkaline salts	.	.	.	.	1.88
Carbonate of lime	}	.	.	.	10.20
Carbonate of magnesia		.	.	.	

100.00

The nitrogen of the dry matter amounts to $\frac{13}{1000}$ ths, which, for the organic substance itself, corresponds to a richness of about 5½ per cent.

One of the samples, that analysed at the *Conservatoire*, only contained 25 per cent. of phosphate of lime, whereas the silica amounted to 81 per cent.; but the nitrogen and organic matter were nearly in the proportions determined by the other analyses previously made in America and Hull, and since at Nantes. A piece of schistous rock had probably caused, by its presence in the mass examined, the considerable diminution in the phosphoric acid.

The examination of this sample, the indications of powerful actions to which its constituent matter must have been subjected; the considerable diminution of the organic substance under the influence of these actions; finally, the more or less close relations between this

deposit and the usual guanos whose nitrogenous principles may have been eliminated, open the door to hypotheses of very great interest.

M. ELIE DE BEAUMONT then mentioned that M. de Molon had sent him some time before, some samples of a guano from the *Ile des Moines*, in the Antilles, which was very rich in phosphate of lime. The appearance of these samples resembled those of M. Bobierre very much, and they would have been presented to the Academy already had M. Malaguti finished their examination.

Comptes Rendus.

On the FIXATION of the RICH PARTS of the MANURE in the EARTH.

By M. P. THENARD.

WHO has not deplored, in passing through villages, especially after heavy rains, the carelessness of the farmers who allow the waters to be blackened with manure, thus wasting, to the great detriment of health and production, the richest parts of their manure.

Well, if, after many more or less serious answers and objections, you say to them; but why, rather than have the goodness washed out of your manure, do you not take it immediately from the stable to the field and bury it at once? they will reply: The manure is not made, it is necessary before putting it into the ground, that it should be heaped up and allowed to ferment. And, in this respect, the farmer is right, for manure taken from the stable at once to the field, all losses deducted for fermentation and even for a little washing, is less profitable than when suitably fermented.

If, then entering into details, you ask him: Why do you accumulate the manure of a whole year on an eighth, sixth, quarter, third, or half your fields, instead of dividing it every year, in equal portions over the whole superficies of your farm? After replying more or less confusedly, as to the convenience of ploughing, etc., you still press him a little, he will finish by saying: In the earth the manure loses nothing; and in the majority of cases, the result is again favorable.

Thus the manure which has become black by fermentation, which runs in the streets on rainy days, which volatilises into the air on sunny days, does not become lost when once put into the ground; it then resists all these causes of destruction; it then awaits patiently the crops which it has to produce, whereas the manure as it comes from the stable, which, in proportion necessarily contains a larger quantity of rich principles, because they have not been removed by natural or artificial causes, is far from producing such satisfactory results, and is in great measure lost in the ground.

This is a double fact, apparently contradictory, which, like many other agriculturalists, I have had occasion to observe and which I am going to try to explain.

One day, having had occasion to treat earth with hydrofluoric acid, which had been previously boiled in dilute hydrochloric acid, so that every part should have been soluble in the hydrofluoric acid, I was

astonished to find at the bottom of my capsule a chocolate brown residue approaching to black, which had resisted this double attack; when dried this product became of a paler color; but when moistened it resumed its original tint; when calcined, it became white and remained so; this was pure alumina, then soluble in hydrofluoric acid. This led me to think that this product might have been a combination of alumina with the organic matter of the earth, a veritable lake, whose base had been protected from the action of the acids, by the coloring matter which would itself be unattackable by these same acids; and, as the earth analysed proceeded from a well cultivated and manured field, it might be, perhaps, a combination of the manure itself with the alumina of the earth.

But in this case alumina should combine with certain elements of the manure. This I verified directly.

I brayed alumina in jelly with manure water (this manure had been heaped up for about five months); the mixture was then thrown in a filter and the water passed through almost colorless; from black, which it had been at first, it had become of a very pale, slightly greenish yellow. As for the alumina, it was very carefully washed on the filter, but neither washing with cold water, nor boiling, changed the brown tint which it had taken from the manure; and the washing waters remained perfectly colorless. A lake had been formed and continued stable. In another experiment, I took alumina hydrated with only 3 equivalents of water, such as it is found in nature; it had been prepared by forming jelly of alumina, then collecting the alumina on a filter, the water which held it in a jelly separating from it. Now in this great state of cohesion it reacts on the solutions of the manure, with the same energy as the most hydrated alumina.

Analysis has demonstrated that alumina may thus directly absorb 50 per cent. of its weight of the coloring matter of manure, corresponding to 2.50 per cent. of nitrogen in the matter combined.

But if, instead of operating directly with alumina, we take a neutral salt of alumina, there is immediately formed an abundant precipitate of a greyish black, and the water is instantly decolorized. This precipitate is a true atomic compound: it gives to analysis about 5 per cent. of nitrogen, and contains only about one-twentieth of its weight of alumina, which demonstrates that the active substance has a very high equivalent, and renders it susceptible of a great many divisions.

Oxide of iron partakes almost to the same degree the properties of alumina; only that the lake appears not to be so stable. May we not discern in the instability of the iron lake, the explanation of the virtues which our farmers correctly attribute to what they call *fine red earth*? These earths contain a considerable quantity of very finely divided sesquioxide of iron, and, probably, in a peculiar state of hydration, which renders them fit to form lakes with manure. Might this be the indifferent oxide mentioned by M. Péan de Saint Gilles? This is a question to be decided. Hydrated silica does not decolor manure water. Carbonate of lime has at first no action on it; but if the liquor is allowed to evaporate spontaneously, or if it is

boiled, replacing in both cases the water evaporated, the decoloration at last takes place. Bicarbonate of lime, on the contrary, acts instantly, whatever may be the excess of carbonic acid.

Aluminate of lime also partakes of the properties of alumina, perhaps even in a more marked degree, for argil, which had no action either cold or hot, adds greatly to the activity of carbonate of lime. Is a double salt of alumina and lime formed, whilst the silica is set at liberty? There is some reason for this idea; indeed, every one knows that by boiling in platinum vessels a mixture of carbonate of lime and argil, no silicate of lime is formed; now when we make the same experiment with manure water, even a little alkalisied with ammonia, we find very considerable quantities of silica. We must likewise add that we have obtained the radical of manure in such a state of purity that it leaves no residue when burned, and, consequently, the silica obtained cannot proceed from the source which we have here indicated.

But without going further it appears that we may conclude from these experiments, that free alumina, the oxides of iron, and carbonate of lime, are preservatives for the manure, because they form with it lakes, which the action of time, water, and air only destroy very slowly, as almost all lakes are destroyed, and that doubtless in proportion as they are needed by the plants.

Consequently the farmer manures his fields in advance with perfect security, and all the more safely as they contain these elements particularly, large quantities of alumina and oxide of iron. For sandy and quartzose soils, as the country people say, burn the manure.

It is likewise in consequence of these phenomena, that argillaceous earths which are rich in themselves, but which have been impoverished by having had too much demanded from them, are so difficult to bring up and require such great quantities of manure before giving satisfactory results anew; whereas those which are always attended to produce so abundantly, and give so little trouble.

But what produces the difference which we have mentioned between the manure as it leaves the stable, and fermented manure? How is it that the one appears to be lost in the earth, whereas the other keeps so well? After what we have said, this question is answered in one word: if we triturate a solution of fresh manure with alumina or oxide of iron, it is decolorized like the other, that is to say, it loses its brown parts; but when we collect the solid parts the water is far from passing through colorless, and although it has lost its brown tint, it preserves a very deep saffron tint, which indicates the presence of a much larger quantity of matters than that given by the decolorized water of fermented manure. Moreover evaporation to dryness shows that the water of fresh manure, leaves ten times as much residue as those of fermented manure.

But besides, if we collect the water thus decolorized and expose it to the air, it soon becomes brown. The alumina then acts again upon it, and that in proportion as the colorable matter has not passed into coloring matter. It is, therefore, necessary, to be as useful as possible,

that the manure should have previously undergone a true oxidation or fermentation.

This fact explains the dislike which farmers feel to dig in fresh manure. In fact when mixed with earth its fermentation becoming very slow, always gives time for rain to come; then the rich matters not being fixed, but being, on the contrary, very soluble, are rapidly removed; this, of course, occasions considerable loss, which long practice has taught the farmer to avoid.

Comptes Rendus.

On a COLORED ORGANIC COMPOUND of ALUMINA and PEROXIDE of IRON found in the SOIL at KUYLOCH in 1824.—Some REFLECTIONS on the BROWN SUBSTANCE called ULMINE: the difference between MINERAL ANALYSIS and PROXIMATE ORGANIC ANALYSIS.—SUMMARY of ANALYTICAL EXPERIMENTS made on a BROWN LIQUID proceeding from the GREASE of SHEEP.—CONSIDERATIONS on SULPHUR with respect to the COMPOSITION of LIVING BODIES.

By M. CHEVREUL.

THE memoir of M. P. Thénard has so much connection with earlier works, and those which I have myself carried on for years that it has suggested to me some observations. I am far from wishing to stop or hinder researches which deserve every encouragement; I would rather show my sympathy for those researches and the interest which I feel in them, because of the great difficulty of the subject, and the light which such studies must sooner or later diffuse over pure science and applied science.

A compound of alumina, oxide of iron, and a colored organic matter, nitrogenous and perhaps sulphuretted, is mentioned in the 12th vol. of the *Mémoires du Muséum* of the year 1824.

This compound was found in the cavern of Kuyloch, which the late Dr. Buckland described as well worthy of the attention of geologists, in consequence of the bones of animals contained in it, and of a soil formed partly of, their remains in a very divided state. I made the chemical examination of this soil at the desire of the President of the Royal Society, Sir Humphry Davy.

Amongst other products I obtained a crystallisable salt, soluble in water, composed of sulphate of potassa and sulphate of ammonia, until then unknown in nature. It had been previously produced artificially by M. Linck.

Cold water removed it from this earth, leaving undissolved the combination of *alumina and peroxide of iron*. I described its properties in the following words:—

“Residue Insoluble in Alcohol.—It was formed of silica, alumina, magnesia, peroxide of iron, and a trace of oxide of manganese, a yellow coloring principle from the alcoholic extract, and a nitrogenous organic matter. On treating the residue with potassa a small quantity of ammonia was disengaged, apparently of fresh formation, and

the potassa water was colored of an orange red by dissolving the coloring principle and the probably altered nitrogenous organic matter. I thought that these two substances formed a kind of *lake* with the alumina and peroxide of iron. The residue, heated in contact with the air, kindled like a pyrophore, and the ash which it left was of a much paler color. When distilled, it gave a slightly acid water, oil, hydrosulphuric acid without sulphite of ammonia, carbonate of ammonia, a product having the odor of hydrocyanic acid, but with which it was impossible to produce Prussian blue, and finally a very voluminous black residue."

I have already observed that the colored nitrogenous organic matter was probably *sulphuretted*, because I obtained a nitrogenous and sulphuretted product by distilling the *lake*. But I did not pronounce positively on that point, but only said, "The soil of Kuyloch therefore contains *sulphur*, in some other state than as sulphuric acid. Is it in the state of hyposulphite, or hyposulphuric acid combined with an organic matter, without being in the acid state? This I have not been able to decide." If I have begun this note by recalling a combination of an organic matter with alumina and peroxide of iron, it is because I intend to shew the importance of sulphur, considered as an element of the principles of organic origin which are excreted from the bodies of animals.

I shall now give a summary of the reasons which I have for considering as ill defined species, those brown or black matters of which some have been called *ulmine*, or *ulmic acid*.

In the two last memoirs on artificial astringent substances which I presented to the Academy in 1809, I described several brown or black substances, insoluble or sparingly soluble in water, acid, and possessing the property of being dissolved by alkaline waters and assuming a brown color. I considered these brown or black compounds as resulting from the union of nitric or sulphuric acid with carburets of hydrogen. This diversity of composition consequently prevented me from considering them as *one sole definite species*.

From 1820 to 1824, I showed that lignose, sulphur, starch, &c., heated with potassa, without contact with the air, give rise to a disengagement of hydrogen gas, and that the aqueous solution obtained from lignose thus dishydrogenised under an alkaline influence, contains a colorless matter which, by absorbing oxygen, becomes brown, and is transformed into a matter which Braconnot considered to be *ulmine*.

The result of my experiments on the decomposition of a great many organic matters united to potassa in excess under the influence of the atmospheric oxygen, is that the brown products resulting from these slow combustions were too numerous and too different for us, in the present state of science, to give to them any one name, and call them either *ulmine*, or *ulmic acid*.

Now, the different memoirs from which these conclusions have

been taken, were published some years ago when I commenced the study of *wool in the grease*. Regarding it as a duty of my situation as director of the Royal dyeing manufactories to examine the primary matter of such various industrial processes, I was then far from thinking that to arrive at truly scientific results, it would be necessary first to establish the basis of *proximate organic analysis*; and it was only after having prepared the introduction to my researches on wool, read to the Academy, in May, 1856, that I ventured to hope that my successors would do some justice to my efforts to surmount the difficulties of this subject; for during my chemical career of fifty-four years, I never found any path which I wished to pursue so beset with obstacles.

In this introduction I have shown the principle of the essential difference between *mineral analysis* and *proximate organic analysis*.

In the *first* we seek the proportions of the elements of the compound which we are analysing, without troubling ourselves about the molecular arrangements, the proximate principles which form the compound, and the arrangement of the atoms. I speak, it must be remembered, of the analysis itself, and not of the study which may, and should be made afterwards to ascertain whether the compound analysed is or is not formed of several proximate principles, and whether it is possible to ascertain experimentally something of the arrangement of the atoms.

In the *proximate organic analysis*, we seek, in conformity with the qualification of *proximate*, the principles which *proximately* form the matter subjected to analysis; for example, in speaking of *eau sucrée*, the water and sugar are the *proximate* constituents of the *eau sucrée*, and not the elements, oxygen, carbon, and hydrogen, forming the water and the sugar.

It is only after separating the proximate principles in a state of purity that we proceed to determine respectively the elementary composition. But from the elementary analysis of a collection of proximate principles, it would be impossible to come to the composition of these proximate principles, as it is possible to do in mineral analysis.

From the differential character of the two orders of analysis which I have been comparing, arise for the proximate organic analysis:—

1st. The *necessity* of respecting the actual composition of the proximate principles which we wish to separate.

2nd. The *consequence* of not having recourse at first to any but the simplest means, those which being least powerful are the least likely to change the molecular constitution of the bodies which we wish to separate.

A *consequence* which requires:—

1st. At first the use of weak and neutral solvents, such as water, alcohol, ether, &c., &c.

2nd. That the operations should be made in those conditions which are most likely to preserve the stability of the proximate principles which we wish to separate. It is therefore necessary to operate at such temperatures as are incapable of altering the elementary com-

position, and to avoid the influence which the oxygen of the atmosphere exerts for the alteration of certain of these principles.

Conformably to the method of *proximate organic analysis*, of which I have described the spirit, after ascertaining in grease water:—

1st. One or several acids capable of being separated from the grease water by phosphoric acid, as *hydrates with an oily appearance*;

2nd. A matter precipitable under the form of flocks by the action of the acids;

3rd. Of chlorine precipitable by nitrate of silver;

4th. Of sulphuric acid precipitable by chloride of barium.

I obtained, only using the weakest solvents, a *brown liquid*, deprived—1st, of volatile acids precipitable by phosphoric acid, in globules of an oily appearance; 2nd, of a matter precipitable in flocks by acids; 3rd, chlorine; 4th, sulphuric acid.

This slightly acid *brown liquid*, entirely soluble in water—yielding nothing, so to speak, either to ether or alcohol, was formed of *potassa*, united with an *acid brown organic matter*.

The question was to know whether this *acid brown organic matter* was one, or composed of several proximate principles.

Now, on precipitating the *brown liquid* with fractions of the quantity of nitrate of lead capable of completely separating the *brown organic matter*, and filtering each time, we obtained three precipitates, the first of which was browner than the third; whence I concluded that the brown matter was composed of several proximate principles.

The decomposition of these lead precipitates by hydrosulphuric acid, performed separately, not having given me any isolated proximate principle, I reunited the three products, which were acid, and I treated the whole successively with ether, absolute alcohol, alcohol at 60° and water. The brown organic matter was really formed of several proximate principles.

The matters extracted by ether and absolute alcohol, of a slightly yellow color, gave a very acid product when distilled, whereas the matter soluble in alcohol at 60°, and especially the matter which was insoluble in it, gave a very ammoniacal product. They were both brown.

The ethereal extract, left to itself under such circumstances that it should lose its last portions of ether very slowly, gave a colorless crystallised acid with a purely acid flavor, soluble in ether, alcohol, and water. This body appeared to me to be a pure proximate principle; it was not an ethereal vinic acid.

I shall speak again of the mother-liquor of this acid which did not crystallise.

There is, in the portion of the brown liquid insoluble in ether and absolute alcohol, an acid brown matter, which I dare not say that I have obtained in a state of purity, but which, being capable of combining integrally with salifiable bases, is very distinct from the colorless crystallised acid. Indeed, it differs from it, not only by its brown color, but likewise by the ammoniacal product which it gives by distillation, and I must likewise add, by a considerable quantity of *sulphur* which manifests itself at the same time.

If this matter is not a *pure proximate principle endowed with acidity*, it must be the *combination* of an acid with a matter which is probably nitro-sulphuretted; water dissolves it, and the solution precipitates baryta in brown flocks, entirely soluble in nitric acid. .

I now return to the mother liquor from which the crystallisable acid had been separated. Does it contain an uncrystallisable acid or a crystallisable acid united to a small quantity of the acid nitrosulphuretted matter of an orange yellow color of which I have just spoken? This I did not ascertain. However it may be, it gives by distillation an acid product sensibly sulphuretted.

Only having obtained a few decigrammes of the crystallisable acid, I dare not affirm that it is free from sulphur; but certainly if it contains any, it is in extremely small proportion.

I have reduced this brown liquid—

1st. To potassa, which I now present to the Academy, in the state of quite pure nitrate and chloride of potassium, although I have had recourse to nothing but crystallisation in their purification.

2nd. An acid brown matter which perfectly neutralised potassa, and which I have again reduced into three substances; a colorless crystallisable acid; a slightly orange, yellow colored, uncrystallisable, sulphuretted acid; and an uncrystallisable, brown, nitrosulphuretted acid, forming a brown precipitate with baryta. It remained to determine whether these two last are proximate principles.

It will be seen that I did not use saline reagents in the analysis of the *brown liquid* until I had exhausted the action of water, alcohol, and ether, on the same liquid used with the intention of separating the proximate principles. It will also be seen how I succeeded in separating the acid matter which saturated potassa into three matters, after having precipitated this acid matter by nitrate of lead and likewise by chloride of barium. I still have to continue the examination of the three matters and to ascertain if the *acid brown flocks*, which separate from the brown liquid during the evaporation of the brown uncrystallisable acid do not proceed from an alteration produced under the influence of the air.

(To be continued).

METEOROLOGICAL CHEMISTRY.

On the QUANTITY of AMMONIA CONTAINED in ARTIFICIAL DEW.

By M. BOUSSINGAULT.

I HAVE given the name of *artificial dew* to the water deposited by the air more or less saturated with vapor when in contact with a surface which has been sufficiently cooled, although, in reality, I would not make any distinction between this dew and that which moistens the plants during a hot, calm, and serene night, that is to say when the state of the atmosphere is favorable to the calorific radiation of objects placed on the soil. Dew is perhaps the only aqueous meteor which man can produce at pleasure; its origin is always the same, the

vapor contained in air ; its appearance is always determined by the same cause, a lowering of temperature in the bodies on which it condenses.

In passing from the state of vapor to the liquid state, water necessarily takes with it the volatile and soluble substances diffused through the atmosphere. Thus we have ascertained in meteoric waters, and especially in dew, a considerable proportion of ammonia.

In 1853 I found that the dew collected at Liebfrauenberg contained per litre :—

				Ammonia. Milligr.
Dew collected during the night 18th and 19th August,				3·1
„	„	9th	„ 10th Sept.	6·2
„	„	11th	„ 12th Sept.	6·2
„	„	21st	„ 22nd Sept.	6·2
„	„	24th	„ 25th Sept.	1·2
„	„	27th	„ 28th Sept.	6·2

Although dew appears very frequently, as is indicated by the moisture with which the grass is almost always covered a little before the sun rises, the cases in which it is possible to obtain sufficient to examine it are extremely rare. Moreover, the circumstances in which the vapor dissolved in the atmosphere penetrates and condenses in a dry soil, without, however, any manifestation of dew as drops, are very common ; and there is reason to presume that, in the course of a year, the earth receives merely from its hygroscopic properties a considerable quantity of water, having, we have every reason to believe, the constitution of dew. Moreover, certain porous matters possess the facility of absorbing considerable volumes of atmospheric air, and, with this air, *dew water*, resulting from the precipitation of the vapor which accompanies it. Thus, every time that there is penetration and condensation of aqueous vapor in a porous body, there is brought with it a certain amount of ammonia, for in fact, there is a deposition of dew. It is this absorption of air, accompanied by a deposition of water which has existed as vapor in the atmosphere, that enables us to explain the appearance of ammonia in porous substances exposed to the air after having been calcined at a strong red heat, as results from some very delicate experiments which I executed some time ago. Thus I found that 1 kilogramme of matter pulverised and exposed to the air for two or three days, after calcination took up :—

										Ammonia. Milligr.
Brick	.	.	.	.	.	.	.	.	.	0·5
Sand	.	.	.	.	.	.	.	.	.	0·8
Phosphate of lime	.	.	.	.	.	.	.	.	.	0·8
Wood charcoal	.	.	.	.	.	.	.	.	.	2·9

I have found moreover, that when these matters were moistened with pure water immediately after calcination, when their porosity

was in some sort destroyed, they acquired no sensible amount of ammonia after an exposure of two or three days to the air.

However, it still remained to demonstrate that condensed water from the atmosphere contained ammonia. This led me to seek for this alkali in artificial dew, which I produced by the following means:—

I filled with ice a cylindrical glass vessel 77 centimetres in height, and 60 centimetres in circumference, as the height of the water at 0° C. (32° F.) did not exceed 66 centimetres, the refrigerating surface was about 3960 square centimetres. The vessel was put on a stand with a gutter round, which poured into a bottle.

The apparatus was placed in a room opening on a terrace of the *Conservatoire Impérial des Arts et Métiers* when it was left exposed, renewing the ice from time to time, from the morning of the 20th till that of the 22nd of May. The temperature of the room was kept at about 24° to 26° C. (75° to 79° F.) A pint of artificial dew was thus obtained; it was an extremely limpid water, on which floated two imponderable particles of soot. This water was not rendered turbid by salts of baryta. Nitrate of silver occasioned a slight cloudiness. Sub-acetate of lead formed a very distinct precipitate indicating the presence of carbonic acid. It had no peculiar odor nor flavor.

Treated with all proper precautions in the apparatus which I used in studying rain waters, I removed a quantity of ammonia answering to 10·8 milligr. per litre, a very large proportion compared to that furnished by the dew collected at Liebfrauenberg, far from all habitations. A special investigation made on the residue of the distillation indicated the certain presence of nitric acid.

The water assumed, by concentration to one hundredth of its original volume, the amber yellow tint characteristic of meteoric waters.

I have often expressed my opinion that the examination of aqueous meteors would lead to a knowledge of certain principles which the atmosphere contains only in extremely small proportions, although their action on organised beings is evident, and sometimes fatal.

In a work executed in 1853, I showed that rain, especially when commencing, brings with it substances which it would be in vain to attempt estimating by direct analysis. Dew, still better than rain, condenses and concentrates these substances; and as we can cause its formation everywhere, and in all meteorological conditions, it will doubtless facilitate the study of the very various matters of which the air is the receptacle and vehicle.

Comptes Rendus, No. 21, May 25th, 1857.

VEGETABLE PHYSIOLOGY.

On the RESPIRATION of VEGETABLES. By M. CORENWINDER.

I HAVE been employed for two or three months in researches on the respiration of vegetables.

I place the vegetables next the soil, under bell-glasses or in bottles; through which I pass, by means of an aspirator, a current of air, and I collect the carbonic acid in baryta water.

M. Boussingault concluded from his experiments on air confined in the soil, that the latter probably contains the most abundant source of carbonic acid rendered available by vegetation.

On my part, I have confirmed his opinion by showing that the soil exhales spontaneously quantities of carbonic acid, which are sometimes very considerable.

I wished to ascertain whether plants retain a portion or the whole of the carbonic acid exhaled by the soil. I demonstrated this fact with a certain number of vegetables in the following manner:

Under the bell-glass of my apparatus I put a flower-pot, containing earth, in which I had sown or planted the plant which I wished to study, after it had attained a certain growth in the open air.

I caused a current of external air to pass through this bell-glass from nine o'clock in the morning until five in the afternoon, and I collected the carbonic acid produced in it. The rapidity of the current did not exceed twenty litres in eight hours. The next day I cut down the plant to a level with the soil, if it had a fibrous root, and I determined, during the same time as the day before, the quantity of carbonic acid furnished by the roots, the soil, and the atmospheric air.

If I found a smaller quantity of carbonic acid on the first day than on the second, all the conditions being similar, the inevitable result is that the difference expressed approximately the quantity of carbonic acid which the plant decomposed during the first day of the experiment.

The following are some of the results hitherto obtained:

Four plants of *thlaspi*, of eleven or twelve centimetres in height, absorbed, in eight hours of the day, at the average temperature of 10° or 11° C. (52° to 54° F.), seventeen cubic centimetres of carbonic acid.

Twelve plants of peas, of 18 to 20 centimetres in height, absorbed during the same period, temperature 14° C. (57° F.), 8 cubic centimetres of carbonic acid.

A lettuce decomposed all the carbonic acid furnished by the root, the earth, and the atmospheric air. The baryta water in the receiving eprouvette did not become the least turbid during the day, although the weather was rainy and cold. The next day, the earth, the roots, and the air gave 16 cubic centimetres of carbonic acid.

Having observed that this plant had many hairs on the stalk, I finally made an experiment on some common nettles.

The experiment commenced at nine o'clock. I remarked, in a few minutes, that the baryta water was turbid; at noon, it no longer absorbed anything, and I stopped the operation. The deposit of carbonate was considerable.

About two o'clock, the sun being upon the bell-glass, I put clear baryta water into the recipient eprouvette, and, during the whole time that the sun remained on it, the water continued perfectly limpid.

But, as soon as the sun passed off, the baryta water became white in a few minutes; and, at the close of the day, the deposit of carbonate was again abundant.

In my memoir I intend giving the numerical results of these investigations, and those which I have yet to make.

From these preliminary experiments, necessarily incomplete, and which I only mention to take date, we must conclude that the chemical phenomena of the respiration of plants are by no means so simple as is generally supposed. The evident result of the above is, that, to ascertain the general law which regulates these phenomena, it will be necessary to make numerous experiments with a very considerable variety of vegetables.

The last experiment which I have made was as follows:

I exposed to the sun, for four hours, a flower-pot, containing seven plants of peas, of 50 centimetres in height.

During the whole time that the sun was on them, the baryta water remained perfectly limpid, the carbonic acid of the soil, &c., was therefore entirely absorbed; the next, the weather being fortunately as hot as the previous day, and the sun as strong, the earth and the roots exhaled a considerable quantity when the stalks were cut down.

Comptes Rendus, No. 22, June 1st, 1857.

PHYSICS.

On SLOW ACTIONS PRODUCED under the COMBINED INFLUENCE of HEAT and PRESSURE. By M. BECQUEREL.

M. BECQUEREL has been for a long time occupied with the slow actions produced on the surface or in the superficial layers of the earth, at the ordinary temperature and pressure. The origin of these actions is either chemical, electrical, or mechanical; but the chemical cause is the most efficacious. The following examples will give an idea of the influence of each of these causes.

When a plate of iron remains exposed to moist air, it soon becomes oxidised here and there on points where there is heterogeneousness, or where there are foreign bodies.

These points constitute so many voltaic couples, which start the original chemical action. In the electro-chemical reaction which takes place, water is decomposed, the hydrogen brought to the oxide combines with the nitrogen of the air or adherent organic matter, to form ammonia or carbonate of ammonia, which is usually found in the rust. The effect is still more manifest when we apply to the iron plate a piece of charcoal or some other body which is a good conductor, less oxidisable than iron. Copper, lead, and silver, in contact with certain solutions, produce analogous effects.

Rocks with a feldspath base, or containing an alkali, when broken and their fragments, carried by the waters, roll into the torrents, are decomposed by the mutual rubbing of their parts. M. Becquerel

showed this decomposition twenty years ago, by braying in an agate mortar, in the presence of water, baysalt, feldspath, &c.: the humid paste formed soon gave the alkaline reaction. The decomposition of bodies subjected to porphyrisation is sometimes facilitated by mixing them with other bodies, the elements of which might react by double decomposition on the elements of the former.

For example—by trituration together, in equal atomic proportions, nitrate of lead and iodide of potassium, we obtain in a few moments iodide of lead and nitrate of potassa. Operating with sulphate of soda and carbonate of lime, a double decomposition is likewise produced.

M. Daubrée has lately produced fresh proofs in support of the above observations; he operates as follows:—He introduces into a cask, to which he gives a rapid rotatory motion, water containing pieces of feldspath and quartz; after some time a kind of mud is deposited, produced by the trituration of the parts, and the water becomes alkaline. The feldspath has consequently been decomposed, at least in some degree.

M. Becquerel had hitherto acted in these researches at the ordinary temperature and pressure of the atmosphere; he has since experimented at more or less elevated temperatures and pressures, so as to have some idea of what must be produced in soils of sediment, when they are covered by rocks of granite, porphyry, basalt, &c. In this memoir are considered only the combined influences of heat and pressure in chemical and electro-chemical action. The mechanical actions will form the subject of another note.

He operates as follows:—Into a tube of five or six millimetres in internal diameter, and two decimetres in length, closed at one end, are put the solid body and the solution which is to react upon it; on these are poured sulphuret of carbon or ether, the tube is then closed in the lamp and placed in a stove heated to 100° or 150° C. (212° to 302° F.) Sometimes into this tube another tube is introduced, containing another volatile liquid, whose elements should react on the solid body or the solution. Finally, when electro-chemical effects are wished for, the apparatus intended to produce them are placed in the tube. With these various modes of proceeding, M. Becquerel has obtained the following products:—

1st. Arragonite in rectangular prisms, with two feather edges to each summit, the angles of which may be measured with the goniometer.

2nd. Protoxide of copper in beautiful octohedral crystals.

3rd. The sulphurets of copper in prisms of six articulated planes, having the appearance of nature.

4th. Sulphuret of silver and lead, in lamellæ, with a metallic aspect.

5th. Green carbonate of copper (malachite) and blue carbonate of copper in little lumps.

6th. Insoluble and crystallised metallic iodides, bromides, and cyanides, &c.

The result of M. Becquerel's investigations is, that, under the combined influences of heat and pressure, slow actions receive a fresh

activity and produce effects of great interest to physico-chemical science and geology.

Comptes Rendus.

*On the CONSERVATION of FORCE.**

By Professor FARADAY, D.C.L., F.R.S.

VARIOUS circumstances induce me at the present moment, to put forth a consideration regarding the conservation of force. I do not suppose that I can utter any truth respecting it, that has not already presented itself to the high and piercing intellects which move within the exalted regions of science; but the course of my own investigations and views makes me think, that the consideration may be of service to those persevering laborers (amongst whom I endeavor to class myself), who, occupied in the comparison of physical ideas with fundamental principles, and continually sustaining and aiding themselves by experiment and observation, delight to labor for the advance of natural knowledge, and strive to follow it into undiscovered regions.

There is no question which lies closer to the root of all physical knowledge, than that which inquires whether force can be destroyed or not. The progress of the strict science of modern times has tended more and more to produce the conviction that "force can neither be created nor destroyed;" and to render daily more manifest the value of the knowledge of that truth in experimental research. To admit, indeed, that force may be destructible or can altogether disappear, would be to admit that matter could be uncreated; for we know matter only by its forces: and though one of these is most commonly referred to, namely gravity, to prove its presence, it is not because gravity has any pretension, or any exemption, amongst the forms of force as regards the principle of *conservation*; but simply that being, as far as we perceive, inconvertible in its nature and unchangeable in its manifestation, it offers an unchanging test of the matter which we recognise by it.

Agreeing with those who admit the conservation of force to be a principle in physics, as large and sure as that of the indestructibility of matter, or the invariability of gravity, I think that no particular idea of force has a right to unlimited or unqualified acceptance, that does not include *assent* to it; and also, to *definite amount* and *definite disposition of the force*, either in one effect or another, for these are necessary consequences: therefore, I urge, that the conservation of force ought to be admitted as a physical principle in all our hypotheses, whether partial or general, regarding the actions of matter. I have had doubts in my own mind whether the considerations I am about to advance are not rather metaphysical than physical. I am unable to define what is metaphysical in physical science; and am exceedingly adverse to the easy and unconsidered admission of one supposition upon another, suggested as they often are by very imperfect

* Read before the Royal Institution of Great Britain.

induction from a small number of facts, or by a very imperfect observation of the facts themselves; but, on the other hand, I think the philosopher may be bold in his application of principles which have been developed by close inquiry, have stood through much investigation, and continually increase in force. For instance, *time* is growing up daily into importance as an element in the exercise of force. The earth moves in its orbit in time; the crust of the earth moves in time; light moves in time; an electro-magnet requires time for its change by an electric current; to inquire, therefore, whether power, acting either at sensible or insensible distances, always acts in *time*, is not to be metaphysical; if it acts in time and across space, it must act by physical lines of force; and our view of the nature of the force may be affected to the extremest degree by the conclusions, which experiment and observation on time may supply; being, perhaps, finally determinable only by them. To inquire after the possible time in which gravitating, magnetic, or electric force is exerted, is no more metaphysical than to mark the times of the hands of a clock in their progress; or that of the temple of Serapis in its ascents and descents; or the periods of the occultations of Jupiter's satellites; or that in which the light from them comes to the earth. Again, in some of the known cases of action in time, something happens whilst the *time* is passing which did not happen before, and does not continue after; it is, therefore, not metaphysical to expect an effect in *every case*, or to endeavor to discover its existence and determine its nature. So in regard to the principle of the conservation of force; I do not think that to admit it, and its consequences, whatever they may be, is to be metaphysical; on the contrary, if that word have any application to physics, then I think that any hypothesis, whether of heat, or electricity, or gravitation, or any other form of force, which either willingly or unwillingly dispenses with the principle of conservation, is more liable to the charge, than those which, by including it, become so far more strict and precise.

Supposing that the truth of the principle of the conservation of force is assented to, I come to *its uses*. No hypothesis should be admitted, nor any assertion of a fact credited, that denies the principle. No view should be inconsistent or incompatible with it. Many of our hypotheses in the present state of science may not comprehend it, and may be unable to suggest its consequences; but none should oppose or contradict it.

If the principle be admitted, we perceive at once, that a theory or definition, though it may not contradict the principle, cannot be accepted as sufficient or complete unless the former be contained in it; that however well or perfectly the definition may include and represent the state of things commonly considered under it, that state or result is only partial, and must not be accepted as exhausting the power or being the full equivalent, and therefore cannot be considered as representing its *whole nature*; that, indeed, it may express only a very small part of the whole, only a residual phenomenon, and hence give us but little indication of the full natural truth. Allowing the

principle its force, we ought, in every hypothesis, either to account for its consequences by saying what the changes are when force of a given kind apparently disappears, as when ice thaws, or else should leave space for the idea of the conversion. If any hypothesis, more or less trustworthy on other accounts, is insufficient in expressing it or incompatible with it, the place of deficiency or opposition should be marked as the most important for examination; for there lies the hope of a discovery of new laws or a new condition of force. The deficiency should never be accepted as satisfactory, but be remembered and used as a stimulant to further inquiry; for conversions of force may here be hoped for. Suppositions may be accepted for the time, provided they are not in contradiction with the principle. Even an increased or diminished capacity is better than nothing at all, because such a supposition, if made, must be consistent with the nature of the original hypothesis, and may, therefore, by the application of experiment, be converted into a further test of probable truth. The case of a force simply removed or suspended, without a transferred exertion in some other direction, appears to me to be absolutely impossible.

If the principle be accepted as true, we have a right to pursue it to its consequences, no matter what they may be. It is, indeed, a duty to do so. A theory may be perfection, as far as it goes, but a consideration going beyond it, is not for that reason to be shut out. We might as well accept our limited horizon as the limits of the world. No magnitude, either of the phenomena or of the results to be dealt with, should stop our exertions to ascertain, by the use of the principle, that something remains to be discovered, and to trace in what direction that discovery may lie.

I will endeavor to illustrate some of the points which have been urged, by reference, in the first instance, to a case of power, which has long had great attractions for me, because of its extreme simplicity, its promising nature, its universal presence, and its invariability under like circumstances; on which, though I have experimented* and as yet failed, I think experiment would be well bestowed: I mean the force of gravitation. I believe I represent the received idea of the gravitating force aright, in saying, that it is *a simple attractive force exerted between any two or all the particles of masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance*. The usual idea of the force implies *direct* action at a distance; and such a view appears to present little difficulty except to Newton, and a few, including myself, who in that respect, may be of like mind with him.†

This idea of gravity appears to me to ignore entirely the principle of the conservation of force; and by the terms of its definition, if taken in an absolute sense "*varying inversely as the square of the distance*" to be in direct opposition to it; and it becomes my duty, now, to point out where this contradiction occurs, and to use it in illustration of the principle of conservation. Assume two particles of

* Philosophical Transactions, 1851, p. 1.

† See Note, p. 7.

matter A and B, in free space, and a force in each or in both by which they gravitate towards each other, the force being unalterable for an unchanging distance, but varying inversely as the square of the distance when the latter varies. Then, at the distance of 10 the force may be estimated as 1; whilst at the distance of 1, i.e. one-tenth of the former, the force will be 100; and if we suppose an elastic spring to be introduced between the two as a measure of the attractive force, the power compressing it will be a hundred times as much in the latter case as in the former. But from whence can this enormous increase of the power come? If we say that it is the character of this force, and content ourselves with that as a sufficient answer, then it appears to me, we admit a *creation* of power, and that to an enormous amount; yet by a change of condition, so small and simple, as to fail in leading the least instructed mind to think that it can be a sufficient cause:—We should admit a result which would equal the highest act our minds can appreciate of the working of infinite power upon matter; we should let loose the highest law in physical science which our faculties permit us to perceive, namely the *conservation of force*. Suppose the two particles A and B removed back to the greater distance of 10, then the force of attraction would be only a hundredth part of that they previously possessed; this, according to the statement that the force varies inversely as the square of the distance would double the strangeness of the above results; it would be an *annihilation* of force; an effect equal in its infinity and its consequences with *creation*, and only within the power of Him who has created.

We have a right to view gravitation under every form that either its definition or its effects can suggest to the mind; it is our privilege to do so with every force in nature; and it is only by so doing, that we have succeeded, to a large extent, in relating the various forms of power, so as to derive one from another, and thereby obtain confirmatory evidence of the great principle of the conservation of force. Then let us consider the two particles A and B as attracting each other by the force of gravitation, under another view. According to the definition, the force depends upon both particles, and if the particle A or B were by itself, it could not gravitate, i.e. it could have no attraction, no *force* of gravity. Supposing A to exist in that isolated state, and without gravitating force, and then B placed in relation to it, gravitation comes on, as is supposed, on the part of both. Now, without trying to imagine *how* B, which had no gravitating force, can raise up gravitating force in A; and how A, equally without force beforehand can raise up force in B, still, to imagine it as a fact done, is to admit a creation of force in both particles; and so to bring ourselves within the impossible consequences which have already been referred to.

It may be said we cannot have an idea of one particle by itself, and so the reasoning fails. For my part I can comprehend a particle by itself just as easily as many particles; and although I cannot conceive the relation of a lone particle to gravitation, according to the limited view which is at present taken of that force, I can conceive its

relation to something which causes gravitation, and with which, whether the particle is alone, or one of a universe of other particles, it is always related. But the reasoning upon a lone particle does not fail; for as the particles can be separated, we can easily conceive of the particle B being removed to an infinite distance from A, and then the power in A will be infinitely diminished. Such removal of B will be as if it were annihilated in regard to A, and the force in A will be annihilated at the same time; so that the case of a lone particle and that where different distances only are considered become one, being identical with each other in their consequences. And as removal of B to an infinite distance is as regards A annihilation of B, so removal to the smallest degree is, in principle, the same thing with displacement through infinite space; the smallest increase in distance involves annihilation of power; the annihilation of the second particle, so as to have A alone, involves no other consequence in relation to gravity; there is difference in degree, but no difference in the character of the result.

It seems hardly necessary to observe, that the same line of thought grows up in the mind if we consider the mutual gravitating action of one particle and many. The particle A will attract the particle B at the distance of a mile with a certain degree of force; it will attract a particle C at the same distance of a mile with a power equal to that by which it attracts B; if myriads of like particles be placed at the given distance of a mile, A will attract each with equal force; and if other particles be accumulated round it, within and without the sphere of two miles diameter, it will attract them all with a force varying inversely with the square of the distance. How are we to conceive of this force growing up in A to a million fold or more? and if the surrounding particles be then removed, of its diminution in an equal degree? Or, how are we to look upon the power raised up in all these outer particles by the action of A on them, or by their action one on another, without admitting, according to the limited definition of gravitation, the facile generation and annihilation of force?

The assumption which we make for the time with regard to the nature of a power (as gravity, heat, &c.), and the form of words in which we express it, i.e. its definition, should be consistent with the fundamental principles of force generally. The conservation of force is a fundamental principle; hence the assumption with regard to a particular form of force, ought to imply what becomes of the force when its action is *increased* or *diminished*, or its *direction changed*; or else the assumption should admit that it is deficient on that point, being only half competent to represent the force; and, in any case, should not be opposed to the principle of conservation. The usual definition of gravity as *an attractive force between the particles of matter VARYING inversely as the square of the distance*, whilst it stands as a full definition of the power, is inconsistent with the principle of the conservation of force. If we accept the principle, such a definition must be an imperfect account of the whole of the force, and is probably

only a description of one exercise of that power, whatever the nature of the force itself may be. If the definition be accepted as tacitly including the conservation of force, then it ought to admit, that consequences must occur during the suspended or diminished degree of its power as gravitation, equal in importance to the power suspended or hidden; being in fact equivalent to that diminution. It ought also to admit, that it is incompetent to suggest or deal with any of the consequences of that changed part or condition of the force, and cannot tell whether they depend on, or are related to, conditions *external* or *internal* to the gravitating particle; and, as it appears to me, can say neither yes nor no to any of the arguments or probabilities belonging to the subject.

(To be continued in our next).

BIBLIOGRAPHY.

THE CHEMICAL ATLAS, OR TABLES: SHEWING AT A GLANCE THE OPERATIONS OF QUALITATIVE ANALYSIS. With Practical Observations and Copious Indices of Tests and Reactions; accompanied by a Dictionary of Simple and Compound Substances, indicating the Tests by which they may be identified: and a Dictionary of Reagents, indicating their Preparation for the Laboratory, the Means of Testing their Purity, and their Behavior with Substances. By A. NORMANDY, Author of "The Commercial Hand-Book of Chemical Analysis," &c., &c., and Editor of "H. Rose's Treatise of Chemical Analysis." London: George Knight and Co., 1857.

SEVERAL works on chemical analysis have for many years held a high position in the estimation of the scientific chemist; and of some of these, in our capacity of reviewers, we have had to express a high opinion. The work before us, the author of which makes no pretension to throw these into the shade, will be found, in our opinion, far more useful to the student of analysis—nay—more, to the practitioner. The directions are more minute, and the number of cases introduced, infinitely more varied. There is scarcely a possible case which the author has not provided for.

We find it difficult to convey to our readers an accurate idea of the plan of this work, without giving a table as a specimen, and this the dimensions of our page will not admit of. We shall therefore allow the author to speak for himself, by an extract from the preface.

"The present work was first begun without any view to subsequent publication, but simply as a guide for the use of my son and a few pupils, but gradually as they advanced in their studies, it became necessary for me to enlarge their means of research; and so from their daily work, and from noting down at the time as they occurred, the difficulties, or the unusual, or misapprehended, or misobserved, or mistaken, or misconstrued phenomena, which are sure to present themselves to the beginner (and occasionally, indeed, to the proficient also), and which I had to explain, to rectify, or to correct,—the present Atlas of Chemical Tables, and the observations which accompany them, have increased to their present number.

"The work being in reality the result of an almost daily practice in the laboratory for the last four years, I hope I may be permitted to say, without violating a becoming reserve, that experience in this respect enables me to affirm that, in spite of the defects which doubtless may be discovered in it, any intelligent person, provided he have some knowledge of chemical manipulation, will be able, almost invariably, to carry on the qualitative analysis of most compounds to a successful issue.

"What I now offer, therefore, to the public, and more particularly to the chemical student, and to the manufacturer, miner, and assayer, is a practical guide to chemical qualitative analysis—an Atlas in which the operations of that branch of chemical research are mapped out, as it were, so as to shew, I believe clearly, and at a glance, not only the various steps of the analysis, but those which lead in the most direct and safe manner, to the contemplated end. The ways thus pointed out are not all right royal roads, for in the course, so to speak, of a complicated voyage of discovery, there are evidently many shoals to wade through, pitfalls to avoid, and occasionally some danger of collision, but all these will, I trust, be found correctly indicated or signalised; and I have no doubt but that, if proper attention be paid to the directions given, and to the observations which accompany them, the traveller—if I may be allowed to continue the simile—will find himself safely landed at the desired termination of his voyage.

"In the Tables, the names of the substances found, or sought for, and those of the reagents used, both for the sake of brevity and of accuracy, are represented by their symbols, and even in the non-tabulated part of the work, where the necessity for condensing words into the smallest possible space is less absolute, the symbols are generally retained; but I have given (page 2) a list of all the symbols used in the work, arranged in alphabetical order, so that the reader may, by referring to it, obtain at once the necessary interpretation without loss of time.

* * * * * "I have likewise added to the Tables observations which the actual practice of the students pointed out as necessary to the complete and thorough apprehension of the operations alluded to in the Tables; and how far I have been successful in thus clearing up the doubts which might arise in the prosecution of the analysis, it is now for the candid reader to decide.

"The Atlas is accompanied by two concise Dictionaries, one of which is a list of the simple substances and of their simple combinations, indicating their behavior with the tests which yield characteristic reactions, and by means of which they may accordingly be identified. Such a Dictionary, in my judgment, was absolutely required; for it is there only that the operator will find all the confirmatory tests to which he is referred in the Tables, and to which it is necessary that the substances found by him should respond before he can confidently trust his analysis; for it is only when a substance exhibits all the phenomena peculiar to it, under the influ-

ence of its appropriate tests, that the proof of its identity becomes irresistible.

"In the other Dictionary will be found the names of the reagents used in chemical analysis, the manner of preparing them for the laboratory, of ascertaining their purity, and their action on the simple bodies, and on their simple combinations. The object of such a book need scarcely be dwelt upon; there is, however, one of its uses which, though less immediately obvious, will often be found, I trust, a very great help to the chemical student or analyst. I allude to the enumeration of the various precipitates produced by reagents upon compounds, and by means of which the operator, from the observation of one of the reactions produced during the analytical process, may either obtain a clue to the nature of the substances which he will probably have to deal with, or rectify, or abandon surmises which he would thus find at variance, or incompatible with the phenomena observed."

From a careful examination, we are able to say that any person possessed of a slight knowledge of chemical manipulation, may by means of the Atlas and Dictionaries, soon make himself a proficient analyst. Professors of chemistry will find it of immense assistance in tuition; and students will find the difficulties of their study very considerably reduced.

Dr. Normandy has performed a task of extraordinary difficulty with very great success. No one who has not had to compile tables can comprehend the immense labor and thought the task demands. The object of a table is to present a condensed, and concise, and yet comprehensive view of a subject. A table should commonly reduce the labor of those for whom it is compiled, and the compiler may be said to take upon himself the labor which he saves his readers. Every one who studies the Atlas must be impressed with the magnitude of the author's labor, as he must also acknowledge the vast extent to which he has economised the time and trouble of those who avail themselves of his friendly assistance.

We trust that this industry will be rewarded by an extensive sale of the work, which is cheap at the price, and that we shall perceive the effects of its study in a great addition to the number of competent analysts.

The publishers, Messrs. Knight, those well known caterers for the wants of the scientific world, have appended to the work a copious catalogue of chemical and other scientific apparatus and chemical preparations with the prices attached, which will be of great utility to students, and especially those at a distance from London, as they will learn from it exactly what they require and what it will cost. We are aware, of course, that the publishers have introduced this catalogue as an advertisement; but we regard it, nevertheless, as a very useful addendum to the work.

THE CHEMIST.

ORIGINAL COMMUNICATIONS.

The SOLVENT POWERS of GLYCERINE. By JOHN S. BLOCKEY.

SOME time back having occasion to experiment with the disulphate of quinine, I accidentally discovered that glycerine, if gently heated, will dissolve more than 8 grs. per fluid drachm, or about $\frac{1}{12}$ of its weight of this salt; this fact will render glycerine a valuable vehicle for the therapeutical administration of quinine, as I noticed in a communication to the "Lancet," a few weeks ago. I have since found that glycerine appears to possess properties that may, perhaps, give it a place among the usual dissolving substances; for instance, salicine soluble in 22 parts by weight of cold water, and in 30 parts of 80 per cent.; alcohol will dissolve in 8 parts of cold glycerine; santonine soluble in 250 parts of boiling water, and in 3 of boiling alcohol, will dissolve in 18 parts of boiling glycerine; the solution, however, becomes thick and almost solid, with only 1 grain of santonine in 36 of glycerine on cooling, and a saturated boiling solution of santonine in glycerine, when cold may be inverted without loss.

Strychnine is soluble in 80 parts of boiling glycerine, but very slightly in cold.

From these experiments, it appears that many substances are soluble in glycerine, in a very different ratio to their solubility in water, &c. Iodide of lead dissolves sufficiently in boiling glycerine, to cause the solution to become turbid on cooling. Aconitine is scarcely soluble at all in this medium. May not glycerine be found a solvent, for many other comparatively insoluble substances, both in the inorganic and in the organic kingdom?

I invariably heat the glycerine to give it greater fluidity, and the quantity that may be thus dissolved, the solution remaining clear on cooling, I estimate as the quantity soluble in cold glycerine.

PREPARATION of IODIDE of BARIUM from WITHERITE.

By C. R. C. TICHBORNE.

As iodide of barium is used in photography, and as iodide of aluminium (another salt proposed as an iodizer) may be made from it, I give the results of some experiments to which I refer in this month's *Photographic Journal*, considering that they may be useful to the manufacturing chemist.

On treating powdered witherite with iodide of iron, we do not get a complete decomposition even after prolonged ebullition. To decompose it, native carbonate is generally heated with charcoal, which

accelerates the decomposition, as it requires a violent heat to drive off the carbonic acid from the witherite alone.

If we keep a mixture of witherite and half its weight of iron filings or borings (those from malleable iron are preferable) at a slow red heat for some time, in an iron or black lead crucible, the carbonate is decomposed, carbonic oxide being liberated. The heat must not be sufficient to run the iron. If the roasting has been carried far enough, the contents of the crucible are found to be strongly alkaline. On adding water and iodine, and then boiling for some time, the iodine forms iodide of barium, combining first with the excess of iron, and is then converted into the barium salt. The residual precipitate, which seems somewhat similar in composition to artificial magnetic oxide of iron, is, from its density, easily washed. The liquors, on evaporation, yield crystals of iodide of barium, which on recrystallisation are chemically pure.

This process is much preferable to that of making hydriodic acid and dissolving the carbonate in it, particularly when operating on a large quantity.

Wolverhampton, 22nd August, 1857.

TRANSLATIONS AND ABSTRACTS.

ORGANIC CHEMISTRY.

MEMOIR on the CONSTITUTION and on the TRUE FORMULA of OXALIC ACID. By M. A. WURTZ.

OXALIC acid is one of the longest known and most important of the organic acids. It appears in a very great number of reactions, as the ultimate product of the transformation and oxidation of the most complex and most varied organic matters. This acid, which has been so much studied, and whose properties are so well known, possesses apparently so simple a constitution, that chemists at first regarded it as a degree of oxidation of carbon, intermediate between carbonic oxide and carbonic acid. This opinion, which was long maintained, and is still held by some men of science, has more recently given way to another theory, which consists in regarding oxalic acid as a true organic acid, as a bibasic acid containing four equivalents of carbon, two equivalents of hydrogen, and eight equivalents of oxygen.

Founded principally on the faculty which this acid possesses of forming acid salts and double salts, in the same manner as tartaric acid, the new theory still wanted a solid basis, a true experimental proof. Moreover, if the molecule of oxalic acid is really as complicated as is indicated by the formula—



we are in some embarrassment as to classifying it. What is its mode of formation? From what body is it derived? What place should it occupy in a systematic classification of organic substances? These questions have never been entered upon. In the reactions in which

we find oxalic acid formed, it appears as a fortuitous product of the oxidation of complex organic matters, and we are unable to discover the link which connects the primary body with its derivative. When acetic acid is formed at the expense of the elements of alcohol, we follow this transformation step by step. We know now that the radical of alcohol is modified by substitution, in order to form the radical of acetic acid; with the exception of this modification, nothing is changed in the arrangement of the molecule. In general, if every thing is simple and precise as regards the mode of formation and the constitution of the monobasic acids, every thing is unknown or obscure, in the corresponding points, as regards the bibasic acids.

I hope that the facts which I have now the honor of submitting to the Academy, will throw some light on these theoretical questions. I think that they are of a nature to elucidate the mode of derivation of oxalic acid, to indicate its place in the series, and to settle its constitution and its true formula.

Oxalic acid is derived from glycol, a compound which I discovered last year (see THE CHEMIST, Sept. 1856, p. 724) by adding synthetically hydrogen and oxygen to the elements of olefiant gas. I regarded it as a diatomic alcohol, because, in forming ethers with the acids, each atom of glycol combined with two atoms of an acid. In being oxidised, glycol is converted into oxalic acid, as alcohol is converted into acetic acid.

The following are the means which I employed for accomplishing this oxidation:—

1. When a few drops of glycol are put in contact with platinum black, a most vivid reaction is instantly observed; the platinum black becomes incandescent, the glycol disappears, and carbonic acid is disengaged in abundance. This disengagement of carbonic acid is also produced when we mix with platinum black, glycol diluted with several times its bulk of water. The mixture becomes heated, and if we exhaust it with water when the reaction is completed, we obtain by the evaporation of the solution only a trace of a fixed acid, forming with lime a soluble salt, and reducing the salts of silver.

These characters are those of glycolic acid.

The oxidation which glycol undergoes in these circumstances being too violent, I resorted to other means.

2. About ten grammes of glycol were dissolved in four volumes of weak nitric acid, marking 86 degrees of the acidimeter, and the solution was kept for several days at the ordinary temperature. Evaporated in vacuo over a plate containing pieces of lime, it left a syrupy and strongly acid residue. This residue was diluted with water, and neutralised with chalk. The filtered solution, having been mixed and concentrated, was precipitated with alcohol, and deposited an abundant precipitate, which was redissolved in boiling water.

The liquor became a mass on cooling, depositing a salt of lime, crystallised in tufts and in mamelons, formed of very fine needles interlaced. This salt of lime was purified by a new crystallisation. At 120° C. (248° F.), it lost 22 per cent. of water.

The dry salt contains :—

	Experiment.		Theory.
	24.87	25.00	
Carbon . . .	24.87	25.00	C ⁴ . 25.26
Hydrogen . . .	8.52	3.400	H ³ . 3.15
Oxygen . . .			O ⁶ .
Lime . . .	28.93		CaO 29.47

The formula



which expresses the composition of this salt, is that of glycolate of lime. The acid which it contains C⁴ H⁴ O⁶, was first noticed by MM. Strecker and Socoloff, as a product of oxidation of glyocolle, or sugar of gelatine. It is identical or isomeric with the acid which M. Cloez extracted from the mother-liquors of fulminate of mercury, and which he called *homolactic acid*. The acid C⁴ H⁴ O⁶ is indeed the homologue of lactic acid C⁶ H⁶ O⁶. It is possible that it effects two modifications, like lactic acid itself. The salt of lime which I analysed and which very much resembles lactate of lime, appears to me indeed to differ, by its sparing solubility in water, from ordinary glycollate of lime. *

Be this as it may, it swells up very much when calcined, and the acid which it contains readily reduces the salts of silver, as has been observed with ordinary glycolic acid.

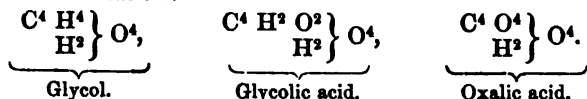
When lime is accurately precipitated by oxalic acid, and the filtered solution evaporated first on a water bath and then in vacuo, crystallised glycolic acid is obtained.

The alcoholic liquor, from which the glycolate of lime was deposited, did not contain any substance analogous to aldehyde and capable of forming a crystallisable combination with bisulphite of soda.

3. Three grammes of glycol were submitted for a few minutes to ebullition, with four volumes of weak nitric acid. A very vivid reaction is set up, abundant red vapors are disengaged, and the liquor abandoned to itself from one day to the next, became a mass of crystals. These crystals were oxalic acid.

4. With monohydrated nitric acid, the oxidation of the glycol is still more energetic. Carbonic acid is disengaged in this case, and the liquor retains in solution oxalic acid, and even a certain quantity of glycolic acid.

It results from these experiments that the products of the regular and successive oxidation of glycol are two acids, glycolic acid and oxalic acid. Carbonic acid, which appears sometimes in these reactions, results from the oxidation of the oxalic acid itself. The following formulæ express the relations which exist between glycol and its products of oxidation :—

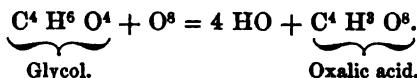


* The acid which M. Dessaignes obtained by hydrating glycolide, and which is isomeric with homolactic acid, likewise crystallises.

These relations are the same as those which exist between alcohol and acetic acid :—



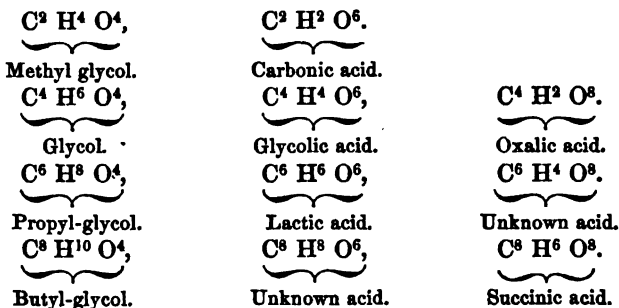
It is evident that in both cases, the products of oxidation are formed by the substitution of oxygen for a certain quantity of hydrogen of the alcoholic radical. The radical of glycol is olefiant gas. The oxygen is introduced by substitution into this radical; when this substitution is complete, oxalic acid is formed. If monoatomic alcohol requires, for its conversion into acetic acid, four equivalents of oxygen, diatomic glycol should require eight to undergo a corresponding transformation :—



In this respect, it may be said that oxalic acid, which results precisely from this oxidation, is the acetic acid of glycol. Glycolic acid is an intermedial product resulting from a less advanced oxidation.

Be this as it may, these experiments furnish a certain proof of the fact, that oxalic acid contains four equivalents of carbon; for, being derived from glycol, it proceeds in fact by synthesis from olefiant gas, which contains four equivalents of carbon.

It may be supposed, that the facts which I have just explained will not remain isolated. There exist, indeed, other organic acids which belong to the same series as oxalic acid, which contain like it, eight equivalents of oxygen, and which contain carbon and hydrogen combined in the same proportions. These are succinic, adipic, lipic, suberic, and sebacic acids. In the same way as oxalic acid is derived from glycol, these acids, which are higher in the series and which have hitherto been found isolated, are connected with the superior glycols, whose existence I have demonstrated. Their place in the series of innumerable organic combinations is henceforward assigned. The following formulæ express the relations which exist between the glycols and the acids which result from their oxidation.



&c.—*Comptes Rendus*, No. 26, June 29th, 1857.

ON INVERSE SUBSTITUTIONS. By M. BERTHELOT.

SECOND PART.*

1. ACCORDING to the facts which I have already detailed, bromide of methylene, $C^4 H^4 Br^2$, heated to $275^\circ C.$ ($527^\circ F.$) with copper, water and iodide of potassium, regenerates principally the olefant gas, $C^4 H^4$, which has formed it; heated with water and iodide of potassium above all hydroguret of ethyle, $C^4 H^6$, a compound in which the bromine of the bromide of ethylene is replaced by hydrogen; thus, we succeed, in fine, in adding hydrogen to olefant gas. All these reactions are so much the more clear as we operate more slowly, and at a temperature as near as possible to $275^\circ C.$ ($527^\circ F.$)

Bromide of propylene, $C^6 H^6 Br^2$, presents analogous reactions. Indeed, heated to $275^\circ C.$ ($527^\circ F.$) with copper, water and iodide of potassium, it regenerates principally propylene, $C^6 H^6$, which gave rise to it; heated with water and iodide of potassium, it produces especially hydroguret of propyle, $C^6 H^8$, a compound in the bromine of the bromide of potassium is replaced by hydrogen.

Bromide of butylene, $C^8 H^8 Br^2$, and bromide of amylene, $C^{10} H^{10} Br^2$, heated to $275^\circ C.$ ($527^\circ F.$) with copper, water and iodide of potassium, likewise reproduced butylene, $C^8 H^8$, and amylene, $C^{10} H^{10}$, which had given rise to them. Thus, by the processes which I have just detailed, we may isolate the alcoholic carburets, ethylene, propylene, butylene, and amylene, contained in a gaseous mixture, separate them from each other under the form of bromides, and then regenerate them in the gaseous state in which they were at first.

2. I endeavored to extend the application of the same methods to other compounds, such as Dutch liquor, chloroform, bromoform, iodoform, perchloride of carbon, bromuretted bromide of propylene and trichlorhydrine.

The heated liquor or chloride of ethylene, $C^4 H^4 Cl^{12}$, is much more difficult to decompose completely than the bromide of ethylene. However, if it be heated to $275^\circ C.$ ($527^\circ F.$) either with copper, water and iodide of potassium, or with water and iodide of potassium, a certain quantity of olefant gas, $C^4 H^4$, is regenerated; but this gas is mixed with monochloruretted ethylene, $C^4 H^3 Cl$.

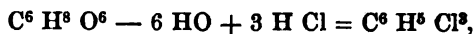
Chloroform, $C^3 H Cl^3$, bromoform $C^3 H Br^3$, and iodoform $C^3 H I^3$, decomposed, either by zincalene, or by copper, water and iodide of potassium, or by water and iodide of potassium only, produce a mixture of marsh gas $C^3 H^4$, of hydrogen, and in the last two cases, of carbonic oxide and carbonic acid. At the same time is formed, in small quantity, a gaseous or very volatile compound, absorbable by bromine, but the nature and origin of which could not be determined with certainty.

The perchloride of carbon, $C^3 Cl^4$, heated with iodide of potassium, copper and water, produced a mixture of marsh gas, $C^3 H^4$, carbonic oxide, hydrogen and carbonic acid.

* For the First Part, see THE CHEMIST, August, 1857, p. 644.

Bromuretted bromide of propylene, $C^6 H^5 Br^2$, heated with iodide of potassium, copper and water, regenerated a mixture of propylene $C^6 H^6$, of hydroguret of propyle, $C^6 H^3$, and of carbonic acid. It is evident, that the three equivalents of bromine which this compound contains, may be replaced by three equivalents of hydrogen.

Finally, trichlorhydrine, $C^6 H^5 Cl^3$, one of the hydrochloric ethers of glycerine, a body isomeric with chloruretted chloride of propylene, heated with iodide of potassium, copper and water, produced propylene, $C^6 H^6$, hydrogen and carbonic acid. We may thus, by a new way, pass from glycerine $C^6 H^8 O^6$, to the hydrocarbons which correspond to it, and especially remove all the oxygen which it contains, it is sufficient to eliminate all this oxygen under the form of water, by replacing this water with hydrochloric acid:—



the hydrogen is substituted for the chlorine. We exert then, in fact, a reducing action very remarkable for the simplicity of its mechanism, and probably susceptible of being generalised.

The whole of the foregoing reactions throw a more complete light on the constitution of the chloruretted and bromuretted compounds; it confirms, indeed, by the synthetical way, the analogies which exist between the molecular grouping of these compounds and that of the hydrocarbons from which they are derived by way of substitution.

Comptes Rendus, No. 26, June 29th, 1857.

DIRECT COMBINATION of the HYDRACIDS with the ALCOHOLIC CARBURETS. By M. BERTHELOT.

1. In a memoir published two years ago, I showed that olefant gas may fix the elements of water and become the source of alcohol: $C^4 H^4 + 2 HO = C^4 H^6 O^2$. Propylene, $C^6 H^6$, may undergo the same transformation, and may be changed into propylic alcohol, $C^6 H^8 O^2$. I have lately been led to generalise this reaction, and to extend it to the various carburets corresponding to the alcohols; but I have had to resort to new processes distinct from those which I had at first employed; for the sulphuric acid used in the first experiments, I substituted the hydracids. Indeed, the conversion of olefant gas into alcohol, and that of propylene into propylic alcohol, are the only ones which succeeded completely with the use of sulphuric acid, but this acid cannot be employed in the case of the hydrocarbons of high equivalent; it acts on these bodies with too much energy, sometimes carbonising them, and sometimes modifying them isomerically. Carpylene, for example, $C^{16} H^{16}$, mixed with concentrated sulphuric acid, gives rise to a homogeneous liquid, not without a brisk disengagement of heat; but the modified carburet very soon separates and floats, whilst the acid retains in solution only traces of organic matter. These phenomena call to mind the reaction of sulphuric acid on essence of turpentine.

I thought that the transformation of carburets into ethers and alcohols might be effected in a more general manner by the action of the hydracids.

2. I had already observed that propylene heated to 100° C. (212° F.) for seventy hours with an aqueous solution of hydrochloric acid is entirely absorbed, and gives rise to hydrochloro-propylic ether. This body is formed of propylene and hydrochloric acid gas united in equal volumes:

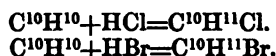


The combination is effected at the ordinary temperature, but much more slowly: it is not accelerated by prolonged agitation.

I have likewise combined, in the same conditions, hydrobromic and hydriodic acids with propylene, and I have obtained hydrobromo-propylic and hydriodo-propylic ethers.

These experiments were performed by heating to 100° C. (212° F.) in flasks sealed in the lamp, the gaseous hydrocarbon with an aqueous solution of the hydracids, saturated cold, and employed in great excess. The ethers formed are purified by distilling them after having agitated them with an aqueous solution of potassa.

3. The foregoing results may be generalised. Indeed, amylene, $\text{C}^{10}\text{H}^{10}$, combines with hydrochloric and hydrobromic acids in the same conditions, although more slowly, and less completely, whence result hydrochloro-amyl ether, $\text{C}^{10}\text{H}^{11}\text{Cl}$, and hydrobromo-amyl ether, $\text{C}^{10}\text{H}^{11}\text{Br}$:



4. Caprylene, $\text{C}^{16}\text{H}^{16}$, gives the same reactions; but the combination remains incomplete even after 100 hours' contact at 100° C. (212° F.) The ethers formed are purified by distillation. Hydrochloro-caprylic ether, $\text{C}^{16}\text{H}^{17}\text{Cl}$, and hydrobomo-caprylic ether, $\text{C}^{16}\text{H}^{17}\text{Br}$, are thus obtained:



If caprylene and hydrochloric acid gas be put in contact at the ordinary temperature, the carburet immediately absorbs seven or eight times its volume: then, the absorption continues gradually becoming slower, and is not increased much in activity by very prolonged agitation. After two hours, it is equal to 10 volumes; after five days, to 12 volumes; after eleven days, to 13 volumes; after seventeen days, to 14 volumes; after twenty-three days, to 15 volumes, &c.

5. Ethaline, C^8H^8 , acts in an analogous manner, whether at the ordinary temperature or at 100° C. (212° F.) At the latter temperature, after 100 hours' reaction, nearly half of the carburet is found combined with the hydrobromic or hydrochloric acid under the form of a neutral compound. The ethers formed could not be separated from the excess of carburet, because the heat necessary for distilling them decomposes them.

6. Finally, olefiant gas itself, C^4H^4 , heated to 100° C. (212° F.), during 100 hours, with an aqueous solution of hydrobromic acid

saturated cold, is completely absorbed; a neutral liquid similar to or identical with hydrobromic ether is formed. Hydrochloric acid in the same conditions furnished only traces of a chloruretted compound endowed with neutrality.

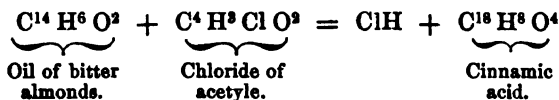
Thus the various carburets of hydrogen corresponding to the alcohols and formed of equal equivalents of carbon and hydrogen may be combined directly and in equal volumes with the hydracids and constitute hydrochloric and hydrobromic ethers, whence results a new approximation between the ethers and ammoniacal salts.

It is known, besides, that the hydrobromic ethers, decomposed by the salts of silver, furnish compound ethers, and, consequently, alcohols. By the methods which I have just explained, we may then, in general, convert the carburets of hydrogen into the ethers and alcohols which correspond to them.

Comptes Rendus, No. 26, June 29, 1857.

RESEARCHES on NEW DERIVATIVES of SALICYLE and on some IODIDES of ORGANIC RADICALS. By M. AUGUSTE CAHOURS.

WHEN we treat oil of bitter almonds in close vessels with chloride of acetylene, according to the recent experiments of M. Bertagnini, cinnamic acid is formed by virtue of the following reaction:—



which agrees perfectly with the mode of decomposition which this acid undergoes on the part of the hydrated alkalies:



Analysis and synthesis concur then in demonstrating that cinnamic acid is a conjugate acid, containing the elements of benzoic acid and acetic acid, and susceptible of reproducing them with the aid of water.

Coumaric acid presenting with regard to hydroguret of salicylene relations analogous to those which are observed between cinnamic acid and oil of bitter almonds, I had hoped to be able, by a similar reaction, to produce this body in considerable proportion, in order to submit it to a profound study. My anticipations have unfortunately turned out completely at fault.

The hydroguret of salicylene is vividly attacked by chloride of acetylene under the influence of heat; hydrochloric acid is disengaged in abundance, and a magnificent crystallised substance is obtained; but the latter, contrary to what I had supposed, was perfectly neutral. Not only, indeed, does it not dissolve in a weak solution of ammonia or

potassa, but neither solid hydrate of potassa nor anhydrous baryta attack it under the influence of heat. It is the same in a boiling alcoholic solution of potassa.

Insoluble in water, this compound dissolves in very small proportion in cold alcohol; boiling, this liquid dissolves it in much more considerable quantities, and abandons it on cooling almost entirely under the form of beautiful needles. Chlorine, bromine and fuming nitric acid react with energy on this substance, giving rise to derivatives by substitution which crystallise very clearly.

The analysis of this compound furnished me the following results:—

I. 0.302 gr. of matter gave 0.131 gr. of water and 0.730 gr. of carbonic acid.

II. 0.295 gr. of the same product gave 0.130 gr. of water and 0.711 gr. of carbonic acid.

These results, brought into hundredths, lead to the following numbers:

	I.	II.
Carbon	65.91	65.72
Hydrogen	4.81	4.89
Oxygen	”	”

which agree with the formula



Indeed, we have:

C^{18}	108	65.85
H^8	8	4.78
O^6	48	29.37
	164	100.00

This body, which presents, as is known, a composition identical with that of coumaric acid, does not possess any of its properties; it is one of those cases of isomerism which the study of organic matters sometimes presents. I shall designate it by the name of *acetosalicycle*.

In the same circumstances, benzoic acid is transformed under the influence of chloride of acetylene into another isomer, acetobenzoic acid, which is divided in its turn into hydrated acetic and benzoic acids with the concurrence of water.

Acetosalicycle is a body of remarkable stability; indeed, when distilled at a dull red over anhydrous baryta, it undergoes no alteration, as the properties and composition of the distilled body attest.

Indeed, 0.327 gr. of matter furnished by their combination with the acid of copper, 0.142 gr. of water and 0.792 of carbonic acid; results which, brought into hundredths, give:

Carbon	66.05
Hydrogen	4.82

and agree perfectly with those furnished by acetosalicycle.

I had already demonstrated in a memoir published some years ago on the benzoic and salicylic combinations, that chloride of benzoile

produces by its reaction on hydroguret of salicyle a compound similar to the foregoing, very clearly crystallised, the formation of which is observed in the dry distillation of benzoate of copper, and which is known under the name of *para salicyle*.

The chlorides of cumyle, anisyle, and succinyle, behave in an analogous manner, and form compounds comparable to the foregoing, which I shall designate under the name of *anno-salicyle*, *aniso-salicyle*, and *succino-salicyle*. These compounds are completely neutral, and perfectly similar to aceto-salicyle.

The chlorides of the organic radicals, in reacting on hydroguret of salicyle, give entirely neutral compounds comparable to the compound ethers. The neutrality of these products appears a very strange fact, and one of which no rational explanation can be given, when we see oil of bitter almonds, a completely neutral substance, generate in the same conditions acid compounds.

The compounds obtained by the reciprocal action of hydroguret of salicyle and organic chlorides, corresponds, as is evident, to those curious products whose formation M. Gerhardt noticed in the reciprocal action of these chlorides, and of the salicylates of methyle and ethyle.

The salicylic element, and the complementary product, acetyle, benzoile, and cumyle, do not appear to be simply juxtaposed, as occurs in the formation of aceto-benzoic and cumo-benzoic acids; but indeed they are so blended and penetrated in so profoundly intimate a manner, that the individuality of each of them has completely disappeared in the products which result from the reciprocal action of the substances put in contact; contrary to what is observed with the ethers, the fatty bodies, the amides, and many other compounds which are capable of reproducing, by fixation of water, the bodies which, by their mutual action, have concurred to their formation. Chloride of cyanogen reacts on salicyluret of potassium, producing cyanide of salicyle, a body endowed with very feeble basic properties, which presents the most complete isomerism with isatine, but from which it differs entirely in properties.

I shall conclude this Note by noticing the existence of iodides of radicals of the acetic group, analogous to the chlorides and bromides which these radicals form. These compounds are produced in the distillation of iodide of phosphorus with acetate, butyrate, or valerianate of potassa, carefully dried; brownish liquids are thus obtained, fuming in the air, which a new rectification and agitation with mercury decolor almost entirely.

The first of these products, iodide of acetyle, boils between 104° and 105° C. ($219^{\circ} 2'$ and 221° , F.); it is very dense; falls to the bottom of water in which it is decomposed, giving acetic and hydrochloric acids; it vividly attacks alcohol, producing acetic ether.

Iodide of butyryle boils between 146° and 148° C. ($294^{\circ} 8'$ and $298^{\circ} 4'$ F.); that of valeryle at 168° C. ($334^{\circ} 4'$ F.) These are dense liquids which alcohol and water decompose in the same manner as the foregoing products.

The composition of these various substances is expressed by the formulæ :—

$C^4 H^3 IO^3$, iodide of acetyle ;

$C^8 H^7 IO^3$, iodide of butryle ;

$C^{40} H^9 IO^3$, iodide of valeryle.

Comptes Rendus, No. 24, June 15, 1857.

CONTRIBUTIONS to the HISTORY of some ORGANIC BODIES.

By M. V. DESSAIGNES.

(Concluded from page 658.)

Hippuric Acid.

I HAVE made known a mode of forming hippuric acid by the contact of the chloride of benzoile and zincic glycocole. I have since obtained the same acid, by heating in a sealed tube equal equivalents of benzoic acid and sugar of gelatine. At $150^{\circ} C.$ ($302^{\circ} F.$), there is no reaction, and the benzoic acid acquires only a red tint; but at $160^{\circ} C.$ ($320^{\circ} F.$), the water begins to trickle in the tube, which announces the commencement of the reaction. I kept up this temperature for twelve hours; the contents of the tube were heated with hot water, which left undissolved a substance in fine, light, white powder, without taste, and which was partially soluble in caustic potassa. This matter requires further examination; by cooling, it allowed about three-fourths of the benzoic acid experimented on to crystallise. In the spontaneously concentrated mother liquor, I obtained a mixture of benzoic and hippuric acids, from which I extracted the latter acid in long and very pure prisms, which I submitted to various trials, which convinced me of their identity with the natural acid. The last mother liquors did not appear to me to contain sugar of gelatine.

Mr. Horsford has pointed out a reaction of sugar of gelatine which consists in a red coloration which it produces by projecting it on potassa in fusion. Other chemists have not confirmed this character, which is, indeed, very difficult to make evident; to succeed, the potassa must be heated very little below its fusing point, and moreover it must be monohydrated. Hippuric acid, with the same precautions, presents the same red coloration.

Notwithstanding the numerous and very good processes of preparing hippuric acid which have been published of late years, I do not think it useless to tell, in few words, the method which has long enabled me easily to obtain this acid pure. Urine, after sufficient concentration, was left to repose for twenty-four hours, then decanted and precipitated cold with hydrochloric acid; the mass thus obtained was allowed to drain on a funnel, then gradually pressed down with a glass rod until it had acquired great firmness. It was then a very permeable crystalline powder, which was rapidly washed until it had

only a slight violet tint. The acid thus washed was completely purified by a single crystallisation with a little animal charcoal.

Nitro-Tartaric Acid.

Nitro-tartaric acid, which is very quickly altered in water, remains dissolved without decomposition in absolute alcohol. The solution in evaporating spontaneously in a flat-bottomed capsule, yields isolated prisms, sometimes of very large size. The following are some of the reactions of this pure acid:—with acetate of potassa it gives an abundant crystalline precipitate; it is the same with ammonia added by degrees. With nitrate of silver, and with acetate of lime, nothing is precipitated at first, but after a little time crystals appear. The nitrate of mercury and the subacetate of lead give a flocculent precipitate. The acetate of lead, the chloride of mercury, the chloride of iron, the sulphate of magnesia and the nitrate of copper do not precipitate.

In order to prepare a neutral salt without decomposition, I neutralised a solution of pure acid gradually with ammonia, maintained at the temperature of 32° ; by adding to this neutral salt a similar quantity of free acid, the acid nitro-tartrate is obtained in brilliant prisms.

The neutral nitro-tartrate of ammonia precipitates the nitrate of silver, the nitrate of mercury and the chloride of iron; it feebly precipitates the acetates of lead and lime.

I analysed the pure acid dried in vacuo; the nitrogen was determined by the method of M. Dumas.

	I.	II.	III.	Calculation.
C . .	20.51	19.98	20.17	C ⁸ . 20.00
H . .	2.18	1.84	1.81	H ⁸ . 1.66
N . .	11.10			N ⁴ . 11.66
				O ²⁰ . 66.68
				100.00

In the salt of silver incompletely washed (it is altered in water) pressed strongly and dried in vacuo, I found 46.51 per cent. of silver, estimated under the form of chloride; the formula C⁸H⁴Ag²N⁴O²⁰+2Aq requires 46.65 of silver.

The acid salt of ammonia dried in vacuo, in which it is partially decomposed, gave by analysis: C=17.92; H=3.19 per cent. The ammonia, estimated by Schlessing's process, weighed 6.33 per cent.; the formula C⁸H⁸N⁴O²⁰, N²H⁶, requires C=18.67 H=2.72, and N²H⁶=6.61.

Nitro-Racemic Acid.

To prepare nitro-racemic acid, racemic acid must be completely dishydrated, pulverised, sifted, rapidly dissolved in warm monohydrated nitric acid, decanted immediately to remove the undissolved acid, and half its volume of sulphuric acid must be added to the

liquor. A firm substance is produced, which, dried between two bricks, under a bell-glass, becomes a white, silky and light mass. This crude acid was dissolved to saturation in warm water, filtered, cooled to 32°, and small, finer and shorter prisms are obtained than those of nitro-tartaric acid, and which, dropped on paper, acquire a pearly aspect. The aqueous solution of this acid soon disengages gas, and one of the products of its decomposition is the acid which I have called tartronic acid. It is dissolved without decomposition, and by spontaneous evaporation it crystallises in small discs formed of microscopical crystals. It precipitates the subacetate of lead; it does not precipitate the acetate of potassa, lime and lead, nor the nitrate of silver; it does not exert any action on polarised light. Nitro-tartaric acid, on the contrary, possesses the rotatory power (these two observations are due to M. Chautard.) Finally its salt of ammonia treated by hydrosulphate of ammonia, regenerates racemic acid and not tartaric acid.

Benzo-Tartaric Acid.

I heated to 180° C. (302° F.) in a tube sealed in the lamp, a mixture in equal proportions of tartaric and benzoic acids. They fuse at once without mixing, and finally produce a single liquid of a brown color. I dissolved the whole in hot water: by cooling a large portion of the uncombined benzoic acid crystallised. The mother liquor was evaporated to dryness, and the residue was incompletely dissolved in a concentrated solution of carbonate of soda. To the liquor filtered and decolored with a little charcoal, a slight excess of hydrochloric acid was added. At the end of a short time, tubercles were formed, composed of microscopical crystals, the appearance of which was not changed by subsequent crystallisations. It is an acid whose characters are intermediate between those of the acids of which it is formed. It is more soluble in cold water than benzoic acid, but it is less so in alcohol; its aqueous solution has no odor when heated. Submitted to the temperature at which benzoic acid fuses and sublimes, it remains without alteration. At a great heat, it fuses, and evolves vapors which condense on any cold body in shining bractæ, which are pure benzoic acid; the mass which remains after the disengagement of these vapors turns brown, and exhales the odor which characterises super-heated tartaric acid.

The new acid in solution saturated cold, does not precipitate the chloride of iron: if the solution is saturated hot, it precipitates it slightly. It gives no precipitate with lime water added to saturation; it does not precipitate nitrate of silver; it slightly precipitates acetate of lead in concentrated solution: it gives crystals with acetate of potassa neutralised by ammonia; it gives a slight pale yellow precipitate with chloride of iron and does not precipitate chloride of calcium.

In a solution saturated to one-fourth with ammonia, the nitrate of silver determines a precipitate which re-dissolves at first. By cooling, it deposits a white powder, which does not re-dissolve, even on being

heated. This salt, washed and dried, gave by calcination 46.35 per cent. of silver. The formula $C^{22}H^{16}Ag^2O^{14}$ requires 46.15. This acid is then benzo-tartaric acid. Indeed, $C^{14}H^{12}O^4 + C^8H^{12}O^{12} - H^4O^2 = C^{22}H^{20}O^{14}$.

Malic Acid.

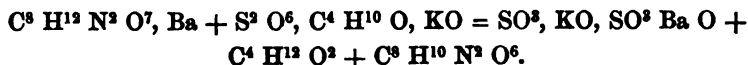
Malic acid, heated with hydrochloric acid, is easily dishydrated, and forms fumaric acid. I tried the inverse transformation with the same reagent. I first ascertained that the fumaric acid destined for the experiment did not contain any trace of malic acid, by crystallising it several times and evaporating the mother-liquors, which produced only homogenous and not deliquescent crystals. This acid was placed with an excess of concentrated hydrochloric acid in a tube sealed in the lamp, and I heated it for at least 140 hours at 100° C. (212° F.) I afterwards evaporated the whole to dryness, redissolved it in a little water, and separated the fumaric acid as much as possible. The last mother-liquor was a deliquescent syrup, a portion of which, dried at 100° C. (212° F.) was easily fused in a tube, and was converted into fumaric acid by dry distillation. The other portion was half saturated with ammonia, and I extracted from it bifumarate of ammonia, precipitating the chloride of iron, thin large limpid prisms which no longer precipitate the chloride of iron, even after the addition of ammonia. These same prisms fuse at 160° C. (320° F.); they are converted into a reddish, insoluble matter; which bimalate of ammonia produces at the same temperature, and which is called, perhaps incorrectly, fumarimide. Bifumarate of ammonia, on the contrary, does not fuse even at 200° C. (392° F.) There is therefore formed a small quantity of malic acid at the expense of the fumaric acid. Perhaps we should succeed better by employing a higher temperature.

I have obtained, by chance, superb prisms of malate of lead, two or three centimetres long, and three or four millimetres wide. To prepare malic acid on rather a large scale, I precipitated with acetate of lead, a still hot solution of bimalate of lime. The precipitate was very dense; not resinous, however, and, contrary to my expectation, at the end of a few days it did not crystallise. After being left to itself for several months, there were formed here and there cavities, each containing a limpid crystal. The cavities increased in number and dimensions, the crystals grew larger, and, after two years, the precipitate was converted into a mass of crystals, all of the volume above mentioned.

● *Aspartic Acid.*

I have tried to prepare aspartic ether, by heating on an oil bath, equal equivalents of dry aspartate of baryta and sulphovinate of potassa. The distilled product consisted of alcohol, and the retort contained sulphate of potassa, which I removed with water, and sulphate of baryta and a reddish matter, which I dissolved with hydrochloric acid, and precipitated with water. This insipid matter, insoluble in water, gave, by being boiled with hydrochloric acid, hydrochlorate of

aspartic acid. It was then the same body which is formed by the dry distillation of the bimalate of ammonia. Its formation is explained by the following equation :—



Aconitic Acid.

Natural hydrochloric acid is converted into succinic acid by fermentation, which is the consequence of its isomerism with maleic and fumaric acids. As it differs essentially from these latter acids in the products of its dry distillation, as well in the free state as combined with ammonia, I have thought that it was not useless to verify once more its metamorphosis into succinic acid. This time I used aconitic extracted from citric acid and I succeeded, as well as with the acid of the equisetum, in converting it into succinic acid by fermentation.

Journal de Pharmacie, July, 1857.

ANALYTICAL CHEMISTRY.

ANALYSIS of BONES. By M. H. BONNET.

HITHERTO many analyses have been made, with the view of arriving at a knowledge of the chemical composition of bones. At first sight, this seems easy; but when we examine the question deeply, we are struck with the difficulties we meet with. The numbers which have been given are far from being correct. Rees is perhaps the only person whose analyses are of any value.

The principal error arises from the difficulty of separating the phosphate of lime, Ph O^5 , 3 Ca O, the formula of which was given by M. Raewski and the phosphate of magnesia, Ph O^5 , 3 Mg O, which behave in exactly the same manner with reagents.

There exist these two erroneous points; one of little consequence, the other, on the contrary, causes incorrect results to be given.

1. It has been desired to indicate the quantity of water contained in a bone. I consider this quite impossible. We are obliged, after having taken a bone from a dissecting room to wash it with abundance of water to free it from fat, vessels, &c., which do not form an integral part of it. Consequently, a proportion of water which has been introduced will be reckoned in the analysis; or else, if we do not subject it to the operation of washing, we have the water of the fat, of the vessels, &c. It would therefore be better to abstain than to persist, when we perform an analysis, in a visible error.

2. The next point is more serious. When we have weighed a dried bone, and after having exposed it to the heat of a muffle, it is removed, incinerated, it is weighed again; this is supposed to give the weight of the mineral matter, whilst the difference of the two weights is said to give the weight of the organic matter. This is fallacious; for, in

order to burn away all the organic matter, it is necessary to push the heat very far; there is removed, consequently, at the same time from the mineral matter, carbonic acid, of which no account is taken. The cypher of the last weight is therefore erroneous; and so, consequently, is that given as the difference of the two weights.

This done, let us see how we should operate. We take a rundle of bone and submit it to a current of water, under a powerful jet of a stop-cock, so that, penetrating with virulence into the canalicules, this current may remove the fat, &c. Notwithstanding this, some still remains, which may easily be seen with the microscope, and it is only after having macerated it in alcohol and ether, that there remain nothing but the integral parts of the bone. After these operations, we dry at 74° to 80° C. (165° 2' to 176° F.) in a Gay-Lussac's stove. It is allowed to cool, weighed, and we have a certain weight D.

The rundle of bone is placed in a small capsule, at the heat of a muffle, long enough for the organic matter to be burned away. It is weighed again and we have the weight D'. It is dissolved in pure hydrochloric acid, which dissolves the phosphate of lime and magnesia, and converts the lime of the carbonate into chloride of calcium.

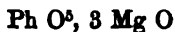
All the phosphoric acid of the two salts is precipitated by chloride of iron, in presence of acetate of soda, in the state of phosphate of iron. By means of the equivalents, by a simple proportion, we know the proportion of phosphoric acid. It belongs to the two bases. Let R be this quantity.

I collected the precipitate and washed it, and uniting the washing waters, we found that we had in the liquor, chloride of calcium (arising from the phosphate and the carbonate) and chloride of magnesia, plus the chloride of iron introduced.

I got rid of the iron by treating by ammonia, and I had oxide of iron, which I washed in order to remove the chlorides of calcium and magnesium in solution, and which remained interposed in it, and I collected the mother-liquors which I analysed.

I precipitate with oxalate of ammonia, all the lime of the chloride in the state of oxalate of lime, which I collect and wash on a filter, *previously weighed*. I dry and weigh the whole. The difference between this last weight and the weight of the filter, gives me the quantity of oxalate of lime; but, however, as the oxalate of lime is not stable when heated, it is better to burn it with the filter in a platinum capsule, to treat it with sulphuric acid, to evaporate to dryness and weigh the sulphate of lime formed, taking account both of the weight of the capsule, and of a few thousandths of ash of the filter. By means of the equivalents, knowing the sulphite of lime, I have the lime. It belongs to the phosphate and the carbonate. Let it be P.

There remains chloride of magnesium in the liquor. I precipitate the magnesia by ammoniacal phosphate of soda in the state of ammonio-magnesian phosphate, which I collect, wash and filter. I weigh:—by the equivalents, I have the magnesia. Knowing the latter, and knowing the formula



of phosphate of magnesia of the bones, I know how much phosphoric acid it requires. I have then this phosphoric acid by a proportion, and consequently, the phosphate of magnesia itself.

Knowing this quantity of phosphoric acid, I subtract it from the total weight R. The difference $R - R' =$ the phosphoric acid of the phosphate of lime, and we have consequently, the phosphate of lime itself.

Knowing now the phosphate of lime by the proportions, I know the lime. Let it be P' . I subtract it from the total weight P and $P - P' =$ the lime of the carbonate; we have consequently that of the carbonate itself.

Let d be the carbonic acid of the latter. I add to the weight D' , the result of the incineration, the weight of the carbonic acid which the heat of the muffle had removed from the carbonate, and I have—

$D' + d =$ the weight of the mineral matter of the bone,
and

$D - (D' + d) =$ the weight of the organic matter,
and finally,

$$D = D' + d + D - [(D' + d)].$$

Journal de Pharmacie et de Chimie, August, 1857.

NEW TEST for GRAPE SUGAR. By M. BOETTGER.

THIS process is especially applicable to the analysis of diabetic urine. After the urine to be tested has been introduced into a test-glass, its own bulk of a solution of carbonate of soda, of the following composition is added to it:—

Crystallised carbonate of soda		1 part.
Water		4 „

Then as much subnitrate of bismuth as will lay on the point of a knife, and the whole is boiled; if the liquid is free from diabetic sugar the subnitrate of bismuth remains white, whilst it blackens more or less, however little sugar of the glucose species it may contain.

Cane sugar has no action in this case. Moreover, the author has ascertained, that the reaction which we have just noticed, belongs only to glucose and its varieties, and that, at all events, it is not due to any of the principles ordinarily contained in the urine, whilst it has been well proved that the alkaline tartrate of copper is reduced by uric acid.

Journal für Praktische Chemie.

ESTIMATION of the MORPHIA in OPIUM. By M. FORDOS.

THE estimation of the morphia in opium, is very interesting in a medical point of view. It is to the presence of this alkaloid, which is endowed with a very energetic action on the animal economy, that

opium owes, if not all its properties, at least those which the physician seeks in its employment. Now, the opiums which are met with in commerce have a very variable composition; the quantity of morphia which they contain may vary from 0 to 14 per cent., and even higher for indigenous opium. This shows the uncertainty which the employment of this substance would present, if its richness in morphia were not determined by analysis, prior to its use in medicinal preparations. A very great number of analytical processes have already been published; but all present practical difficulties which sometimes stand in the way of a satisfactory estimation. Thus the question has just been put to competition, by the Academy of Medicine of Belgium.

The process of estimating morphia which I now describe, appears to me of more easy execution, and it furnishes a more accurate result.

Fifteen grammes of opium cut into thin slices, are allowed to macerate in 60 grammes of water, taking care to agitate occasionally. After 24 hours, the product of the maceration is poured into a mortar, in order to divide the opium carefully by means of the pestle. The whole is then poured on a small filter, and when the liquid has passed through, the filter is washed with 15 grammes of water, with which the mortar and the flask in which the maceration was conducted had been washed. The washing is repeated a second and third time with ten grammes of water at each time. The opium is then sufficiently exhausted.

One-third of the liquor is taken for determining the quantity of ammonia necessary for the precipitation of the morphia. The ammonia is added drop by drop, by means of a graduated burette, and we stop as soon as the liquor presents a slight ammoniacal odor; the quantity of ammonia absorbed is noted.

The estimation of the morphia is then conducted with the other two-thirds of the liquor, which represent ten grammes of opium. To these liquors is added their own bulk of alcohol of 85 degrees, and double the quantity of ammonia required in the first experiment. It is necessary to add a slight excess of ammonia, in order to obtain the complete separation of the morphia. The liquor is agitated and left to itself in a well-corked flask. It very soon deposits crystals—some in fine needles, scarcely colored—the narcotine; the others in prisms—more bulky and rather deeper colored—the morphia. After two or three days, the flask is agitated, and again left to repose for several hours, in order to give the morphia time to precipitate completely. The crystals are then collected on a small filter, and washed with 15 or 20 cubic centimetres of very weak alcohol, at 40° C. (104° F.); this washing removes the mother-liquors, and at the same time frees the crystals from the coloring matter with which they are soiled. Nearly colorless crystals of morphia, and quite colorless crystals of narcotine remain on the filter. The filter is allowed to dry on the funnel itself; ten cubic centimetres of sulphuric ether are then poured on the filter, and then, at twice, 10 or 15 cubic centimetres of chloroform. The crystals of narcotine are dissolved instantaneously in chloroform, and are removed with it. The chloroform does not touch

the morphia. Finally, the filter is washed with 15 cubic centimetres of ether, to remove the last traces of chloroform and narcotine. The filter is dried and the crystals of morphia, which are then very easily detached, are weighed.

In the process which I have just described, by treating the opium with water, all the morphia which is found in the state of saline combination is very well dissolved; only very little narcotine and coloring and resinous matters are dissolved. If we added ammonia to the aqueous solution, we should obtain a dirty precipitate of morphia, narcotine and coloring matters. The addition of alcohol has the effect of retarding the precipitation of the alkaloids, and of giving them time to take the crystalline form again; this alcohol retains in solution the coloring and resinous matters, and very slightly colored crystals are obtained. The washing of the morphia with chloroform is a very simple operation, which completely separates all the narcotine.

Comptes Rendus, No. 24, June 15, 1857.

NOTE on a MEANS of DETECTING the ADMIXTURE of WOOL with SILK,
and of DETERMINING its PROPORTION.

By M. Ch. BARRESWIL.

FOR several years I have used a chemical test for distinguishing wool from silk. I employ the nitric acid of commerce, which, when cold, rapidly dissolves silk and does not attack wool. I have not published this process sooner, because, until lately, there was scarcely any occasion for applying it. The eye alone sufficed for recognising the presence of silk, and the estimation was made by separating the two matters mechanically, one of which composed exclusively in the tissues the warp (in the state of silk flock) or the *broché*. But new tissues are made in which there is such a mixture of matters that there is no means of picking out the filaments, and that the eye alone is no longer sufficient without the aid of the microscope for distinguishing silk from wool.

When we have, for example, a tissue, the woof of which is wool, and the warp wool with admixture of silk flock, the warp and the woof are separated, and the latter is successively treated with nitric acid, water, ammonia, and finally, water for washing; this treatment is repeated twice if we desire to be certain of a complete separation. We operate on the warp in the same manner, taking all precautions to lose none of the matter, then the two samples are dried and weighed. The loss suffered by the woof is noted, and we subtract from the loss undergone by the warp a proportional weight; this loss represents dye dressing, &c.

It may be objected to this process, that it presents the danger of considering as silk foreign matters which are introduced into it, and which although quite different from silk, would have in common with it the property of being dissolved in nitric acid. This criticism is just, but the same objection attaches to all processes of estimation

by the difference. The employment of nitric acid does not dispense, moreover, with the microscopical examination, which is an excellent means of control.

We shall arrive at a more certain result by previously submitting the tissue to several treatments with water, weak alkali, the dilute acids, alcohol and ether, in order to remove, as much as possible, the foreign matters from the filaments before separating the warp from the woof.

I may add that the merit of commercial tests often consists in their quickness. When it is desirable to obtain a very prompt result, we hasten the dessiccation, which is the longest operation, by washing the filaments rinsed with water, and compressed, first with concentrated alcohol, and then with anhydrous ether. The ether is dissipated in a few minutes, and the small quantity of water left on the filaments is very soon evaporated.

While on this subject, I may mention a very interesting practical fact. The separation of wool from cotton is now effected industrially.

The following are the circumstances under which it is done. Old knitted rags, woollen tissues and even cloth are reduced to the state of filaments, and are employed again in the manufacture of tissues under the name of revival (*renaissance*). It happens that in these filaments a little cotton, flax or hemp may be mixed with the wool, and as these matters will not take the dye in the same conditions as the wool, we are apt to find in woollen tissues mixed with renaissance ("devils dust") undried threads which injure the appearance of the tissues and expose the origin of the matters. Cotton and similar matters are removed by the action of boiling dilute hydrochloric acid, which dis-aggregates and crumbles them, and does not perceptibly attack the wool. The cotton reduced to powder, so to speak, is separated mechanically either by agitating the wool in water, or by beating when dry as with an inert powder. By aid of the means just indicated, and of those already known, it is possible to make the analysis of a tissue composed of cotton, wool, and silk.

Treatment with nitric acid dissolves the silk, the action of potassa destroys the silk and wool, and hydrochloric acid removes the cotton. We may, consequently, according to circumstances, vary the processes of testing, and check the results by two different methods.

Journal de Pharmacie, August, 1857.

On the ESTIMATION of COPPER.

A PAPER on this subject has been recently read before the Chemical Society by Mr. E. O. Brown, assistant in the chemical establishment of the War Department. The process described by Pelouze, which consists in dissolving one gramme of copper in nitric acid, mixing this solution with a large excess of ammonia, raising it to the boiling point, and dropping in a standard solution of sulphide of sodium until the blue color disappears, and a precipitate is thrown down having

the composition CuO , 5 CuS , the author found to give results which sometimes vary considerably. He remarked that whenever the contents of the flask were not shaken, but only agitated by boiling, a smaller amount of sulphide of sodium was required. He also experienced considerable difficulty in deciding on the exact point at which the blue color disappeared, since, on agitating the liquid a short time after decoloration, the color was restored, and this continued to be the case for a considerable period after its first disappearance.

These changes the author attributed to a portion of the oxysulphide of copper becoming oxydised at the expense of the salt of copper in solution, reducing the latter to the state of suboxide, thereby causing the blue color to disappear before the whole of the copper is precipitated; from the great affinity of an ammoniacal solution of suboxide of copper for oxygen, agitating the liquid must necessarily give rise to a constant reproduction of protoxide of copper. A portion of the sulphur of the sulphide of sodium also passing into the solution, as hyposulphurous acid, or some other low oxide of sulphur, lessens the effect which the sulphide would otherwise produce.

The process proposed by Parkes, which consists in decolorising the ammonio-nitrate of copper with a standard solution of cyanide of potassium, the author found to be equally fallacious. The quantity of cyanide necessary to produce complete decoloration, being in proportion to the length of time taken in the operation.

From a number of experiments which were made by the author, the reaction of iodide of potassium upon a salt of copper, which results in the production of subiodide of copper and free iodine, appeared likely to be available for this purpose. The amount of iodine thus liberated being determined by means of sulphurous acid. After further experiments, it was found, however, that the use of sulphurous acid would dilute the solution to an inconvenient extent; hyposulphite of soda was therefore substituted, and found to furnish results equally accurate. The process finally recommended by the author is as follows:—

Ten grains of copper are dissolved in dilute nitric acid, and the solution boiled until free from nitrous acid: it is then diluted with about an ounce of water, and carbonate of soda added until a portion of the copper is precipitated. Pure acetic acid is then added in excess, and the solution poured into a flask capable of holding about 12 ounces. About 60 grains of iodide of potassium are then thrown into the flask, and sufficient time having been allowed for the crystals to dissolve, a standard solution of hyposulphite of soda is added, until the greater part of the iodine has disappeared, which is indicated by the liquid changing from a brown to a yellow color. A little clear solution of starch is then added, and the addition of the hyposulphite of soda cautiously continued until no further effect is produced. The bleaching effected by the last portions of hyposulphite may be best seen by allowing the drops to fall into the centre of the flask whilst the liquid is in motion, when streaks of a lighter color will be produced so long as any iodine remains.

When the quantity of iodide of potassium employed is less than six times the weight of the copper to be determined, satisfactory results are not obtained. Care must be taken that the iodide of potassium employed is free from iodate of potash, and that the acetic acid contains no sulphurous acid. The solution of starch should be made by boiling with a large quantity of water, any undissolved portions allowed to subside, and the clear liquid only used. The solution of hyposulphite is obtained by dissolving 4000 grains of the salt in two gallons of water, and standardised by means of electrotyped copper.

Several experiments were made in order to ascertain to what extent the results obtained by this process are liable to error from irregular manipulation or changes of temperature; from these it became evident that slight differences in the mode of manipulating do not in any way affect the results, so long as the solution is not heated.

The presence of tin, lead, iron, or zinc in small quantities is also immaterial; but when the alloy in which the copper is to be determined contains a large proportion of lead or iron, these metals should be previously separated from the solution, since the yellow iodide of lead and the red color of the acetate of iron, render it difficult to distinguish the effect produced by the last quantities of hyposulphite of soda.

Pharmaceutical Journal, August 1, 1857.

INORGANIC CHEMISTRY.

NOTE on the FORMATION of ARSENITE of AMMONIA.

By M. V. DE LUYNES.

To obtain arsenious acid in the state of purity, Berzelius gives the following process:—

The acid of commerce is agitated for several hours, at a temperature of 70° to 80° C. (158° to 176° F.) with ammonia, in a close vessel. The liquor is left to repose for a few minutes; then the limpid portion is decanted while hot into another flask. On cooling, it deposits, according to Berzelius, octohedral crystals of arsenious acid not containing ammonia.

By repeating this preparation, I have indeed obtained, by the cooling of the liquor, an abundant deposit of crystals; but as they did not present the properties of arsenious acid, I examined them carefully, with the following result. These crystals having been left in contact with the mother-liquor in an open vessel, gradually dissolved, and finally disappeared entirely. The ammonia was slowly disengaged into the air. After some time, the liquor had not the least ammoniacal odor, although possessing a strongly alkaline reaction. Finally, after a very long time, a second deposit of crystals was formed, less transparent than the first, and having the form of octohedra. This was pure arsenious acid, not containing ammonia. I recommenced the experiment, setting aside the crystals which were formed in the first

place. These crystals occurred under form of well determined prismatic needles. I could not perceive any octohedron. These crystals had a strong ammoniacal odor. After having allowed them to drain for a few minutes on unglazed porcelain, I dried them rapidly between paper, and submitted them to analysis. They contain in hundredths :—

Water	8.67
Ammonia	13.40
Arsenious acid	77.93
	100.00

Arsenite of ammonia contains :—

Water	7.20
Ammonia	13.60
Arsenious acid	79.20
	100.00

These crystals are then arsenite of ammonia and not arsenious acid, as stated by Berzelius. Arsenite of ammonia, according to M. Pasteur, who first obtained it, decomposes rapidly in the air. This is why, notwithstanding the precautions which I have indicated, we always find an excess of water and a loss of ammonia, as is seen from the numbers given above.

The first crystals which are deposited being arsenite of ammonia, it is easy to explain their solution in the mother-liquor. Indeed, M. Pasteur ascertained that arsenite of ammonia was much more soluble in pure water than in ammonia. It will be understood then, that the water losing its gas in contact with the air, the solubility of the salt increases, and that it may then become completely dissolved; then the ammonia, being entirely disengaged, there remains only a solution of arsenite of ammonia, which is decomposed slowly in the air, giving rise to a deposit of octohedral arsenious acid.

It is evident from the foregoing, that the final result of the preparation indicated by Berzelius is, indeed, octohedral arsenious acid; but the deposition of this compound is preceded by the successive crystallisation and solution of the arsenite of ammonia, which appears only as an intermediate product, and the formation of which I have thought it interesting to notice.

Comptes Rendus, No. 26, June 29, 1857.

NOTE on some PROPERTIES of ARSENIUS ACID.

By M. V. DE LUYNES.

WHEN a mixture of arsenious acid and hydrochlorate of ammonia is heated, ammoniacal gas is set free; it is the contrary of what occurs when an acid is made to act on a chloride.

To make this experiment, the mixture of the two substances

powdered is introduced into a flask fitted with a tube, and heat is applied. An abundant disengagement of ammoniacal gas is produced, and it may be collected over mercury. After some time, the bubbles of gas appear to be charged with white fumes, then the disengagement stops. There are, then, two distinct phases in this reaction. During the first, the ammoniacal gas is disengaged; during the second, the disengagement ceases. It may appear singular, at first sight, that an acid should expel ammonia from its combination. But it is known that arsenious acid and hydrochloric acid, in reacting on each other, give water and chloride of arsenic.



It is, indeed, on this reaction, that a mode of preparing chloride of arsenic is founded. Now, in the foregoing experiment, something analogous takes place. Indeed, if we examine the flask which contains the mixture when the disengagement of gas has ceased, we perceive at once that a large portion of the arsenious acid has condensed at the upper part of it. This is owing to the arsenious acid being more volatile than the hydrochlorate of ammonia. This difference of volatility prevents the maintenance of the two substances in contact for rather a long time, and necessarily abridges the duration of the gaseous disengagement; but it is not the sole cause of the cessation of that disengagement. On opening the flask, after cooling, white fumes, analogous to those which chloride of arsenic gives, and which strongly redden litmus paper, issue forth. By treating the solid residue with water, the white vapors disappear, and we obtain a liquor which acts on tincture of litmus like the powerful acids. This action is due to the hydrochloric acid which results from the decomposition by the water of the chloride of arsenic produced during the experiment. Thus, then, during the first part of the reaction, the ammonia is eliminated on account of the affinity of arsenious acid for hydrochloric acid; or chloride, or perhaps oxychloride, of arsenic is formed. The proportion of the latter compound becoming more considerable, an inverse reaction is produced which is opposed to the continuation of the reaction. Arsenious acid acts then on hydrochlorate of ammonia like the metallic oxides; the ammonia is set free, and chloride and water are produced.

Comptes Rendus, No. 26, June 29, 1857.

On the INFLUENCE of NASCENT HYDROGEN on AMALGAMATION.

By M. L. CAILLETET.

In the memoir which I have the honor of presenting to the Academy, I propose to examine the conditions which determine the amalgamation of metals, such as iron, platinum, and aluminium, which ordinarily resist the action of mercury.

I think, indeed, that I have demonstrated that we may obtain, by several processes, a deposit of mercury on the metals I have named,

and that this amalgamation seems to be connected with a peculiar property of hydrogen in the nascent state.

In the experiments which form the subject of my memoir, and of which I can here give only a summary, I used the ammoniacal amalgam discovered by Seebeck, and the study of which was completed by the remarkable works of MM. Gay-Lussac and Thenard.

By agitating the ammoniacal amalgam with a sheet of iron or of one of the metals which I have mentioned, the ammoniacal compound is destroyed, disengaging hydrogen and ammonia, and the plate is covered with mercury. The amalgam of sodium produces the same phenomenon, only the action of water is necessary. By covering the very dry amalgam with a layer of naphtha, the deposit did not take place; a drop of water sufficed for causing the adherence of the mercury. The disengagement of hydrogen which takes place in the foregoing experiments led me to investigate whether there would not be precipitation of mercury on all the metals which I have mentioned in all the cases in which hydrogen is generated. By plunging into a vessel containing mercury and acidulated water, the two platinum electrodes of a battery capable of decomposing water, so that the positive plate may be in the acidulated water and the negative plate in contact with the mercury, we perceive that as soon as the bubbles of hydrogen appear on that plate, the mercury is fixed as in the foregoing experiments.

Amalgamated aluminium decomposes water acidulated with pure sulphuric or nitric acid with great energy, there is a disengagement of hydrogen and a salt of alumina remains in solution. I have not tried to amalgamate manganese, nickel and cobalt, but I have proved that their ores, arsenical nickel and cobalt, oligist iron and sulphuret of iron might be moistened with mercury. Temperature seems to have no influence on the mercurial deposit. Experiment demonstrates that in presence of hydro-sulphuric acid, chlorine and phosphuretted hydrogen, amalgamation does not occur.

In investigating the causes of the foregoing experiments, it will be seen that the electricity brought into play could not account for them, since by decomposing by means of the battery a salt of copper, lead or silver, in presence of mercury, we obtain, if the current is of average power, oxygen at the positive pole and reduced metal at the negative. In my hypothesis of the efficacious action of nascent hydrogen on amalgamation, I ought not to obtain the mercurial deposit in this experiment: this is indeed what the result proved, and the copper, lead or silver is deposited on the plate without its being amalgamated. If we repeat the foregoing experiment by using a salt of mercury, we still see the oxygen disengaged at the positive pole, and the mercury deposited on the negative plate. If the current acts without a disengagement of hydrogen, the revived mercury is deposited in shining globules which do not adhere to the negative plate. By making nascent hydrogen intervene, the amalgamation is instantly determined; for this purpose the plate thus covered with mercurial globules is used on the negative plate of a voltameter, the water is

decomposed; the moment the hydrogen appears, the globules change form, and spread, and the plate is completely amalgamated.

If we pour into a voltameter in which water is acidulated with nitric acid, a few drops of nitrate of mercury, so as to obtain at the same time on the negative plate mercury and bubbles of hydrogen, we remark, not without astonishment, that the mercury is no longer under the form of globules, but that it has amalgamated the negative electrode.

In summing up the experiments which I have just described, it seems to me that it may be concluded that electricity alone is insufficient for determining the amalgamation of the metals which ordinarily resist the action of the mercury, whilst all the facts which I have presented tend on the contrary to prove that this amalgamation requires the presence of hydrogen in the nascent state, and that it could not occur in presence of another gas. Further experiments will probably show what is the mode of action, hitherto unknown, of nascent hydrogen on amalgamation.

Comptes Rendus, No. 24, June 15, 1857.

NEW PROCESS for PREPARING INFLAMMABLE PHOSPHURETTED HYDROGEN.

By M. BOETTGER.

THIS process, which is completely free from danger, is based on the action which cyanide of potassium in powder is capable of exerting on hydrated phosphuret of copper obtained by the humid way. The latter compound is procured by decomposing at the boiling temperature, a solution of sulphate of copper by means of phosphorus; the product constitutes a powder of a greyish black, composed of phosphuret of copper and basic phosphate. This powder is kept under water. When it is dried it may with impunity be mixed with cyanide of potassium; the disengagement takes place only when a little water is added.

The cyanide of potassium cannot be replaced by potassa or soda, and water should not be replaced by dilute alcohol. In the former case, no disengagement occurs; in the second, unflammable phosphuretted hydrogen is developed.

It is known that phosphuretted hydrogen readily blackens solutions of nitrate of silver. M. Boettger applies this reaction to the production of a kind of sympathetic ink; for this purpose, it is sufficient to expose to the disengagement of gas a paper on which characters have been traced in a solution of nitrate of silver; the characters immediately appear black and are very stable, resisting, not only the action of alkaline liquors, of solutions of cyanide of potassium or of hypochlorite of lime, but also the influence of dilute sulphuric, nitric or hydrochloric acid.

The amorphous phosphorus reduces sulphate of copper only in as much as it still contains ordinary phosphorus. The author proposes

to turn this property to account in industry for freeing amorphous phosphorus from the ordinary phosphorus which it may contain, if we do not prefer to have recourse to the very simple and practical process of separation which M. E. Nickles has made known.

The phosphuret of copper in question is composed according to the formula Ph Cu^2 . In the crude state it is mixed with basic phosphate of copper, which does not impede the disengagement of phosphuretted hydrogen, but which may be averted by boiling with a solution of bichromate of potassa acidulated with sulphuric acid.

The phosphuret of copper which resists this latter agent is decomposed, although slowly, when it is boiled with hydrochloric acid; the products of the reaction are unflammable gas and chloride of copper. However, this phosphuret may inflame spontaneously when exposed to the solar rays. This, at least, is what happened to M. Boettger in placing the crude phosphorus in the sun for the purpose of drying it.

Journal für praktische Chemie.

INDUSTRIAL CHEMISTRY.

NOTE on DYEING by SUBSTITUTION. By M. CARLOS KEECHLIN.

If we pass a piece of violet cotton tissue, dyed with madder, into sulphuric acid at 15° Beaumé, wash it with water, and then steep it, thus modified, in aluminate of soda,* to which hydrochloric acid is added, until the precipitate formed is no longer redissolved; the tint becomes almost instantaneously red. The following is the explanation of this interesting reaction. By passing the tissue into an acid, we remove the iron mordant fixed on the stuff *without touching the coloring matter*, which, on being passed into the aluminate, attracts alumina to combine with it and replace the iron which has been removed from it. The quantity of alumina which is fixed is in the direct ratio of the force of the iron mordant which has been removed.

The reds thus obtained may be soaped, but do not resist so well as those produced in the ordinary way.

We may profit by this fact, for realising in printing a species of conversion.

If we print on madder violet with oxalic acid thickened with gum, and if after this impression we wash carefully and pass into aluminate of soda, we have red and rose effects on violet.

Journal de Pharmacie, August, 1857.

* Alum	100 grammes.
Water	0.6 litre.
Caustic soda, 38°	0.2 „

This aluminate, which keeps well, should, at the time of use be saturated with a little acid, until the precipitate of alumina, which is soon formed, is no longer redissolved.

Before the sample treated with the acid is submitted to its action, it must be rinsed sufficiently.

M. PLESSY.

*The IRON and COAL of CONNAUGHT; with a brief COMPARISON
between CREEVELEA and ENGLISH IRONS.*

By WILLIAM ANDERSON, C.E.

THE extraordinary quantity of iron and coal imported into Ireland from the sister countries, would induce any one to suppose that this island has not been endowed with the minerals in question. While the indomitable energy of man is opening vast fields for commerce in the heart of remote continents; while the brilliant achievements of Dr. Livingstone are drawing upon him the eyes of the civilised world, it is not surprising that the efforts of a Mining Association in the west of Ireland should attract but little attention, although its exertions, if crowned with success, will produce a considerable change in the prospects of the country.

Lough Allen is the first of that wonderful chain of lakes forming the Shannon. There, on the borders of the county of Leitrim, to the west of the lake, lies a district of extraordinary interest, not only in a scientific, but in a commercial point of view. Due west of the lake lie the Braulieve mountains, rising some 1100 feet above its level; through them flows the river Arigna, a mountain torrent, emptying itself into the southern extremity of the lough. These mountains at three different altitudes present an outcrop of coal, in parallel and nearly horizontal strata, varying from one to three feet in thickness. From these the coal can be procured at a very small cost, by the simplest mining operations; the peculiarity of position rendering pumping and winding machinery unnecessary. The Arigna detaches and carries down during floods vast quantities of ironstone, strata of which crop out abundantly throughout the district, and for many years the Arigna Works derived their supply of ore from this source alone. About seven miles to the northward are the Creevelea Iron Works, now in active operation. They were established on an extensive outcrop of ore at the foot of Dowbally mountains, and derive their coal from the locality described above. The communication at present is imperfect, but a railway is in contemplation, by means of which the carriage of coal to the Works, and of iron to the shipping pier on the lake, may be accomplished at a very low cost.

It is difficult to conceive a locality more favorably situated for supplying Ireland with the most valuable of metals. Not many miles from Sligo on the one hand, on the banks of the Shannon on the other, it is in direct water communication with Dublin and Limerick; probably, therefore, 9s. or 10s. per ton will cover the cost of carriage to all the principal towns, while the quality of iron that can be produced will insure an eager demand at the current prices of Scotch pigs.

Many years ago, when the Arigna Company was in full operation, this iron was considered as good as any Scotch or English in the market; but from mismanagement, or other causes, they abandoned their works, and it is only very lately that the Creevelea iron has come before the manufacturer. Being anxious to ascertain its quality, I resolved upon comparing it with the list of British irons experi-

mented upon by Mr. Fairbairn, the results of whose experiments may be found in his recent work "On Cast and Wrought Iron." I selected a test-bar of the same dimensions used by him, namely, 4 feet 6 inches long, and 1 inch square, and having cast two bars of Creevelea and two of a mixture of this iron with Scotch, I broke them by hanging weights on their centres, while their ends were held up by supports 4 feet 6 inches apart. The following was the result :—

Creevelea broke		628·0	} Mean, 600·5.
Ditto		578·0	
Mixture, half Scotch, half Creevelea, broke		554·5	} Mean, 539·25.
Ditto	ditto	524·0	

Out of a list of fifty-one varieties of iron given by Mr. Fairbairn, not one gives so high a result; the strongest iron there only carried 567 lbs.

	lbs.
1. Ponkey, No. 3, C. B.	567
2. Devon, No. 3, H. B.	587
3. Low Moor, No. 2, C. B.	462
4. Apedale, No. 2, H. B.	457
5. Gartsherrie, No. 3, H. B.	427

The specimens on the table are Bowling, Apedale, Blair, Creevelea, and Hematite. A close examination of the fractures will show how very near is the resemblance between the best English irons and the Irish specimen. The order of their specific gravities is—

Bowling	7·25
Creevelea	7·09
Hematite	7·09
Apedale	6·89
Blair, Scotch	6·82

The coke made for smelting is of excellent quality, as may readily be inferred from the goodness of the iron; from 35 cwt. to 40 cwt. are required to produce one ton of metal, a proportion not much over that found necessary in Staffordshire. The ore is very rich, yielding about 60 per cent. of iron, a ton being produced by the reduction of from 32 to 35 cwt. of ironstone.

With this Irish iron, of a density inferior only to Bowling and Devon, a tenacity superior to all, a closeness of grain and structure so nearly resembling what we know to be the best iron that England can produce, would it be rash to hope for the very highest results from a vigorous application to this source of wealth in our island? And though the experiments I have been able so far to make are few and perhaps not altogether reliable, I am sure to meet with sympathy here, when I look forward to the time when the tide of imported iron will set the contrary way; when the intelligence and industry of our countrymen in the west will make the names Creevelea and Arigna, stamped upon plate or bar, as sure a guarantee of excellence as the famous brand Low Moor.

Journal of the Royal Dublin Society, July, 1857.

EXAMINATION of some PROPOSITIONS ENUNCIATED by MM. RIVOT and CHATONEY, in their MEMOIR on the MATERIALS EMPLOYED in MARINE CONSTRUCTIONS. By M. VICAT.

(Concluded from p. 668.)

NATURAL POUZZOLANES.

"It can," say MM. Rivot and Chatoney, "be considered useful to make the complete analysis of the pouzzolanes regarded as good in practice only with the view of preparing for the future valuable documents, when science will be able to solve the questions which are now very obscure. It results from this for the present time that the employment of pouzzolanic mortars is the only means of enlightenment as to their true nature."

We subscribe the more willingly to this last conclusion, because it has been our invariable rule for forty years, and because it was by observing it that we arrived in 1846, with regard to the pouzzolane of Italy compared with all the varieties of natural pouzzolanes, at conclusions directly contrary, as will be seen, to those of MM. Rivot and Chatoney.

ARTIFICIAL POUZZOLANES.

Those engineers who have been so kind as to read my memoir of 1846 on artificial pouzzolanes compared with Italian pouzzolanes, will readily be convinced that it was scarcely possible, practically at least, to submit both to more minute experiments, pursued with more constancy in all periods of solidification, through which their combination pass with fat lime, to arrive thereby at recognising the great superiority of the pouzzolanes produced by the slight torrefaction of pure argils over all others, including the pouzzolane of Rome, which has not prevented MM. Rivot and Chatoney, after having rightly rejected the combinations of fat limes and argils which I had never extolled, from expressing themselves as follows:—

"Pure argils employed *after being burned* as pouzzolanes are in conditions analogous to those of crude argils, and consequently can admit of good results only in *very rare cases*, and under the express condition of very special precautions.

"Ferruginous and calcareous argils submitted to long burning cannot be considered as pouzzolanes, since they contain combinations of lime with silica and with alumina, at the same time as argil on which lime might still act by the humid way, &c.

"For these reasons, crude or *other* (that is to say burnt) argils cannot in general act as *good* pouzzolanes, and we do not think it necessary to stop to indicate methods of analysis."

The prescription of artificial pouzzolanes is then as explicit as possible, and for very singular reasons, for it is of little importance in practice that science cannot explain the phenomena to which these pouzzolanes give rise, if, however, mixed with lime they set well under water and endure in it. But we have not come to the end of the contradictions, as we shall see.

"Powdered silex should be regarded as a *good pouzzolane*. . . . We have spoken at the commencement of this second part of the memoir of our experiments with mortars of fat lime with silex, which have set under water in eight days, and have acquired *great hardness* therein."

We beg that this approximation may be well noted. "Artificial pouzzolanes not being generally capable of acting as good pouzzolanes, and powdered silex should be regarded as a good pouzzolane." This double affirmation merited a strict examination, and we have not hesitated to forget our results of 1846 to study again, comparatively, the pouzzolanes of silex and the two types of artificial pouzzolanes recognised in our last work as resisting sea-water under the condition stated.

These two pouzzolanes were composed as follows:—

Pouzzolanes of pure argil	$\left\{ \begin{array}{l} \text{Silica} \quad . \quad . \quad 68\cdot00 \\ \text{Alumina} \quad . \quad . \quad 31\cdot25 \\ \text{Loss} \quad . \quad . \quad 0\cdot75 \end{array} \right\} 100\cdot00$
Pouzzolanes of ochreous argil	$\left\{ \begin{array}{l} \text{Silica} \quad . \quad . \quad 66\cdot86 \\ \text{Alumina} \quad . \quad . \quad 22\cdot75 \\ \text{Peroxide of iron} \quad . \quad 10\ 57 \\ \text{Loss} \quad . \quad . \quad 0\cdot02 \end{array} \right\} 100\cdot00$

Powdered silex, reduced to the finest degree possible by mechanical means, proceeding:—1st from gun flint; 2nd from agate silex with reddish tints.

Each of these substances was very carefully mixed with 15 per cent. of its weight of fat lime weighed while quick, and reduced to paste by the ordinary means, so as to give to the mixture a good consistence; then the mixtures introduced into drinking-glasses and immediately immersed, led to the results given in the subjoined table.

These experiments with artificial pouzzolanes and the agate silex and gun flint, were commenced in Oct. 1856, and concluded in Feb. 1857. In the course of their duration, the temperature of the laboratory varied from 15 to 20° C. (59 to 68° F.)	Quickness of setting measured by the needle in use.	Hardness measured after four months' immersion by the number of turns of a screw required for penetrating 5 millimetres into the mass.
Mixture of fat lime and pouzzolane of white argil in the proportions indicated	8½ days	40 turns
Mixture of fat lime and ochreous argil	2 days	24 turns
Mixture of powdered gun flint and fat lime in the proportions indicated	From the 24th to the 80th day	3 turns
Mixture with the same silex in the state of sand	Did not set	" "
Mixture of powdered agate silex and fat lime	From the 80th to the 35th day	2 turns
Mixture with the same silica in the state of sand	Did not set	" "
Mixture of powdered hyaline quartz and fat lime	Did not set	" "

The completely inert part performed by hyaline quartz was foreseen; it was only as the completion of experiments on materials composed exclusively of silica in the crystalline state, that we included it in this table.

We were so far from expecting these almost negative results given by silex, that in making them our first idea was that the black silex of Havre, qualified as a good pouzzolane by MM. Rivot and Chatoney, must possess exceptional properties. We hastened to request some of the chief engineers of that place, who kindly sent us a sufficient quantity; this black silex, which is more difficult to pulverise than the foregoing, was treated and tested in the same manner; its mixture with 15 per cent. of fat lime, immersed in fresh water on the 15th of March, 1857, could only bear the needle which indicates setting, on the 1st of May following, that is to say after 55 days. The same silex powdered, heated to redness in a crucible, and projected while incandescent into water, acquired a greater degree of energy. Its combination with fat lime set in 26 days.

Disconcerted by this new mishap, we asked ourselves if the experiments at Havre had really been performed with fat lime. We could not doubt this, for the qualification of *fat* which is given to it is made more precise by the word itself printed in italics. We do not suppose that engineers of the position of MM. Rivot and Chatoney could be deceived as to the quality of the lime furnished to them; and we only testify our astonishment that they have been able to affirm *de visu* the great hardness acquired by their experiments, whilst here mortars with silex more active than that of Havre, are still so weak after eight months, that they may be cut with the nail, and that when iron alone is capable of attacking the analogous combinations in which artificial pouzzolanes replace silex. We know not what increase in hardness the action of time might produce on these siliceous mortars, but we maintain our observations as to the period of eight months, observing that among the hydraulic compounds known, there is not one which, after this lapse of time, does not give incomparably better results.

In truth, then, the statement of the memoir must be reversed, and we must affirm that the pure argils, and as an exception some slightly burnt ochreous argils, are excellent pouzzolanes, very capable of furnishing with the concurrence of fat lime combinations resisting the saline action, whilst other kinds of powdered silex are regarded as qualified for pouzzolanes, materials to which no engineer would dare, after trial, to intrust the success of the least important hydraulic construction, even in fresh water.*

Such differences appeared to us too serious to be left on our own responsibility, we have therefore called some well known men of our city to verify them; MM. Gueymard and Lory, the former chief

* We are not contented with limiting the degree of fineness of ground silex, to that which is designated under the name of porphyrisation, we have gone further, by the successive levigations and decantations of the supernatant portions.

engineer of Mines *en retraite*, formerly dean of the Faculty of Sciences; the latter, a distinguished geologist, professor to the same faculty, were kind enough to undertake this task, and have been enabled to convince themselves of the accuracy of our experiments and their results.

We could not terminate these observations without remarking on the improbability of another class of affirmations presented by MM. Rivot and Chatoney, in support of the new processes of preparing mortars, which they deduce from their theories; these authors *affirm with the greatest assurance*, although Vetruvius has not said a single word of them, that the Romans must have known these processes, that is to say the previous digestions,* and that to their employment is due the perfect preservation in the sea of their pouzzolane mortars, "whilst from the absence of the same precautions, analogous mortars recently employed in the Mediterranean have failed."

How could MM. Rivot and Chatoney be ignorant that all the hydraulic works executed by the Romans in their marine constructions, such as moles, &c., were kept up under the emperors only by means of frequent repairs, and that in the decline, owing to want of this repair, their ruin progressed to such an extent, that now we can only find a few vestiges disseminated under the water, either in the gulf of Naples or throughout the length of the coast which extends from Gaeta to Civita Vecchia? and as regards the accusation of failure brought against the modern works executed on the Mediterranean, we know that it has strangely surprised, to say the least, M. Noel, the inspector general of the maritime service, who intends in a short time to correct this error; the defence will be in too good hands for us to interfere.

We may remark, in conclusion, that it is on a passage in which Belidor attributes to the Romans, "an unwillingness to employ lime for their edifices except after it had been slaked two or three years," that MM. Rivot and Chatoney rely for concluding, probably by induction, that the ancients knew and practised the system of digestions.† It is truly vexatious to endeavor on such proofs to resuscitate this old path of the secret of the Romans, to which we did justice in 1819 and 1828, and at which Arago struck such a rude blow in

* It is impossible to find anything in Vetruvius, who was so accurate in noting essential things, which can warrant this singular affirmation. It may be ascertained by reading book II. page 157; VIII. p. 275; and V. p. 511; what he says of the fabrication of mortar of the betomage and on the construction of jetties on the sea.

† We read in note 49, page 217, of the *Commentaries on Vetruvius* (translation of Manfrass edit. Panckonke) "that of Vetruvius, Pliny and Palladius, the author of the Compendium, said nothing of the time which should elapse between the moment of the extinction of the lime and that of its use; but for masonic works, lime and bricks, the author of the Compendium recommends its employment immediately after extinction, but for stuccoes, it should be slaked a long time before employment. It is this also which Vetruvius recommends, giving the reason for this case only. Now a long time can signify here only from 20 to 30 days at most, and not two or three years as Belidor says."

1845.* But since this endeavor has been made, we shall now inter it irrevocably with the assistance of Frontin in his remarkable work on the aqueducts of the City of Rome. We borrow the following from him.†

"It was in the year 441 of the foundation of Rome, that the first aqueduct brought to it the water of Appia; until then the Romans were contented with the waters of the Tiber, of wells or of springs."

"A second aqueduct brought to Rome, the water of the Anio, in the year 481, and in 608, this aqueduct and the foregoing were already threatened with ruin from age. The former was 167 and the latter 157 years old."

"In this same year 608, the water of Marcia was brought to Rome, and in the year 719, or 111 years after, its aqueduct and the two foregoing, had fallen into ruin (*pene delapsos*). Agrippa restored them."

Thus, of these three aqueducts, none lasted for a century and a half in good condition.

The importance which the inhabitants of Rome attached to the arrival of these waters, must now be known, and all the efforts of the consuls and emperors to provide them more abundantly, in at a later period tripling their volume, in order to comprehend that all the practical science of the age must have been brought to bear on the construction of the aqueducts, and we have just seen how it ended.

This practical science, Frontin completely reveals, "by insisting on the execution of the regulations in force, which prescribed without any other indication to build in masonry only from the Kalends of April to November, ceasing during the heats of summer." With us, there is no interruption, but in good practice, masonry is watered from time to time during the great heats. This is all the difference; and if after these quotations MM. Rivot and Chatoney continue to pretend that the Romans employed *digestions*, we shall be forced to conclude that this process was bad, for the old ramparts and the old walls of our castles of the middle ages have lasted longer than the Roman aqueducts, and when it has been necessary to destroy them in order to make room for other buildings, they have had to be blasted, and yet the masons of this period were unacquainted with the system of digestions.

It must be understood, with a little reflection that a process so common as that of the preparation of mortars could the less easily be forgotten, as building has never ceased in Italy, even in times of barbarism and invasion; for, indeed, it must have been necessary to build again after devastations. The practical tradition of the processes could not then be lost; it must have been transmitted from mason to mason, unless we suppose workmen of this class to have been entirely exterminated by the barbarians, which is beyond all probability.

Comptes Rendus, No. 21, May, 85, 1857.

* See his report to the Chamber of Deputies, 1845. This report was inserted in the *Annuaire des Bureau des Longitudes* of 1846.

† Des Aqueducs de la Ville de Rome; by Frontin (Sextus Julius prætor urbanus) translations of Builly, edit Panckonke, pp. 371, 378, 379, 467, and 469.

AGRICULTURAL CHEMISTRY.

On a COLORED ORGANIC COMPOUND of ALUMINA and PEROXIDE of IRON found in the soil at KUYLOCH in 1824.—SOME REFLECTIONS on the BROWN SUBSTANCE called ULMINE: the difference between MINERAL ANALYSIS and PROXIMATE ORGANIC ANALYSIS.—SUMMARY of ANALYTICAL EXPERIMENTS made on a BROWN LIQUID proceeding from the GREASE of SHEEP.—Considerations on SULPHUR with respect to the COMPOSITION of LIVING BODIES. By M. CHEVREUL.

(Concluded from page 691.)

I REMOVED the following bodies from the grease of sheep :—

1. Water.
2. Ammonia.
3. Carbonic acid.
4. Aroma of sheep pens.
5. Aroma *a*.
6. Phocenic acid.
7. Volatile acid. *b*.
8. Stearine.
9. Elaierine.
10. Crystallisable fatty proximate principle.
11. Stearate of potassa.
12. Elaierate of potassa.
13. Phocenate of potassa.
14. Volatile acid *b* combined with potassa.
15. Colorless crystallisable acid.
16. Uncrystallisable orange yellow acid.
17. Brown nitrosulphuretted acid.
18. Nitrosulphuretted acid matter insoluble in water.
19. Crystallisable carbonate of potassa.
20. Sulphate of potassa.
21. Silicate of potassa.
22. Chloride of potassium.
23. Oxalate of lime.
24. Phosphate of lime.
25. Ammoniac-magnesian phosphate.
26. Carbonate of lime.
27. Oxide of iron.
28. Oxide of manganese.
29. Oxide of copper.

If the stearine, elaierine, stearic acid, elaieric acid, volatile acid *b*, colorless crystallisable acid, uncrystallisable orange yellow acid, and brown nitrosulphuretted acid, as I obtained them, were proximate principles in a state of purity, if no soda existed in the grease, I should consider the proximate analysis of this matter as concluded, because I should then have had only to study the properties and elementary composition of such of these principles as were new. Unfortunately my work was not so nearly at an end; there might,

possibly, be other bodies in the grease besides those which I have named, and it was important to prove the absence or presence of soda.

However this may be, some important facts were proved at once:—

1st, The analogy of many of the proximate principles excreted by the skin and hair, with the proximate principles excreted by the kidneys:

2nd, The *oxalate of lime* found in the alkaline grease of sheep, and the acid grease of the alpaca.

3rd, The presence in the grease of sheep of *silicate of potassa*: for I have ascertained the presence of this salt in the soluble part of the grease of an unshorn sheep, without using glass vessels in the analysis.

4th, The excretion by the skin and wool of sheep of an appreciable quantity of *sulphur*. This is an important fact in physiology. I have ascertained in the matter which exudes from the skin of man, in the state of perspiration, a sulphuretted matter, which if not identical with, is at any rate analogous to that of this grease.

The cause of the length of the labors of which this grease has been the subject, will be seen readily when I have explained the two senses which I attach, and which should always be attached, to the words *complexness of chemical composition*, if we wish to distinguish the precise from the vague, the defined from the undefined.

First Sense.—COMPLEXNESS OF DEFINED COMPOSITION.

This expression should only be applied to *chemical species*, that is to say, to matters which are definite from their elementary composition and their properties.

It is used, in a general sense, to designate every species found to consist of several elements:

In a particular sense, to designate the complex species which we reduce in several proximate principles, such as a salt which we reduce into an acid and a base.

In this case, the expression *complex composition*, may be contrasted with that of *simple composition* which is applied to the composition of proximate principles.

The expression *complex composition* may be used to compounds which, in certain circumstances, under the influence of certain reagents, are resolved into compounds which cannot be considered as the proximate principles of the matter whence these compounds proceed.

In this sense we frequently use the word *separates*.

Second Sense.—COMPLEXNESS OF UNDEFINED COMPOSITION,

Is applied to a matter resulting from the union or mixture in *undefined proportions* of different bodies.

Such a matter, in my opinion, should never receive a *special name*; this is a rule which I have always observed, never giving any *specific name*, except to bodies of an exactly definite composition.

This is the explanation of the slowness of my work on grease, which, when complete, will fill a volume. There the example will be found joined to the precept, and I have throughout distinguished conjecture and even induction from *facts* proved by experiment.

It is in conformity with these views that I shall examine the relation of M. Paul Thénard's work on manure, and compare it with my researches.

1st, I began by recalling the existence of *natural lake* by insisting, from 1824, on the complex nature of the organic matter which was combined with alumina and peroxide of iron. Indeed, I have thenceforward mentioned a yellow coloring principle and a nitrogenous matter, very probably sulphuretted. I can now affirm, from my investigations on grease, that it was really so.

2nd, I have found in grease, a compound insoluble in water, containing a brown nitrosulphuretted acid matter, at the same time as a *brown organic acid matter* soluble in water, constituting with potassa a compound which is likewise soluble in water.

3rd, I have not given to the *brown organic acid matter* soluble in water a specific name, because I have proved that it is formed of several proximate principles united in indefinite proportions; still I have every reason to believe that the colorless crystallisable acid is a *pure proximate principle*.

4th, The existence of the *lake* of the soil of Kuyloch, the analogy between the *brown liquid* of the grease and the juice of manure, the existence of a nitrosulphuretted matter in this juice, and in a portion of the grease which water does not dissolve, have led me to think that sulphur might well be one of the elements of the matter extracted from the juice of manure by M. Paul Thénard; moreover, this same matter being formed, very probably, of several proximate principles, united in undefined proportions, we ought to wait for further experiments before giving it a specific name.

I cannot conclude this Note without recalling a fact connected with the question which has been much discussed of late years, the introduction of phosphate of lime into plants.

On the 26th August 1830, I read to the Academy a work on the leaves of the woad, and the extractive principle they contain, in which I mentioned the fact which I now wish to recall.

After having separated; 1st, A nitrogenous matter, by the coagulation of the juice of the woad when filtered and exposed to a temperature of from 44° to 54° C. (111° to 129° F.); 2nd Citrate of lime and sulphate of lime, by the concentration of the filtered and coagulated juice; 3rd, A matter insoluble in alcohol, by means of this solvent; I obtained, on treating with water, this matter which alcohol did not dissolve, a *brown acid liquid* from which ammonia precipitated *ammoniaco-magnesian phosphate*, and it is remarkable that there remained in solution with an organic nitrogenous matter and a yellow coloring principle, *phosphoric acid, lime, and oxide of iron*.

Whence I concluded: 1st, That if the leaves of the woad were buried as a green crop for a manure, the principles of phosphate of lime might introduce themselves into the plant, even when these leaves had undergone the ammoniacal fermentation, or when the juice of the leaves, without fermentation, had been neutralised by ammonia from any source whatever; 2nd, The probability that certain manures might be in a state analogous to that of the brown liquid of the juice of the woad, neutralised by ammonia, and that thence the proximate principles of the phosphate of lime penetrate into the plants in solution, in neutral and even alkaline liquids.

One thought which has supported me through the very lengthened labors of my examination of the grease of sheep, is the conviction of the service which proximate organic analysis will sooner or later render to agriculture when it is applied, as I understand it, to the examination of manures: it will certainly lead to consequences which are not, in general, even suspected.

In the examination of manures, the attention of the chemist should be directed at once to the separation of the proximate principles, to the action which the oxygen of the atmosphere may exert upon some of them, and on the influence of the earthy matters of the soil not only in combining with these principles, but, independently of the affinity, to augment their stability or accelerate their decomposition by the action which is termed of *presence*.

Comptes Rendus.

On the INFLUENCE which the ASSIMILABLE NITROGEN of MANURES exerts on the PRODUCTION of VEGETABLE MATTER.

(Concluded from page 678.)

By M. BOUSSINGAULT.

I SHALL now give a summary of the facts proved in three experiments:—

	Weight of the dried plant, the seed being 1.	Vegetable Matter elaborated.	Carbonic acid decomposed by the Plant per diem.	Acquired by the Plants during 86 days.	
				Carbon.	Nitrogen.
	gr.	gr.	c.c.	gr.	gr.
Exp. A, the soil having received nothing . . . }	8 6	0·285	2·45	0·114	0·0028
Exp. B, the soil having received phosphate, ash, nitrate of potassa . . . }	198·8	21·111	182·00	8·444	0·1666
Exp. C, phosphate, ash, bicarbonate of potassa . }	4·6	0·391	3·42	0·156	0·0027

The influence of the nitrogenous manure in the developement of the vegetable organism is here shown in the clearest manner.

The sunflowers whose soil had received saltpetre and phosphate grew as large as they would have done if in good garden earth; they assimilated 8.44 of carbon. The seeds which contained 0.019 of albumen produced, from the effect of saltpetre, plants containing more than 1 gramme of it.

On a soil deprived of all assimilable nitrogenous matters, with or without phosphate of lime and alkaline salts, the sunflowers only attained a height of 1 centimetre. On acting on the carbonic acid diffused in air or dissolved in water, they did not remove 0.2 of carbon, and the nitrogenous principles of the atmosphere which intervened in these circumstances did not yield them 3 milligrammes of nitrogen. These last results prove that to give active aid to vegetable production, the basic phosphate of lime and alkaline salts must be associated with a substance which is capable of furnishing assimilable nitrogen. Farm-yard manure, the best of all manures, has precisely this association.

In the experiments in which saltpetre was not used, the 2 or 3 milligrammes of nitrogen acquired by the plants during three months' vegetation proceeded very probably from the ammoniacal vapors and nitrous compounds which exist or are formed in the atmosphere. I have succeeded in discovering its presence in the air by the following means.

To ascertain the Presence of Nitrates.—Six U tubes were placed in connection with an aspirator. The first two tubes which the air passes through first, were filled with small pieces of brick, impregnated with a solution of carbonate of potassa; then came two tubes filled with alkaline pumice stone; then two more tubes containing chalk moistened with a solution of carbonate of potassa. The fragments were from a new brick; but having been deposited for a long time in a store-house; they had been washed with distilled water before calcining them, so as to remove the nitrates which they might contain, and which calcination, without charcoal, does not always completely destroy, or rather transforms into nitrites or other very persistent nitrous compounds. The carbonate of potassa was prepared by incinerating cream of tartar, and ascertaining that it did not contain the least trace of nitrate. The apparatus was sheltered from the rain, in a box in which was an aperture for the air, at 8 decimetres above the soil, near a vine.

The aspirator worked almost incessantly, night and day, from the 7th July to the 7th October, 1856. The matters contained in the tubes were kept always moist. When the experiment was concluded, we found a very appreciable quantity of nitrate in the first tube; there was also a trace of this salt in the second tube, and none at all in the following tubes; at least the reaction could not be ascertained, although, on the one hand, tincture of indigo, and on the other, gold leaf, are capable of proving distinctly a twentieth of a milligramme of nitric acid.

The air, when aspired, passed directly into the first tube in which were the fragments of brick moistened with a solution of carbonate of potassa. I did not judge it necessary to pass it through sulphuric pumice to retain the ammoniacal vapor: what I wanted to ascertain was merely the presence or absence of nitrate in an earthy matter of a porous nature, soaked in dissolved carbonate of potassa and exposed to a current of air. Whatever may have been the cause there was undoubtedly an appearance of nitrate: I say appearance, and not production, because the experiment, as arranged, showed nothing else. If, indeed, it is possible that the ammonia of the air which had not been eliminated, had been nitrified on the contact of potassa mixed with a porous body by the organised oxygen, neither does it seem improbable that the nitrates might have been brought by the dust which the atmosphere always carries with it. Saltpetre is everywhere on the face of the globe; the smallest particles of vegetable earth which the wind carries evidently contain it, and the air drawn into the apparatus might deposit it on the moist brick in the first tubes. I should observe here that even when this air had been taken first over sulphuric pumice, so as to fix the ammonia, this method did not prevent the nitrates from appearing; for, as far as they are concerned, the action of the sulphuric acid would be limited to retaining their bases, and the nitric acid, being set free or transformed into nitrous compounds, would still be drawn by the current and retained by the potassa in the first tubes.

However this may be, and considering solely the fact of the appearance of nitre where none had been before the passage of the air, we must acknowledge that this experiment, performed above the level of the earth, near a vine, and on the confines of an immense forest, leads to a result in exact conformity to that obtained long before by M. de Luca in analogous circumstances, as to the abundance of vegetation, as this careful observer found that there is a formation of nitric acid when we pass into a solution of potassa, a current of air deprived of ammonia, free from dust, and taken in a green-house, in which many plants of various descriptions were growing.

Estimation of the Nitrogen brought by the Atmosphere.—We placed near the plants which were being experimented on, a cylindrical glass vessel three centimetres deep, presenting an open surface equal to that of the flower pots. Into this were put 500 grammes of washed and calcined sand with which had been mixed 10 grammes of oxalic acid considered pure, although containing in reality 0.0011 gr. of nitrogen which was taken account of. The mixture, kept moist, was left exposed to the air. When it rained, and at night to avoid dew, the vessel was covered with a bell glass. After seven weeks the sand contained 0.0013 gr. of nitrogen, of which a portion certainly constituted ammonia. That is, however, a simple indication; for everything leads us to presume that the quantity of nitrogenous principles that a damp soil receives from the atmosphere depends at once on the extent of the exposed surface, the

duration of the exposure, and the locality. I say the locality, and that is a very important circumstance; for the air is not always equally pure. The impurity of rain water shows, better even perhaps than the most delicate analysis, the degree of impurity of the atmosphere. Thus it is that the meteoric waters collected at Paris and at Lyons contain much more ammonia, nitrates, and organic matters, than the rain, snow, fog, and dew, which fall at a great distance from the great centres of population; this fact has been proved by the observations of M. Barral at Paris and M. Bineau at Lyons, as compared with my own at Liefrauenberg, and those of Messrs. Lawes and Gilbert at Rothamstead.

Influence of Assimilable Nitrogen on the Development of the Vegetable Organism.

The previous experiments have established that phosphate of lime and the alkaline salts, added to the soil without the concurrence of a nitrogenous manure, do not contribute sensibly to the development of the organism. The matter elaborated in this condition by the vegetable weighs hardly any more than that which is produced when the earth, rendered sterile by the fire, contains no saline substance, when the vegetation is performed only with the resources found in the seed, and a *limited plant* is the result. When, on the contrary, the phosphate and saltpetre are associated, they act with the energy of manure. It is, I believe, allowable to conclude from these facts that the growth of a plant is subordinate to the previous absorption of an assimilable nitrogenous substance, whose effects it is perhaps not impossible to measure. At any rate I have tried.

With this view I introduced into the calcined sand supplied with phosphate of lime and salts of potassa, various proportions of nitrate of soda, or if the term be preferred, various quantities of assimilable nitrogen.

The soil, being calcined and manured with the phosphate, was divided into four flower pots, free from all organic matter. Into each of these vessels were planted two seeds of helianthus, weighing 0.110 gr. The vegetation lasted fifty days. The water for irrigation, free from ammonia, contained about a fourth of its volume of carbonic acid gas. The plants were grown in the open air, but sheltered from rain and dew.

No. 1 contained no nitrate of soda.

									gr.
No. 2	.	.	.	.	.	.	.	.	0.02
No. 3	.	.	.	.	.	.	.	.	0.04
No. 4	.	.	.	.	.	.	.	.	0.16

During vegetation the plants were vigorous, with beautiful green leaves. Their dimensions at the close of the experiment were as follows:—

	Height, Length, Width of the Largest Leaf.			Weight of of the Dried Plants.
	Cent.	Cent.	Cent.	Gr.
No. 1, without nitrate	9	3.7	1.5	0.507
No. 2, 0.02 gr. of nitrate	11.2	5.4	2.0	0.830
No. 3, 0.04 „ „	11.5	6.8	2.8	1.240
No. 4, 0.16 „ „	21.5	9.1	3.7	3.390

On subtracting the weight of the seeds from the weight of the dried plants we find that the organic matter elaborated during the vegetation was:—

No. 1	0.397
No. 2	0.720
No. 3	1.130
No. 4	3.280

The influence of the assimilable nitrogen is manifest, and it is not without surprise that, in the result of experiment No. 2, we find that 8 milligrammes only of this nitrogen introduced into the soil is sufficient to double the organic matter of the sunflowers. Thus, the proportion of the weight of the seed to the dried plant, which was ::1:4.6 when no nitrate had been given, became

::1: 7.6 in No. 2
 ::1: 11.3 in No. 2
 ::1: 30.8 in No. 4

Analysis has estimated, in sunflower No. 1—

Nitrogen	gr. 0.0053
In the seeds there was, nitrogen	0.0038

In fifty days' vegetation, nitrogen acquired 0.0020

In the plants grown in soils to which nitre had been added, the estimations of the nitrogen led to the following results:—

	Nitrogen in the nitrate and seed.		Nitrogen in the plants.
	gr.	gr.	gr.
No. 2, nitre added	0.02	0.0066	0.0062
No. 3, „ „	0.04	0.0100	0.0097
No. 4, „ „	0.16	0.0297	0.0251

Indications of nitric acid were found in the soil of experiments Nos. 2 and 3. In the soil of No. 4 there was rather less than 0.08 gr. of nitrate of soda, and in each of the plants grown with this salt, the nitrogen acquired by the organism did not exceed that which the nitrate had introduced.

The striking part of this second series of investigations is that of showing how a nitrogenous substance, introduced into the soil, contributes to the growth of the vegetable, but also how much the organic matter elaborated by the plant augments by the addition of the smallest quantity of assimilable nitrogen.

We can convince ourselves, by consulting the numbers expressing the quantity of carbon fixed by the sunflowers, that the decomposition of the carbonic acid gas has been greater when the plant has had more nitrate of soda at its disposal, or if the expression be preferred, more nitrogenous manure :—

	Carbon contained in the organic matter. gr.	Carbonic acid decomp. per diem. c.c.
Exp. 1	0·159	5·3
Exp. 2	0·288	10·6
Exp. 3	0·452	17·2
Exp. 4	1·312	40·5

These researches show—1st, That phosphate of lime, alkaline and earthy salts indispensable to the constitution of plants, nevertheless have an action on vegetable only when combined with matters capable of giving assimilable nitrogen.

2nd, That the assimilable nitrogenous matters contained by the atmosphere are in too small proportion to determine, in the absence of a nitrogenous manure, an abundant and rapid vegetable production.

3rd, That saltpetre associated with phosphate of lime and silicate of potassa acts as a complete manure, because the sunflowers grown under the influence of this mixture were, as regards vigor and size, comparable to those grown in a well manured garden bed.

I must add in conclusion, that it is very remarkable to see a plant pass through all the phases of vegetable life, germinate and come to maturity, in one word attain its normal developement when its roots grow in calcined sand, containing, in the place of organic debris in a state of putrefaction, salts of great purity, perfectly definite compounds, such as nitrate of potassa, basic phosphate of lime, and alkaline silicates, and to ascertain that by means of these auxiliaries, all belonging to the mineral kingdom, this plant progressively augments the weight of its organism, by fixing the carbon of carbonic acid, the elements of water, and by elaborating, with the radical of nitric acid, albumen, caseine, &c., namely the nitrogenous principles of milk, blood, and muscular flesh. Moreover, there is probably more analogy than has been supposed between these salts which I have just mentioned and stable manure. In fact, stable manure, in which Braconnot found no less than forty substances, changes its constitution in a singular manner when it has remained for some time in properly prepared land. The fermentation which continues in the soft parts; the slow combustion producing the humus, the soil, these advanced states of the decomposition of organised bodies and animal dejections; the action which the air, water, and soil exert on all these matters, cause farm-yard manure to bring to the plants alkaline and earthy salts, phosphates, and, as retainers of assimilable nitrogen, the nitrates and ammonia.

Comptes Rendus.

EMPLOYMENT of SOLUBLE GLASS in the CULTURE of CEREALS.

By M. KNOPP.

M. KNOPP has obtained excellent effects by previously steeping wheat and rye intended for sowing, in a solution of soluble glass. This fact comes in support of the experiments tried by M. Paul Thenard (see THE CHEMIST, August, 1857, p. 684), with the view of using for the same purpose, not indeed soluble glass, which would be too costly, and which would have to be prepared purposely, but the silicates which are produced so abundantly in the high furnaces, and which encumber the manufactories, where they are known by the name of dross.

The experiments made by M. Knopp were concluded in October, 1856; the author continues his experiments this year.

Dingler's Polytechnisches Journal.

PHYSIOLOGICAL & PATHOLOGICAL CHEMISTRY.

EXPERIMENTS proving that SUGAR is not formed in the LIVER of ANIMALS after their DEATH.

By M. L. FIGUIER.

M. CL. BERNARD asserts that the liver has the property of secreting sugar after the death of the animal. To establish this, he takes the liver of a dog, immediately after killing it, fixes the trunk of the vena porta on a gutta percha tube, and adjusts the other extremity of this tube to a tap of water; he then subjects it for forty minutes to the action of the current of water. He asserts that the liver, thus freed from the glucose which it contained, and then left alone, becomes charged with a considerable quantity of sugar, by virtue of what may be termed a *posthumous secretion*.

This experiment is not decisive, inasmuch as simple washing by a current of water traversing the liver for forty minutes is a very insufficient means of freeing the liver from the glucose which it contains. I have ascertained by many experiments described in my memoirs, that very peculiar precautions are necessary to remove, by the action of water, all the sugar imprisoned in the hepatic cells.

But when this washing is properly and strictly performed, we find that the tissue of the liver, when it has been thoroughly freed from all its soluble products, has not, in any way, the property of forming sugar after death. The following is the process which I followed, to render this fact quite evident.

I took a sheep's liver, shortly after the death of the animal, and I minced it carefully. The tissue thus divided was passed through a fine hair sieve. What passed through the sieve was a true pulp, in which the hepatic tissue was in a great state of division. I washed this pulp in cold water, by decantation, a great many times. After this treatment there remained an almost colorless mass, containing no glucose. It was then left to itself for twenty-four hours, to ascertain whether any sugar would be formed. Now, the water in which this

fibrous mass was boiled, after this time, did not contain the least trace of glucose. Thus, the substance of the liver, when well freed from sugar by a rigorous washing, does not become charged with a fresh quantity of sugar after a certain time.

But to decide positively whether the liver secretes sugar after the death of the animal, it was necessary to determine by chemical analysis the quantity of sugar existing in a washed liver, and, after twenty-four hours, to repeat this estimation, so as to ascertain whether, during this interval, the quantity of sugar had augmented. This comparative determination was made in the following manner :

A sheep's liver, taken shortly after the death of the animal, was subjected for *an hour and a half* to the action of a current of water entering by the vena porta and issuing from the inferior vena cava.

After this washing, the liver, which weighed 900 grammes, was divided into two portions, of the same weight each, about 450 grammes.

One of these halves was carefully minced and subjected to decoction in boiling water; the liquid produced by this decoction was slightly concentrated, precipitated with subacetate of lead, and the excess of the lead salt removed by carbonate of soda. The filtered liquor was heated by the cupreo-potassic re-agent which, under the influence of ebullition, gave a voluminous reddish yellow precipitate of sub-oxide of hydrated copper.

This precipitate of sub-oxide of copper, being collected on a filter, washed and calcined for a long time in a platinum crucible, in contact with the air, to transform it into binoxide of copper, left a residue of this oxide weighing 0.123 gr.

After twenty-four hours, the second half of this liver, which had been left alone, was subjected to the same operation as the first. It was treated with water, precipitated with sub-acetate of lead, then with carbonate of soda, then put in contact, when boiling, with the cupreo-potassic reagent. The reddish yellow precipitate of suboxide of copper, collected on a filter, and calcined, in contact with the air, to transform it into black binoxide, left a residue of this oxide weighing 0.102 gr.

The result of this experiment is, that notwithstanding washing with a current of water for *an hour and a half*, the liver contained a considerable quantity of glucose; and, moreover, that *this quantity had not increased in this organ when left alone for twenty-four hours*.

I attained the same result in two other experiments in which I determined the weight of the soluble matters existing in each half of the same liver, before and after the lapse of twenty-four hours. This quantity, instead of augmenting, had undergone a slight diminution.

I have said above that the cause of the error which supposes that sugar is formed in the liver after death, is the insufficiency of the means employed to wash the organ. A current of water taken for forty minutes merely through the ramifications of the vena porta, is not sufficient to free the liver from all its soluble matters, especially when operating on a liver which is much charged with sugar, as that of one of the carnivora must always be. I thought that by operating

on a liver less charged with sugar, and prolonging the time of the washing, it might be completely freed from all saccharine matter; and that we could then, without altering or dividing the organ—in a word, without touching its anatomical integrity, ascertain whether sugar formed again spontaneously in its well washed tissue. I have ascertained that the horse is one of the animals whose liver contains the least sugar. I have profited by this condition to make a final experiment, which has confirmed these.

At the Veterinary School at Alfort I obtained the liver of a horse which had just been killed, and I submitted it for two hours and a half to a powerful current of water. Before this operation, I had ascertained that this liver contained sugar. After this washing, the liver was quite free from glucose; for a piece, weighing 250 grammes, yielded not a trace to boiling water.

Having been left untouched for twenty-four hours, this liver gave no sign of containing a trace of sugar, when tested by reagents.

This experiment was repeated, in exactly the same manner, on the livers of two other horses, and gave precisely similar results:—The existence of sugar in the liver, examined immediately after the death of the animal; an absence of sugar, after being washed for two hours and a half in a current of water; and a complete absence of sugar, twenty hours after washing.

In a fresh memoir on the glucogenic power of the liver, I shall very shortly have the honor of communicating to the Academy the result of some experiments on the glucogenic matter which exists in the liver, according to M. Cl. Bernard; and which, according to my views, is nothing but the product of the decomposition, by potassa, of *albuminose*—an organic product, the existence of which, in the liver, I have shown; and which was studied and described in a former memoir. I shall prove that this *glucogenic matter* is formed with most of the albuminoid substances, and may be obtained by operating with the albumen of eggs precipitated by alcohol, redissolved in water, and treated with boiling caustic potassa. I shall likewise endeavor to show the chemical difference existing between the sugar contained in the liver and that found in the vena porta, and in the general circulation of animals fed on an exclusively meat diet.

Comptes Rendus, No. 28, June 8th, 1857.

EXPERIMENTS upon DIGESTION.

By FRANCIS G. SMITH, M.D., Professor of Institutes of Medicine in the Medical Department of Pennsylvania College.

In the *Medical Examiner* for July and September, 1856, the writer published some experiments performed upon Alexis St. Martin, with the view of determining the nature of the acid of the gastric juice, and the influence of the latter fluid upon various alimentary substances. The conclusions arrived at were, that the acid reaction is due to the presence of *lactic acid*; that nitrogenised food is converted into albu-

minose; that fatty articles undergo no change beyond minute division; that *amylaceous*, or *starchy materials* (in man) are converted into glucose or *grape sugar*.

To the last of these conclusions the objection has been urged, that the glucose manifested in the products of digestion "*existed beforehand in the bread used as food.*"*

To those who did not witness the experiments, such an objection may appear well grounded, particularly as it is well known that glucose is formed in the process of panification; but with those who were present, and who saw the evidences of a much larger quantity of glucose in the fluid of digestion than they ever met with in the examination of bread alone, such objections have less weight.

Granting, however, the justice of the criticism, and feeling great respect for the source from whence it came, but at the same time contending for truth alone, the writer has, during the past month, performed a corroborative experiment, with the assistance of his distinguished friend, Dr. E. Brown-Sequard, who kindly *lent himself* for the purpose, in place of St. Martin, unfortunately no longer within reach.

Dr. Brown-Sequard has, as Montegre had, the faculty of vomiting at will, and thus exhibiting the condition of alimentary articles at any stage of digestion. With the view of determining the action of gastric juice upon amylaceous food, he lent his valuable aid and experimental tact in the following researches.

After a night's fasting, the stomach was thoroughly cleansed by copious draughts of water, and a portion of plain arrowroot, carefully washed before boiling, and subsequently tested for glucose (of which not a trace was visible), was administered. After about thirty minutes had elapsed, the contents of the stomach were ejected by a simple spasmodic contraction, and without nausea. The fluid was distinctly acid to litmus, and gave a copious precipitate of red suboxide of copper, with Trommer's test.

The process was continued with subsequent ejections for nearly an hour, with the same result on each repetition, with the exception that the quantity of glucose detected was, if anything, increased. The fluid was not tested for starch or dextrine.

A portion of the same arrowroot was treated with saliva, and allowed to stand about the same length of time that the previous portions had remained in the stomach. Trommer's test revealed glucose, but by no means in so large a quantity as in the previous experiments.

It may be urged by some that these results are not reliable, inasmuch as the process of digestion was not healthily performed. To this it is answered, that there was no evidence of disorder in the function other than the spasmodic contraction alluded to, and that distinguished physiologists have placed implicit confidence in the ex-

* Prof. J. C. Dalton, in Amer. Jour. of Med. Sci., October, 1856.

periments of Montegre* and others, of a similar character. Taken in connection with the experiments upon St. Martin, therefore, this one seems to bear out the conclusion then arrived at, that "starchy materials are digested in the human stomach; that human gastric juice does not prevent the conversion of starch into grape sugar; and that this conversion may take place in the stomach independently of the action of saliva." In regard to the last point, it may be observed, that the fluid character of the food obviated the necessity for insalivation, as Bernard's experiments have shown, and that the glucogenic change, therefore, could have been but little, if at all, influenced by saliva, the effects of which are declared by Bernard to be nullified by the acid reaction of the gastric juice.

Medico-Chirurgical Review, July, 1857.

On the TOXICOLOGICAL EFFECTS of CARBONIC ACID.

By M. WAUNER.

I HAVE the honor of submitting to the Academy the results of the following three experiments, which I have made on guinea-pigs.

In the first experiment, I caused one of these little animals to respire carbonic acid, and at the expiration of a minute and a half, it no longer gave any sign of life; in the second, one of them was stifled by means of a mask of caoutchouc which, covering the entire head, prevent the ingress and egress of air, by hermetically sealing the mouth and nose; the animal died in three minutes; finally, in the third experiment, the whole body of the animal, except the head and neck, was plunged into a vessel containing caoutchouc dissolved in oil; but in this case death did not occur until eight hours and forty-eight minutes after the introduction of the animal into this solution.

Immediately on the death of these animals, a very small portion of the pulmonary tissue, of the mucous membrane and of the subcutaneous cellular tissue, were successively placed under the lens of a microscope; the capillary vessels of these several tissues were, in their course, of a black slate color and distended, as if they had been injected with a liquid of that color.

Some of the blood of these asphyxiated animals was likewise examined with the microscope; the globules, instead of being of a reddish color, presented also the same color as the capillary vessels.

Was not the death of these three animals due to the same cause, that is to say, to the action of the carbonic acid which, brought into contact with the blood, as Lehmann has already remarked, crystallises the hæmatosine? Thus, for the first and second experiment, whether the carbonic acid be introduced by inspiration, or whether it be that which, continually arriving in the lungs from all the organs of

* Adelon and Chaussier, Art. "Digestion," in *Dict. des Sciences Méd.*, t. ix. pp. 422-3; Stevens, *De Alimentorum Concoctione*; Spallanzani, *Exper. sur la Digestion*.

the body, cannot be expelled; this acid, being then introduced by endosmose into the capillary vessels of the pulmonary cellules, combines with the hæmatosine of the blood contained in these small vessels, forms in them small crystals, and thus necessarily impedes the motion of the blood. The same chemical phenomena occurred likewise in the guinea-pig, subjected to the third experiment; only the action of the carbonic acid was slower on the capillaries of the groups of all the body.

From these three experiments, we may conclude :—

1st. That the beating of the heart ceases as soon as the motion of the blood is stopped, either in the group of the capillaries of the lungs, or in the group of the capillaries of the whole body.

2. That the action, hitherto designated under the name of asphyxia, could only be the more or less complete impediment of the progress of the blood by an agent which, combining with one of the principles constituting this fluid, would render it more or less unfit for undergoing the motion, being also arrested in the capillaries, so that in asphyxia either by submersion or by strangulation, &c., &c., it would, with the want of air, which would be the cause of death, be indeed the non-expulsion of the carbonic acid from the lungs.

Comptes Rendus, No. 24, 1857.

PHYSICS.

On the CONSERVATION of FORCE. By Mr. FARADAY.

(Concluded from page 702.)

If the definition *denies* the occurrence of such contingent results, it seems to me to be unphilosophical; if it simply *ignores* them, I think it is imperfect and insufficient; if it *admits* these things, or any part of them, then it prepares the natural philosopher to look for effects and conditions as yet unknown, and is open to any degree of development of the consequences and relations of power: by denying, it opposes a dogmatic barrier to improvement; by ignoring it becomes in many respects an inert thing, often much in the way; by admitting, it rises to the dignity of a stimulus to investigation, a pilot to human science.

The principle of the conservation of force would lead us to assume, that when A and B attract each other less because of increasing distance, then some other exertion of power, either within or without them, is proportionately growing up; and again, that when their distance is diminished, as from 10 to 1, the power of attraction, now increased a hundred-fold, has been produced out of some other form of power which has been equivalently reduced. This enlarged assumption of the nature of gravity is not more metaphysical than the half assumption; and is, I believe, more philosophical, and more in accordance with all physical considerations. The half assumption is, in my view of the matter, more dogmatic and irrational than the whole,

because it leaves it to be understood, that power can be created and destroyed almost at pleasure.

When the equivalents of the various forms of force, as far as they are known, are considered, their differences appear very great; thus, a grain of water is known to have electric relations equivalent to a very powerful flash of lightning. It may, therefore, be supposed that a very large apparent amount of the force causing the phenomena of gravitation, may be the equivalent of a very small change in some unknown condition of the bodies, whose attraction is varying by change of distance. For my own part, many considerations urge my mind toward the idea of a cause of gravity, which is not resident in the particles of matter merely, but constantly in them, and all space. I have already put forth considerations regarding gravity which partake of this idea,* and it seems to have been unhesitatingly accepted by Newton.†

There is one wonderful condition of matter, perhaps its only true indication, namely *inertia*; but in relation to the ordinary definition of gravity, it only adds to the difficulty. For, if we consider two particles of matter at a certain distance apart, attracting each other under the power of gravity and free to approach, they will approach; and when at only half the distance each will have had stored up in it, because of its *inertia*, a certain amount of mechanical force. This must be due to the force exerted, and, if the conservation principle be true, must have consumed an equivalent proportion of the cause of attraction; and yet, according to the definition of gravity, the attractive force is not diminished thereby, but increased four-fold, the force growing up within itself the more rapidly, the more it is occupied in producing other force. On the other hand, if mechanical force from without be used to separate the particles to twice their distance, this force is not stored up in momentum or by inertia, but disappears; and three-fourths of the attractive force at the first distance disappears with it: How can this be?

We know not the physical condition or action from which *inertia* results, but inertia is always a pure case of the conservation of force. It has a strict relation to gravity, as appears by the proportionate amount of force which gravity can communicate to the inert body; but it appears to have the same strict relation to other forces acting at a distance, as those of magnetism or electricity, when they are so applied by the tangential balance, as to act independent of the gravitating force. It has the like strict relation to force communicated by impact, pull, or in any other way. It enables a body to take up and

* Proceedings of the Royal Institution, 1855, Vol. ii., p. 10, &c.

† "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."—See *Newton's Third Letter to Bentley*.

conserve a given amount of force until that force is transferred to other bodies, or changed into an equivalent of some other form; that is all that we perceive in it: and we cannot find a more striking instance amongst natural, or possible phenomena of the necessity of the conservation of force as a law of nature; or one more in contrast with the assumed variable condition of the gravitating force supposed to reside in the particles of matter.

Even gravity itself furnishes the strictest proof of the conservation of force in this, that its power is unchangeable for the same distance; and is, by that, in striking contrast with the variation which we assume in regard to the *cause of gravity*, to account for the *results* at different distances.

It will not be imagined for a moment that I am opposed to what may be called the *law of gravitating action*, that is, the law by which all the known effects of gravity are governed; what I am considering, is the definition of the *force of gravitation*. That the result of one exercise of a power may be inversely as the square of the distance, I believe and admit; and I know that it is so in the case of gravity, and has been verified to an extent that could hardly have been within the conception even of Newton himself when he gave utterance to the law: but that the *totality* of a force can be employed according to that law, I do not believe, either in relation to gravitation, or electricity, or magnetism, or any other supposed form of power.

I might have drawn reasons for urging a continual recollection of, and reference to, the principle of the conservation of force, from other forms of power than that of gravitation; but I think that when founded on gravitating phenomena, they appear in their greatest simplicity; and precisely for this reason, that gravitation has not yet been connected by any degree of convertibility with the other forms of force. If I refer for a few minutes to these other forms, it is only to point in their variations, to the proofs of the value of the principle laid down, the consistency of the known phenomena with it, and the suggestions of research and discovery which arise from it.* *Heat*, for instance, is a mighty form of power, and its effects have been greatly developed; therefore, assumptions regarding its nature become useful and necessary, and philosophers try to define it. The most probable assumption is, that it is a motion of the particles of matter; but a view, at one time very popular, is, that it consists of a particular fluid of heat. Whether it be viewed in one way or the other, the principle of conservation is admitted, I believe, with all its force. When transferred from one portion to another portion of like matter the full amount of heat appears. When transferred to matter of another kind an apparent excess or deficiency often results; the word "capacity" is then introduced, which, whilst it acknowledges the principle of conservation, leaves space for research. When employed in changing the state of bodies, the appearance and disappearance of the heat is

* Helmholtz, On the Conservation of Force. Taylor's Scientific Memoirs, 2nd Series, 1853, p. 114.

provided for consistently by the assumption of enlarged or diminished motion, or else space is left by the term "capacity" for the partial views which remains to be developed. When converted into mechanical force, in the steam or air-engine, and so brought into direct contact with gravity, being then easily placed in relation to it, still the conservation of force is fully respected and wonderfully sustained. The constant amount of heat developed in the whole of a voltaic current described by M. P. A. Favre,* and the present state of the knowledge of thermo-electricity, are again fine, partial, or subordinate illustrations of the principle of conservation. Even when rendered radiant, and for the time giving no trace or signs of ordinary heat action, the assumptions regarding its nature have provided for the belief in the conservation of force, by admitting, either that it throws the ether into an equivalent state, in sustaining which for the time the power is engaged; or else, that the motion of the particles of heat is employed altogether in their own transit from place to place.

It is true that heat often becomes evident or insensible in a manner unknown to us; and we have a right to ask what is happening when the heat disappears in one part, as of the thermo-voltaic current, and appears in another; or when it enlarges or changes the state of bodies; or what would happen, if the heat being presented, such such changes were purposely opposed. We have a right to ask these questions, but not to ignore or deny the conservation of force; and one of the highest uses of the principle is to suggest such inquiries. Explications of similar points are continually produced, and will be most abundant from the hands of those who, not desiring to ease their labor by forgetting the principle, are ready to admit it either tacitly, or better still, effectively, being then continually guided by it. Such philosophers believe that heat must do its equivalent of work: that if in doing work it seem to disappear, it is still producing its equivalent effect, though often in a manner partially or totally unknown; and that if it give rise to another form of force (as we imperfectly express it), that force is equivalent in power to the heat which has disappeared.

What is called *chemical attraction*, affords equally instructive and suggestive considerations in relation to the principle of the conservation of force. The indestructibility of individual matter, is one case, and a most important one, of the conservation of chemical force. A molecule has been endowed with powers which give rise in it to various qualities, and these never change, either in their nature or amount. A particle of oxygen is ever a particle of oxygen—nothing can in the least wear it. If it enters into combination and disappears as oxygen,—if it pass through a thousand combinations, animal, vegetable, mineral,—if it lie hid for a thousand years and then be evolved, it is oxygen with its first qualities, neither more nor less. It has all its original force, and only that; the amount of force which it disengaged when hiding itself, has again to be employed in a reverse direction

* Comptes Rendus, 1854, Vol. xxxix., p. 1212.

when it is set at liberty; and if, hereafter, we should decompose oxygen, and find it compounded of other particles, we should only increase the strength of the proof of the conservation of force, for we should have a right to say of these particles, long as they have been hidden, all that we could say of the oxygen itself.

Again, the body of facts included in the theory of definite proportions, witnesses to the truth of the conservation of force; and though we know little of the cause of the change of properties, of the acting and produced bodies, or how the forces of the former are hid amongst those of the latter, we do not for an instant doubt the conservation, but are moved to look for the manner in which the forces are, for the time, disposed, or if they have taken up another form of force, to search what that form may be.

Even chemical action at a distance, which is in such antithetical contrast with the ordinary exertion of chemical affinity, since it can produce effects miles away from the particles on which they depend, and which are effectual only by forces acting at insensible distances, still proves the same thing, the conservation of force. Preparations can be made for a chemical action in the simple voltaic circuit, but until the circuit be complete that action does not occur; yet in completing we can so arrange the circuit, that a distant chemical action, the perfect equivalent of the dominant chemical action, shall be produced; and this result, whilst it establishes the electro-chemical equivalent of power, establishes the principle of the conservation of force also, and at the same time suggests many collateral inquiries which have yet to be made and answered, before all that concerns the conservation in this case can be understood.

This and other instances of chemical action at a distance, carry our inquiring thoughts on from the facts to the physical mode of the exertion of force; for the qualities which seem located and fixed to certain particles of matter appear at a distance in connexion with particles altogether different. They also lead our thoughts to the *conversion* of one form of power into another; as for instance, in the *heat* which the elements of a voltaic pile may either show at the place where they act by their combustion or combination together; or in the distance, where the electric spark may be rendered manifest; or in the wire or fluids of the different parts of the circuit.

When we occupy ourselves with the dual forms of power, electricity and magnetism, we find great latitude of assumption; and necessarily so, for the powers become more and more complicated in their conditions. But still there is no apparent desire to let loose the force of the principle of conservation, even in those cases where the appearance and disappearance of force may seem most evident and striking. Electricity appears when there is consumption of no other force than that required for friction; we do not know how, but we search to know, not being willing to admit that the electric force can arise out of nothing. The two electricities are developed in equal proportions; and having appeared, we may dispose variously of the influence of one upon successive portions of the other, causing many

changes in relation, yet never able to make the sum of the force of one kind in the least degree exceed or come short of the sum of the other. In that necessity of equality, we see another direct proof of the conservation of force, in the midst of a thousand changes that require to be developed in their principles, before we can consider this part of science as even moderately known to us.

One assumption with regard to electricity is, that there is an electric fluid rendered evident by excitement in plus and minus proportions. Another assumption is, that there are two fluids of electricity, each particle of each repelling all particles like itself, and attracting all particles of the other kind always, and with a force proportionate to the inverse square of the distance, being so far analogous to the definition of gravity. This hypothesis is antagonistic to the law of the conservation of force, and open to all the objections that have been, or may be, made against the ordinary definition of gravity. Another assumption is, that each particle of the two electricities has a given amount of power, and can only attract contrary particles with the sum of that amount, acting upon each of two with only half the power it could in like circumstances exert upon one. But various as are the assumptions, the conservation of force (though wanting in the second), is, I think, intended to be included in all. I might repeat the same observations nearly in regard to magnetism,—whether it be assumed as a fluid, or two fluids or electric currents,—whether the external action be supposed to be action at a distance, or dependent on an external condition and lines of force—still all are intended to admit the conservation of power, as a principle to which the phenomena are subject.

The principles of physical knowledge are now so far developed, as to enable us not merely to define or describe the *known*, but to state reasonable expectations regarding the *unknown*; and I think the principle of the conservation of force may greatly aid experimental philosophers in that duty to science, which consists in the enunciation of problems to be solved. It will lead us, in any case where the force remaining unchanged in form is altered in direction only, to look for the new disposition of the force; as in the cases of magnetism, static electricity, and perhaps gravity, and to ascertain that as a whole it remains unchanged in amount:—or, if the original force disappear, either altogether or in part, it will lead us to look for the new condition or form of force which should result, and to develop its equivalency to the force that has disappeared. Likewise, when force is developed, it will cause us to consider the previously existing equivalent to the force so appearing; and many such cases there are in chemical action. When force disappears, as in the electric or magnetic induction after more or less discharge, or that of gravity with an increasing distance; it will suggest a research as to whether the equivalent change is one within the apparently acting bodies, or one *external* (in part) to them. It will also raise up inquiry as to the nature of the internal or external state, both before the change and after. If supposed to be external, it will suggest the necessity of a physical process, by which the power is communicated from body to body; and

in the case of external action, will lead to the inquiry whether, in any case, there can be truly action at a distance, or whether the ether, or some other medium, is not necessarily present.

We are not permitted as yet to see the nature of the source of physical power, but we are allowed to see much of the consistency existing amongst the various forms in which it is presented to us. Thus, if in static electricity, we consider an act of induction, we can perceive the consistency of all other like acts of induction with it. If we then take an electric current, and compare it with this inductive effect, we see their relation and consistency. In the same manner we have arrived at a knowledge of the consistency of magnetism with electricity, and also of chemical action and of heat with all the former; and if we see not the consistency between gravitation with any of these forms of force, I am strongly of the mind that it is because of our ignorance only. How imperfect would our idea of an electric current now be, if we were to leave out of sight its origin, its static and dynamic induction, its magnetic influence, its chemical and heating effects? or our idea of any one of these results, if we left any of the others unregarded? That there should be a power of gravitation existing by itself, *having no relation to the other natural powers, and no respect to the law of the conservation of force*, is as little likely as that there should be a principle of levity as well as of gravity. Gravity may be only the residual part of the other forces of nature, as Mossotti has tried to show; but that it should fall out from the law of all other force, and should be outside the reach either of further experiment or philosophical conclusions, is not probable. So we must strive to learn more of this outstanding power, and endeavor to avoid any definition of it which is incompatible with the principles of force generally, for all the phenomena of nature lead us to believe that the great and governing law is one. I would much rather incline to believe that bodies affecting each other by gravitation, act by lines of force of definite amount (somewhat in the manner of magnetic or electric induction, though without polarity), or by an ether pervading all parts of space, than admit that the conservation of force could be dispensed with.

It may be supposed, that one who has little or no mathematical knowledge should hardly assume a right to judge of the generality and force of a principle, such as that which forms the subject of these remarks. My apology is this, I do not perceive that a mathematical mind, simply as such, has any advantage over an equally acute mind not mathematical, in perceiving the nature and power of a natural principle of action. It cannot of itself introduce the knowledge of any new principle. Dealing with any and every amount of static electricity, the mathematical mind can, and has balanced and adjusted them with wonderful advantage, and has foretold results which the experimentalist can do no more than verify. But it could not discover dynamic electricity, nor electro-magnetism, nor magneto-electricity, or even suggest them; though when once discovered by the experimentalist, it can take them up with extreme facility. So in respect of the force of gravitation, it has calculated the results of the

power in such a wonderful manner, as to trace the known planets through their courses and perturbations, and in so doing has *discovered* a planet before unknown; but there may be results of the gravitating force of other kinds than attraction inversely as the square of the distance, of which it knows nothing, can discover nothing, and can neither assert nor deny their possibility or occurrence. Under these circumstances, a principle, which may be accepted as equally strict with mathematical knowledge, comprehensible without it, applicable by all in their philosophical logic whatever form that may take, and above all, suggestive, encouraging, and instructive to the mind of the experimentalist, should be the more earnestly employed and the more frequently resorted to when we are laboring, either to discover new regions of science, or to map out and develop those which are known into one harmonious whole; and if in such strivings, we, whilst applying the principle of conservation, see but imperfectly, still we should endeavor to see, for even an obscure and distorted vision is better than none. Let us, if we can, discover a new thing in *any shape*; the true appearance and character will be easily developed afterwards.

Some are much surprised that I should, as they think, venture to oppose the conclusions of Newton: but here there is a mistake. I do not oppose Newton on any point; it is rather those who sustain the idea of action at a distance, that contradict him. Doubtful as I ought to be of myself, I am certainly very glad to feel that my convictions are in accordance with his conclusions. At the same time, those who occupy themselves with such matters ought not to depend altogether upon authority, but should find reason within themselves, after careful thought and consideration, to use and abide by their own judgment. Newton himself, whilst referring to those who were judging his views, speaks of such as are competent to form an opinion in such matters, and makes a strong distinction between them and those who were incompetent for the case.

But after all, the principle of the conservation of force may by some be denied. Well, then, if it be unfounded even in its application to the smallest part of the science of force, the proof must be within our reach, for all physical science is so. In that case, discoveries as large or larger than any yet made, may be anticipated. I do not resist the search for them, for no one can do harm, but only good, who works with an earnest and truthful spirit in such a direction. But let us not admit the destruction or creation of force without clear and constant proof. Just as the chemist owes all the perfection of his science to his dependence on the certainty of gravitation applied by the balance, so may the physical philosopher expect to find the greater security and the utmost aid in the principle of the conservation of force. All that we have that is good and safe, as the steam-engine, the electric-telegraph, &c., witness to that principle,—it would require a perpetual motion, a fire without heat, heat without a source, action without reaction, cause without effect, or effect without a cause, to displace it from its rank as a law of nature.

TOXICOLOGY.

Some REFLECTIONS on the SEARCH for PHOSPHORUS in CASES of POISONING.

By MM. CORNET and VAN HAUW.

THE extensive use of lucifer matches has given rise to accidents of various kinds; they have even been employed criminally for poisoning. It was thus that we have lately had occasion to seek for the presence of phosphorus in the organs of a child poisoned by these lucifers. The following is a summary of the case which led to these researches.

On the 21st of March, 1856, at 6 o'clock in the afternoon, in the Commune of Ovtscamp, the girl X. gave to her child (a boy) aged six months, with the intention of killing it, several ends of lucifer matches, shortly after having given it some biscuit in milk. As soon as the phosphorus was swallowed, she gave it a little water and sugar, and quitted the house, leaving the child to persons whom she paid to take care of it.

Towards four o'clock in the morning, ten hours after having taken the matters already mentioned, the child having become very ill, vomited considerably, and, according to the depositions of the persons to whom it had been entrusted, and who were in the dark, a train of fire issued from its mouth, which according to them, was filled with fire.

These people, to assuage the sufferings of the poor little creature, gave it water, which it vomited immediately, and which it ultimately refused to take; twelve hours after, that is to say, twenty-two hours after taking the poison, the victim gave its last sigh.

The post-mortem examination, which was made with the greatest care by the medical men, showed no morbid lesion in the organs of the victim; being unable to obtain the matters which had been vomited, the physicians were forced to remove the digestive and urinary organs with their contents.

Being called upon by the tribunal of Bruges to search by chemical analysis for the existence of the poison in the organs of the victim, we had recourse to the various processes known in science, and especially to that which had been recently published by the learned German chemist, Mitscherlich (*See THE CHEMIST*, April 1857, page 404.) All the results were negative in this sense—that all of them demonstrated the absence of free phosphorus in the organs.

These negative results necessitated on our part comparative researches, in order to be able to prove that our operations had been properly conducted, and that the conclusions which we had arrived at could leave no doubt.

These operations being of a nature to elucidate the important question of the search for phosphorus in cases of poisoning, we shall explain them in detail. They enabled us to establish, on one hand, the incontestable superiority of Mitscherlich's process over all the

other processes previously employed for detecting traces of free phosphorus in organic matters; and on the other hand, they placed us in a situation to remove a doubt raised by M. Nickles as to the value of Mitscherlich's process applied to cases in which phosphorus is mixed with fatty bodies.

First Experiment.

On the 15th of April, at 3 o'clock in the afternoon, four ends of lucifers were given to a rabbit after having given to it at intervals bread steeped in milk (these matches were taken from the same box as those administered to the child). Until nearly 7 o'clock, the animal appeared well and ate bread; it passed urine twice. Next day, at 5 o'clock in the morning, it appeared to be very ill, was no longer inclined to eat, and remained crouching in a corner. At 6 o'clock, it died, lying on the left side, without having vomited.

The post-mortem examination, which was made twelve hours after, proved as follows:—the stomach was found full. This organ, having been removed, was emptied and placed on a plate; examined with the greatest care, it showed no trace of inflammation. The matter which it contained was thick and brownish, and the food which the animal had taken was recognised in it; it gave out no odor of phosphorus, and produced no light in the dark, even when moved about with a spatula.

Eight grammes of this matter, analysed by Mitscherlich's process, gave phosphorescent light in the tube during several minutes.

The stomach and the fecal matters removed from the intestines, tested separately by this method, did not present any trace of this metalloid.

Second Experiment.

Two days after, at 9 o'clock in the morning, we gave to a sharp dog, six months old, six ends of lucifer matches, in a piece of bread and about a pint of bread and milk.

This poisoning presented the following symptoms:—

At 10 o'clock, the dog, lying on the left side, began to suffer; some milk was given to it.

11 o'clock, respiration quicker; difficulty in getting on and off the stool; it still drank milk.

11.30, same state of depression and suffering; better at intervals.

12 o'clock, it does not know where to place its head; sometimes, it raises it in the air, sometimes it presses it between its paws; and remains lying on the right side.

12.30, the suffering increases, the respiration is more and more impeded; it is made to swallow a little milk; the pains are relieved for a short time only to re-appear and again increase; it remains stretched on the ground, rises, and then lies on its left side; it constantly makes efforts to swallow its saliva, and its throat seems to burn; it stretches out its paws, is very restless, rises again and lies on its right side.

1.30, it passed urine copiously; its state was so much worse that it began to groan and howl.

2 o'clock, after having taken some milk, it appeared better.

3 o'clock, it got up, its state was so much improved that in the evening it appeared re-established, and next day it was as well as if nothing had happened; the dog has since passed liquid excrements, and has also made water twice.

The excrements and the urines by the process under consideration gave a negative result.

Third Experiment.

Two days after, also at 9 in the morning, twenty-six ends of matches were given to the same dog, in bread and milk; the following are the symptoms observed:—

At 10 o'clock it presented the same state as the first time; some milk was given to it.

At 11 o'clock, it vomited; the vomited matters were collected, and gave out the odor of phosphorus; the vomitings were repeated several times, and became more and more glaring. In the last vomiting, we observed several ends of lucifers; the animal refused all drink, rose occasionally, remained motionless for a few minutes, and lay on the right side.

12 o'clock, considerable improvement; this continued, and the animal had recovered by the afternoon. During this time it had passed urine twice, although it had not voided any excrement.

Sixteen grammes of vomited matter were analysed, and showed phosphorescent lights, for nearly a quarter of an hour.

The remainder of the vomited matter was placed in an open flask to be preserved in the laboratory.

Fourth Experiment.

A paste composed of starch, fat, and sugar, with which was mixed one-hundredth part of phosphorus, was put into a small flask, and submitted to Mitscherlich's method of analysis. At the boiling point, luminous white vapors begun to appear, and succeeded in rising in the tube and presenting small lights; then they descended into the receiving flask; the latter having lasted a long time, we discontinued the operation.

After cooling, the refrigerator contained a clear liquid, the product of the distillation, at the bottom of which was found a deposit resembling very fine sand, and which was ascertained to be very finely divided phosphorus.

Fifth Experiment.

Being desirous of ascertaining whether free phosphorus could be found after some time in the vomited matters left in contact with the air, we analysed at the end of the month of January, 1857, the vomited matters mentioned above; they had become thick and horny.

One half was mixed with distilled water acidulated with sulphuric acid, and was introduced into a small flask, according to Mitscherlich's process. After boiling for several minutes, phosphorescent lights were manifested in the tube, as in the foregoing experiments.

From the whole of these facts, it results :—

1st. That in all medico-legal researches relative to cases of poisoning by phosphorus, preference must be given to Mitscherlich's process, which always leads to the discovery of the poison, even when it is intimately mixed with fatty bodies (first experiment).

2nd. That the search for the poison must be made with the matters contained in the stomach, and better still with those vomited.

3rd. That when once phosphorus is changed into either phosphoric or hypophosphoric acid, &c., its detection becomes very difficult, and that consequently, the jurist cannot in this case pronounce himself in a formal manner.

4th. That it is also possible to detect free phosphorus in the vomited matters (result of the third, fourth, and fifth experiments) even when these matters have been left for nearly a year in an open flask.

Annales de la Société Medico-Chirurgicale de Bruges.

CASE of COLICA PICTONUM produced by the WHITE LEAD TREATMENT of a SEVERE SCALD.

By G. A. KUNKLER, M.D., of Madison, Ia.

THE following case may not prove without interest, in showing how readily the toxic effects of lead may manifest themselves in certain persons, after its external application to a denuded surface.

Kate B., an Irish servant girl, scalded her arm by a kettle of boiling water falling upon it during the latter part of last December. The injury extended from the elbow over the whole forearm and hand, and was followed by extensive vesication. After the contents of the vesicles were evacuated, by puncturing them with a needle, the common white paint, of the consistency of cream, was applied freely with a camel's hair brush, the parts covered with cotton, and a roller lightly applied over the whole. The accident occurred early in the morning, and on the following day, the dressings having become dry were renewed. On the evening of the third day, I saw the patient again, and then found her laboring under unmistakable symptoms of *colica saturnina*, such as acute abdominal pain, retraction of the umbilicus, constipation, and *slight discoloration of the gums*. The burn was doing well; under the use of opium, purgatives, &c., she was rapidly relieved. The use of the paint was discontinued, and the linseed oil and lime water mixture substituted, from the use of which the burn got well. During the past five years, I have used the white paint in a number of cases of burns and scalds of every imaginable description, without ever noticing any injurious result; my attention having been first